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AN ANNOUNCEMENT

It is with deep regret that announcement is made of the retirement of Professor Titchener from the editorship of THE AMERICAN JOURNAL OF PSYCHOLOGY, effective with the last issue.

Psychologists universally will acknowledge that under his direction the JOURNAL has reached its highest usefulness and distinction, and that it has occupied a place among the very best existing scientific periodicals.

The JOURNAL will be continued by the persons whose names appear below, who will try to maintain it at the high standard set by Professor Titchener. Communications and manuscripts may be sent to any one of the four editors.

MARGARET FLOY WASHBURN
KARL M. DALLENBACH
MADISON BENTLEY
EDWIN G. BORING

MECHANISM, MEANING AND TELEOLOGY IN BEHAVIOR

By ARTHUR ERNEST DAVIES, Colorado College

In a former article,[1] I pointed out that behaviorism is historically connected with the frank acceptance of a biological point of view in the study of human and animal life. The statement of Watson that behaviorism is the only consistent functionalism, is an inadequate recognition of the truth of this view. It is inadequate because the functional psychologist never overcame the dualism of mind and matter that is an inheritance from the Cartesian philosophy of the 17th century, and he only succeeded in masking the subjectivism that is inseparable from this point of view by falling back upon a theory of parallelism of body and mind, or upon the more speculative double-aspect theory. As a result, functionalism merely asserted that the idea of development may be applied separately to the body and the mind. It then became the business of the psychologist, (1) to show that in the latter, as in the former, there were functions of increasing complexity, (2) to describe these, and (3) to point out their inter-relations. Moreover, the distinction between body and mind committed the functionalist, when dealing with the mind, to the method of introspection, and, when dealing with the body, to the method of objective observation. This reliance upon introspection by the functionalist should have warned Watson of the error in his statement of the historical relations of behaviorism. It was suggested, in the former article, that the connection of behaviorism with genetic psychology was much more intimate and exact. In support of this position, it was pointed out that genetic psychology had two independent starting-points and lines of development: the one, definitely biological, in the study of animal behavior, and giving rise to what is known as comparative psychology; the other, biological in its methods and aims, in the study of the mind of the child and of the so-called 'primitive' races. In both these movements, illustrated in part by the work of Jennings, Thorndike, and Mark Baldwin, introspection, as commonly understood, was thrown aside; and instead of asking what takes place within the organisms in question, they sought to ascertain what selected organisms actually do in definitely controlled conditions. The first task of genetic psychology, therefore, was the description of behavior, and behaviorism might with a good deal more accuracy be said to be the systematic application of the principles and methods of genetic psychology.

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[1] The Influence of Biology on the Development of Modern Psychology in America, *Psych. Rev.*, 30, 1923, 164-175.

Now within these more general relations, it is to be expected that the particular methods of approach to behaviorism should exhibit among the members of this school variations of numerous kinds. In the case of Watson, for example, three somewhat distinct motives may be made out. There is, (1) a decided reaction against the traditional psychology which shows itself in his rejection of introspection, and his repudiation of anything in the nature of 'philosophy' in dealing with the problems of animal and human life. Sometimes in reading Watson one is led to wonder if he has escaped the danger that lies in all reaction whatever and has, as the Germans say, "thrown out the baby with the bath water." This would, of course, be the case if, as many who are not at all unfriendly to behaviorism are saying, his rejection of mind or consciousness is for the psychologist mistaken. The question that is raised by this remark is not whether, as the founder of a new science, he is within his rights in delimiting its enquiries in any way he saw fit—no one, I think, would dispute this point; the question rather is whether the rejection of mind or consciousness in the sense of a spiritual principle or substance necessitates its repudiation in any sense whatever. The criticism might be made that he has failed to do in a systematic way what, for example, Locke felt it incumbent upon him to do in regard to innate ideas, namely, subject the notion of mind or consciousness to a critical examination in order to ascertain if there is any sense in which the notion can be intelligently used.[2] It is true that Watson has shown in a

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[2] I may remark here that I agree entirely with Woodworth that the behaviorists' attitude toward consciousness is quite unsatisfactory, and is likely to remain so as long as it is not subjected to a rigid and careful critique. I find an illustration of this unsatisfactory state of affairs in Weiss' misunderstanding of a remark of mine in the summary of the article previously referred to. What I said was: "I do not see that behaviorism, so long as it remains on scientific grounds, can be criticized for starting with the reality and observability of behavior; but if and when it discusses the mental order, either as something that does or does not exist, it is open to the criticism of raising a question which, because the answer to it must ultimately rest upon an appeal to consciousness, undermines its own position." The point was, of course, that behaviorism has the choice of two courses. First, it may confine its studies to behavior in the sense of neuromuscular reactions, and then the question of consciousness is not raised. But if, in the second place, it speaks of consciousness at all, it must do so in some traditional sense, or in some sense that it is to be expected it is able to define. Now whichever of the latter alternatives it chooses, the possibility of mutual understanding, I insisted, rested upon experience of the kind of thing that the word connotes.

I may add that because he did not see that I was leaving the question of the meaning of my consciousness open, Weiss says, "my statement is number of his writings, first, that the notion of mind or consciousness is a shifting and unreliable one; and secondly, that it is possible to deal systematically with some of the problems usually connected with this notion without having recourse to introspection. But this hardly meets the case. So long as he remains in the paradoxical position, first, of denying all reality to mind; and secondly, of saying that if it exists it has no interest for the psychologist, there will be reasonable grounds for supposing that his reaction against traditional opinions has exercised an extreme influence upon the way behaviorism has developed in his hands.

(2) The second source of Watson's behaviorism is his acceptance of the methods and aims of biological science. There are many who are not Watsonian behaviorists who are in agreement with him at this point. His book on *Behaviorism* illustrates well enough the positive motives to assimilate psychology without remainder to biology. The necessity of this attempt, in the extreme form in which it appears in Watson, seems to many to lie not so much in the warnings of history, in the lack of development of the science, say, from Aristotle to Wundt, but in Watson's own reaction to that history, in his rejection of the traditional subject-matter of the science.

(3) The third motive is illustrated in his *Psychology*. In this book, behaviorism ceases to be, or to be primarily, what it was in the other, a body of doctrine, and functions definitely at a "standpoint." It is, of course, possible to make too much of this distinction, but the difference in content and emphasis of the two books are the outcome, for Watson himself, of the different motives that prompted their writing. I should say that the spirit that determined the work of genetic psychology is more in evidence in the latter book, and the value of it lies in showing that the objective methods of the biological sciences may be expected in competent hands to be serviceable in the study of the problems of life and mind.

The positive influences that define Weiss' relations to behaviorism are in the main of a different sort. Except in the general recognition of the biological character of behaviorism,

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meaningless to those who deny the existence of consciousness *in any traditional sense in which the word is used,*" (Italics mine.), and he says this because he *assumes* that I was using the word consciousness as "*that something which* specifically may be characterized as redness, etc." It is interesting to note that in this paragraph in which Weiss undertakes to state his attitude toward consciousness, he arrives at the conclusion that there is no such thing, that is to say, he accepts the first of the two main alternatives in the quotation from my article, but he does so on the ground that one of the traditional definitions is not acceptable, without inquiring if any other definition is possible. That is to say, he ignores the second alternative, and, therefore, has carried us no further than we were at the beginning.

there is little in Weiss' exposition that is in either the spirit or the method of Watson. One way of stating the reason for this would be to say that Weiss is not as competent a biologist as Watson. It would, however, be truer and more to the point to say that he has brought to the service of behaviorism a special training in mathematics and the mathematical sciences. From this point of view, it was almost inevitable that he should say, as he does, that, however far we may be at the present time from a satisfactory or complete understanding of behavior, the psychologist must regard it, as the scientists regard all other phenomena, as a form of motion. The laws of behavior, when they come to be written will be specially complex formulae of the type that, in more simple forms, we have become acquainted with in mathematical physics. This view, as we shall see, imports into behaviorism a definitely mechanistic conception and raises many questions that, at present, we cannot answer. I think, for example, that it is because he has taken this position that Weiss is brought into agreement with Watson in the attitude he takes toward mind or consciousness. In assimilating psychology to physics he is led to the denial of consciousness as inevitably as is Watson when, from more complex motives, he assimilates psychology without remainder to biology. I shall return to this point later. A second distinguishing characteristic of Weiss' behaviorism is his admission of the usefulness of philosophy in defining and elucidating the problems of this type of psychology. In his earlier relations to behaviorism, it was philosophy in the form of epistemology that was chiefly emphasised. That this was so was not surprising, for he was merely following an historic tradition that stretches back to the physicists of the 16th. and 17th. centuries. Questions about the nature and conditions of human knowledge, its certainty and validity, seem not to be remote when specifically physical problems are under consideration; and the more theoretical physics becomes the more the need is felt for a statement of the grounds on which its conclusions rest. This ground, of course, can be nothing else than human knowledge itself. In the latest systematic statement of his views, however, Weiss leaves his former love and boldly embraces metaphysics, on the ground that it is only by so doing that scientific development can be secured for behaviorism.[3] This seems to me unfortunate, first, because it leaves us in doubt as to whether he has receded from his former position; and secondly, because it introduces into his behaviorism a major premise, physical monism, that appears unduly to influence the points of view and particular doctrines that he is led to adopt. This is the least satisfactory feature of his two articles, and I have not been able to rid myself of the impression that certain questions would have received a more empirical and less theoretical treatment if he had avoided the delusive promise that, no less for the behaviorist than for the rest of us, is inseparable from the premature introduction of theories of reality into the work-shop of the scientists.

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[3] Behaviorism and Behavior, *Psych. Rev.*, 31, 1924, 35.

If now we consider the course we have taken, it will be evident that students of behavior are confronted, now from one point of view and now from another, with three special problems. In the first place, behavior, from the standpoint of the mathematical sciences, is determined, and it calls for a mechanistic explanation. In the second place, from the standpoint of biology, it possesses survival-value, and thus raises the question of meaning. In the third place, from the philosophical and especially epistemological point of view, behavior, if it is creative or has meaning, can hardly avoid being thought of as having some relation to an end.

I propose to say something on every one of these points, but before I do so I shall state my own attitude toward the movement or series of movements that have crystallized under the name behavior. I do this in order to dispose, at the start, of the possibility of anyone thinking that I am prompted by any other motive in what I may say than the desire to look at the facts that traditionally belong to psychology in an unprejudiced way, and to find a formulation that summarises the facts themselves. It has seemed to me a pity that criticism is so often left to those who belong to another party or group, and that it consequently assumes the form of a polemic. Immanent criticism, a criticism of particular positions and the positions of particular writers, from the standpoint of the antecedents and larger relations of the subject, might lead us nearer to the removal of contradictions, to a recognition of internal inconsistencies, if any, of principle, and to a systematic development of doctrine. Behaviorism owes this to itself and to those to whom they would recommend whatever it is that goes, or is to go, by the name behaviorism. Unless this is done, there is danger that, as in the similar case of pragmatism, some one will write an article on the 'thirteen kinds of behaviorism.' This would be unfortunate if it led to an indefinite postponement of the recognition of what is common amid all the variety of doctrine that is unavoidable in the working out by individuals of the fundamentals of this branch of psychology.

I hasten, therefore, to say that I am not opposed to the concept of behavior as applied to the subject-matter of psychology. Indeed, before Watson challenged current conceptions I had already, in 1901, formulated a definition of psychology that was definitely behavioristic in statement and intention. At the time, I thought of writing this out for wider circulation, but, from the point of view I then held, it seemed to me such an obvious thing to say as to be lacking in novelty. Psychology was defined, in the notes referred to, as "the systematic study of the adjustments of psycho-physiological organisms to their environment," and the effort was made to give a definite content to each of the major terms of the definition. I do not think, of course, that this definition would be accepted by all groups of behaviorists at the present time, but twenty years ago there was better hope of this, and even now all would probably agree that the conception is definitely behavioristic. As I have not seen any reason to depart from the notion then formulated, it may be taken as a statement of my relations to behaviorism, and of my present attitude toward its various current developments.

I pass now to the topics proposed for discussion in this paper. I shall consider (1) behavior and mechanism; (2) behavior and meaning; (3) behavior and teleology. A question not far away is, "Is there any way to reconcile mechanism and teleology on behavioristic principles?" An affirmative answer would have to be given if it could be shown that behavior displays both determined and undetermined characteristics, and if certain questions involved in the notion of cause could be satisfactorily disposed of. I postpone this question because it seems to me important enough to receive separate consideration.

I. Mechanism in Behavior

I wish to state my attitude toward an assumption that seems to affect the particular form and emphasis of a good deal of contemporary behavioristic discussion. In order to introduce the particular occasion that gives rise to the assumption in question, it is necessary to divide 'psychologists' roughly into three groups: (a) those who regard "consciousness, as such" as the legitimate subject-matter of their science; (b) those who define the materials of the science in terms of behavior, meaning by that term the activities, neuro-physiological or chemico-physical, that constitute the adjustments of organisms to their environments; (c) those who combine the points of view of (a) and (b) in some sort of workable, or unworkable, relation. There are, that is to say, the right and left wings, and the middle-of-the-roaders. I have included the third group not merely for the sake of completing a very tentative classification, but in order to say that I shall not refer to them again in this paper, because most, if not at all, who belong here seem not to have realized the very fundamental nature of the questions that have been raised by behaviorism; and because I think these questions can be discussed with any degree of clearness only when they are approached from the standpoint of the more radical contrast presented by the positions of the first two groups. When we consider this contrast it seems to be taken for granted by both sides that the main issue is definitely whether mechanism has any place in, or is a characteristic of the facts considered by psychology. On the one side, mind or consciousness is repudiated because it has not lent itself to mechanical formulation; on the other, behavior is looked upon with suspicion because it is, in principle, capable of reduction to nothing but mechanism. Because of these negative views, it has come to be assumed by both parties that the case for consciousness is bound up with an indeterministic position of such an extreme character that all order, regularity, or law is, in the nature of the case, excluded. Thus, in order to oppose this indeterminism the behaviorist is led to over-emphasise mechanism, and in order to oppose mechanism, the mind psychologist is led to over-emphasise indeterminism. All this seems to me unfortunate in the extreme, and one of the consequences of it, among the behaviorists themselves, is Weiss' refusal to admit to the world of existence,—and that means, as I take it, the world of reality,—anything that falls outside what he calls the "movement continuum." One would have thought that questions of reality were sometime in their discussion questions of fact, and it is the question of fact that I find assumed in a good deal of behavioristic discussion. Whether 'consciousness' could or could not sustain a rigid examination as to its factual existence is not now being considered; what I am saying is that the importance and even necessity of making such an examination has been obscured by reason of the position that behaviorists and others have taken with respect to questions that have no necessary relation to this one. Mechanism, I have pointed out, is one of these, the "movement continuum" is another.

I am quite prepared to be told that in taking this position I am entirely mistaken. For is it not true that a great deal of effort and ingenuity have been expended by the behaviorist in showing that consciousness in the form of images or what not is merely a set of non-conventionalized motor patterns that may differ from individual to individul depending upon the particular social and cultural conditions under which the individuals in question have grown up? But this is beside the point. For the question I am raising does not refer to the kind of explanation behaviorism has to offer of images and so forth, but to the terminology in which such experiences are most accurately described. What a good many persons are interested to know is if behaviorists have the kind of experiences that most of us speak of as images, ideas, etc., for unless they have there is lacking a common ground upon which discussion can proceed. Behaviorism seems to have balked at this point because it has unwarrantably supposed that in order to speak of consciousness at all one has to mean a "*something which* specifically may be characterized as redness, sweetness, affection, awareness, envy, etc."[4] That is to say, as respects its conception of consciousness, behaviorism is held in thrall by the 17th. century Cartesianism, and seems quite unable to suppose that any other meaning of the word is possible.

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[4] An attempt to determine consciousness in more objective fashion is made by W. S. Hunter in his article on "The Problem of Consciousness," *Psych. Rev.*, 31, 1924, 1-31. With respect to the question of comparing what is in my mind or consciousness with what is in another person's, we shall, of course, be told by the behaviorist that this is wholly a matter of the "speech reaction." I experience three difficulties in accepting this explanation. The first is that it is not an answer to the question raised. What, for example, we ordinarily mean by an image is the apprehension of a material object when that object is outside the range of our perceptual, that is, receptor-effector system. Does the behaviorist have to be in physical contact with the object in order to describe and react to it? Or can he describe and react to it in its physical absence? If he can, how is this possible unless he apprehends it in some non-physical fashion? The second difficulty is that the speech reaction is not a primary, but a secondary or acquired relation to things. Nothing that we know certainly warrants the view that below thought our relations to objects, material and personal, are conditioned by developed language. Thought is so conditioned, but it has also a phylogenetic relation to percepts and images. The domain in which the two latter function does have a decided and controlling effect on behavior, on the ways in which the neuro-muscular system adjusts itself grossly to its environment. If, for example, the primitive man has an hallucination, say, of a ghost, he does not make a speech describing what he sees, he runs away as fast as he can and shuts himself up in his kraal. Afterwards, when he has recovered from his fright, he speaks about it, and words begetting other words amplify, develop, and obscure the original occurrence. Do the behaviorists propose to substitute in this region of man's life mythology for science through the unregulated use of the speech reaction? A third difficulty appears when we ask what, for a mechanistic science, speech is. The answer has to be, a certain number of air waves that have an individual configuration. Is not this a survival, under a disguised and more scientific form, of Democritus' 'eidola' that floated off from one individual to another making inter-individual cooperation possible? I may add that there seems to me a good deal that might be said for Woodworth's contention that the speech reaction of the behaviorists is really nothing but the old-fashioned introspection in disguised form. This however would take me too far afield.

It would, I think, induce to clearness if we dropped the word "consciousness" altogether and spoke of the mental order in somewhat the same way as, referring to facts of a particular kind, we now speak of the physical order of events. It would then be possible in an empirical way to collect specific instances under each category, to describe their characteristics, and to ask ourselves whether, and if so to what extent, the mental order is or is not capable of the same kind of explanation as the physical. The differences between the mind psychologist and the behaviorist would then refer to the latter consideration, assuming, of course, that we were able to come to a common agreement as to what the characteristics are that justified us in classifying an event as mental or physical. Such a procedure however, reasonable as it would seem, is precluded by the behaviorists when they accept dogmatically a philosophical theory about the world, physical monism, for example, which prejudices from the start the attitude they are led to take toward facts that belong to the mental order. If this method is justified on the part of behaviorism, I do not see why, accepting, for example, a theory of spiritual monism, the mind psychologist can be criticised for saying that the "movement continuum," the whole physical order of events, is as epiphenomenal as the behaviorist now contends the mental order of events is. My opinion is that we shall get nowhere in either case, and that it would be better to adopt a strictly empirical attitude in our study of the problems of life and mind.

Accepted as a controlling point of view for the study of behavior, the mechanistic position compels us to classify the questions involved in the investigation of the so-called secondary qualities as fictitious. On this basis, there are no secondary qualities, and, therefore, no genuinely scientific questions that can be formulated in regard to them. Yet, let it be said, a very considerable portion of the time and energy of a good many of the leading minds in this country and in England, during the last twenty years, have been given to the discussion of just such questions. In fact, the modern form of the realistic controversy may be said to center upon the status of the secondary qualities. Quite apart from the answer that has been given, by different groups of realists, to this question,—from the contention that secondary qualities are physical, mental, or neutral entities,—it is obvious that the evidence by which either or all of these positions is supported is ignored by the behaviorist when he places his doctrine on some such general argument as this: 'Everything that is real is physical; secondary qualities, if they cannot be ignored, are real; therefore, secondary qualities are physical.' It is nothing to the point to say that this conclusion agrees with the position of one of the realistic schools, for, first, the evidence of the other schools, so far as it is evidence, must be taken as throwing doubt upon the validity of the conclusion in question; and, secondly, it is not at all likely that the new realists would agree that there is any justification for identifying, as the behaviorists seem to do, the conditioned (the secondary qualities) with the condition (the neuro-muscular organism), that is its invariable antecedent.

It may be pointed out that the attempt to resolve, by the magic of an accepted philosophical theory, what exists into its series of antecedent conditions is to repeat, in a new connection, the mistake of historical empiricism, and to dissolve the whole substantial world itself, the "movement continuum" to wit, into the accidental configuration that is known as the individual. This is the necessary logic of the position of Weiss,—and he states it in so many words. He writes: "When the behaviorist begins to formulate the facts which he is to include under the term behavior on the assumption that the universe is the totality of the electron-proton aggregates and the changes that occur in their spatial relationships, he may begin his analysis in the following concrete manner: Observing an infant which he must regard as an electron-proton aggregate, he finds that the child's visual and auditory sense organs are being exposed to practically the same electron-proton conditions as his own and the adults around it. Yet its responses are entirely different. If for instance the child's nurse responds to the position of the hands of the clock by preparing the infant's bath, the infant if it responds to the clock at all will do so by cooing, crying, listening posture, etc. Unless the infant responds to the clock in some specific manner, there is no way for the behaviorist to determine whether that electron-proton aggregate known (to the behaviorist) as a clock, is modifying that aggregate known as the infant. Further, *both clock and infant are only specific movement effects produced in that aggregate known as the behaviorist.*"[5] This is as clear a statement of solipsism as one could wish; and, I submit, there is no real choice between spiritualistic and materialistic solipsism. We have reached a position where logic has gone bankrupt, and it is only a matter of taste with which variety of the doctrine, if any, one prefers to be identified.

The advantage of approaching the problems of behavior from the standpoint of biology, rather than from the standpoint of physics, is that we are not thereby committed inevitably to the theory of mechanism. It is true that one of the aims of science is prediction and that prediction implies uniformities of some sort, but this may be granted without begging the question as to the kind of uniformities that are involved in human and animal behavior. Watson has given a general statement of what psychology would be if and when the behavioristic programme is carried out. He says: "A psychology of interest to all scientific men would take as its starting point, first, the observable fact that organisms, human and animal alike, do adjust themselves to their environment by means of hereditary and habit equipment. These adjustments may be very adequate or they may be so inadequate that the organism barely maintains its existence; secondly, certain stimuli lead the organism to make responses. In a systematic psychology completely worked out, given the responses the stimuli can be predicted; given the stimuli the responses can be predicted."[6] Such a statement, Watson admits, is "crass and raw in the extreme," but I think it must be admitted that in so far as it emphasises "the necessity for maintaining uniformity in experimental procedure and in the method of stating results in both human and animal work," it must be accepted as fairly expressing the scientific aim in this field of inquiry. The statement also has the advantage of leaving open the question of the specific relation of stimulus to to response, and reserves for separate consideration the nature of the mediating links between the two. In other words, Watson is here insisting that scientific psychology is primarily concerned with experimentally determining a set of correlations between stimulus and response, and that when this has been done we have all the ground that science can provide for inferring the response when the stimulus is given, and the stimulus when the response is given. It is true that Watson believes that all behavior can ultimately be explained in terms of a physico-chemical interpretation of the connection between stimulus and response, but one would have to be very forgetful of the present-day situation of biology to say that the acceptance of a biological point of view commits the psychologist to the position of materialistic determinism.

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[5] *Op. cit.*, 40. Italics mine.

Finally, it is of importance, from the biological point of view, to indicate a certain feature of behavior that is likely to be overlooked when problems of explanation are under consideration. Explicit behavior, those forms of adjustment by which an organism adapts itself in overt action to changes in its environment, has a significance beyond the spatial and temporal relations it happens to sustain to the physico-chemical alterations that occur within the organism in question. We may, for example, be able at some future date to write a series of physico-chemical formulae that express the changes that take place within the organism when certain types of adjustment to the environment occur; but from such a series of equations we shall never be able to infer the value of the adjustment in question for the species to which the individual belongs. This reference of the activities of an individual to the continuity of the species is what the biologist means by survival-value, and it would seem evident that a complete account of behavior requires us to interpret it not only in terms of the mechanisms of reacting organisms, but also in terms of its mediate values. But if we say this, we have already passed beyond the mechanistic point of view, and are confronted with the second topic of discussion, namely, behavior and meaning.

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[6] *Psychology from the Standpoint of a Behaviorist,* 1919, 10.

II. MEANING IN BEHAVIOR

The problem of meaning is a difficult one. It need not, however, be made more difficult than it is by failure, on the part of those who discuss it, to distinguish between the statement of the problem and the proposed solution of the problem. In the light of this distinction, no one, I presume, would claim that we have reached a satisfactory understanding of the mechanism of meaning, and he would be a very courageous or ignorant man who would claim that there is agreement on all sides as to what the problem is. If we contrast, for example, the "escort of images and ideas" of the older psychologists with, say, Stout's "conative continuity," we catch a glimpse of the differences that have emerged among the mind psychologists about the problem in question; and if we contrast with either or both of these Weiss' "biophysical and biosocial equivalence," we have a fairly good measure of the distance we are, at the present time, from agreement on a fundamental and important question in psychology. Now these differences in doctrine, it would be absurd to deny, do go back to differences in methods and points of view of those who expound them; but a more minute comparison of the doctrines would, I think, reveal the fact that they are also in part dependent upon a difference in the way the common problem has been conceived by the several writers. That is to say, the problem of meaning has a larger or narrower denotation at the discretion or convenience of the person who discusses it; it is a variable category that narrows itself to the problem of how we perceive things, or that enlarges itself to include all the problems that are embraced in the fact of our most advanced culture,—the social, economic, political, aesthetic, moral, religious behavior and experience of the race. I am no wiser than my peers, and, therefore, do not propose for myself the rôle of deciding, amid this diversity of usage, where praise or blame, if either, belongs. The humbler task belongs to me to point out that there is implied a common fact, namely, the fact of complication, and the question that is raised by this fact is, what account we are able to give of it.

Complication in human experience and behavior is only another name for development and, if we so take it, it may not be difficult for all of us to agree that a primary condition of any profitable discussion is that the problem be broken up into a number of constituent questions, and that each be given a careful definition. Failure to do this adequately, I think, has led, and is likely to lead, to a good deal of unprofitable discussion; and, moreover, it has allowed to creep into current views assumptions of a quasi-philosophical sort that, if they are to be admitted at all, should be introduced by name, along with the other facts and factors of the problem, to the rest of the company. At present, they are in the position of the man "without a wedding garment," and when detected immediately become suspect. One such general assumption is that all complications are of the same general character, and that, therefore, when we have studied one we have, in principle, studied all. This, of course, may be so, but I do not know how we are to be certain of it in the absence of a classification of the particular complications we meet with in experience and behavior, and a detailed study of each. There can be no objection to this procedure on the part of the scientific behaviorist or psychologist, and if it is suggested that 'meaning' is a problem, one of a number of other problems, that arises in the effort of animal and human organisms to adjust themselves to their environment, the question, perhaps, had been cleared of a number of possible misunderstandings.

Mechanism in behavior was conceived and discussed in the last Section as a question that involved a study of the changes that take place in the receptor-effector apparatus with which the organism is endowed; it resolved itself into a study of behavior as consisting of modes of activity that were necessitated by inherited structures. The only question at issue is what the nature of the mechanisms is by which such behavior is brought about.

In this Section, in discussing 'meaning,' I shall have in mind chiefly the relation of behavior to the stimulating conditions by which it is made possible. Before taking this subject up in detail, I want to remove, as a possible source of difficulty, a whole class of facts from the limits of the discussion. It is, for example, agreed that the fundamental mechanisms, whatever their nature, by which behavior is mechanically determined have been evolved in the history of the species *pari passu* with the evolution of the environment to which these mechanisms enable the organism to adjust itself. In this way, we may imagine, a relation has been established between certain conditions in the environment and certain conditions in the organism, such that when the former act on the latter there results what is called behavior. We are here in the field of what the biologist calls heredity. I am not prepared to say what the limits of heredity in this sense are, nor am I able to say, if it has limits, what its upper range is. All that is necessary to state is (a) that there is a sufficiently close correspondence between environment, in this field, for us to approximate roughly Watson's ideal of prediction; and (b) that we reach here the closest resemblance, in the whole range of behavior, to what may be called mechanism. I have said that I want to exclude all this from the present section, although here too the question of the correspondence of behavior to its stimulating conditions is involved; but, I think, the facts themselves and the demands of scientific method require the separation of behavior in this sense from behavior that does, or seems to, involve 'meaning.' However, it is not to be presumed that behavior in this latter sense is independent of the mechanisms that are at the basis of behavior in the former sense. The only question is of their adequacy; or, to put it differently, the problem of meaning arises when the environment presents a complexity that goes beyond that which is characteristic of the situations in which the fundamental reaction-patterns have evolved. From this point of view, behavior is said to have meaning when the adjustment has some obvious and statable relation to this complexity. The study of meaning, therefore, is a study of the mechanisms by which what is new in the conditions of life is met by behavior that goes beyond the limits of heredity. The question underlying this study is whether behavior ever exhibits this characteristic. This would seem to be the case.

I shall take as my first illustration what is commonly referred to as a perceptual situation. This is made up of a number of physical objects, some of which, let us say, are confined to given spaces, and some are changing their spatial relations to other objects and to one another at varying rates. There is, in this situation, a framework of stable objects and, within this framework, an indefinite number of other objects that change from a state of rest to one of motion, and others from a state of motion to one of rest. Whatever the variable details in the two classes of objects may be, the situation has a certain configuration, and may be said to have a measurable duration. In order to make the situation thus described concrete, let us say it is fairly realised in a city street. If we introduce into this situation an individual who wishes to pass from one side of the street to the other, it is obvious that while his behavior has a determinate relation to the situation as a whole, the more prominent features of it will consist of the variations that occur in passing from one side of the street to the other, in the sudden stoppings and darting forward as he threads his way among the moving objects that constitute for him the important features of the total situation.

There is, of course, nothing new in saying that in such a situation the individual's movements are to be classified as behavior, or that the senses, so far from giving him detailed information about the nature of the objects to which the individual reacts, simply enable him to adjust himself to these objects in a self-preservative way. This doctrine of perception was formulated as far back as the 17th. century.[7] In modern terms, this is to say that behavior refers to a situation which lies outside the neuro-muscular changes to which the mechanistic behaviorist would reduce all behavior whatever, and it is contended that the neglect of this feature in his descriptions obscures for the behaviorist an important problem in his explanation of how behavior is possible. A city street we ought to remember, even if we are psychologists, is the product of the industrial revolution, and the practical situations it presents are surely not among the conditions under which our remote ancestry developed. Its psychological features are as new as its economic, and require, as urgently as the latter, the development of new methods of coping with the questions that it presents, as, for example, our "Safety First" campaigns attest. New patterns, or new combinations of patterns, are a necessity of the situation; but in any case the resulting behavior, if it survives, will show a direct relation to the conditions under which it evolved, and to this extent it will have meaning.

There is, I should say, nothing novel in this contention. For the motor patterns that figure so conspicuously in behavioristic psychology, that is to say, our inherited stock of 'neurograms,' to use a term of Morton Prince with a somewhat different meaning, are themselves the product of the influence of environment on the development of the nervous system, and carry within themselves, that is to say, in the configurated pattern of motions that is characteristic of each of them, a reference to the conditions under which they too have developed. Hence, in order that a motor pattern may become functionally active in an organism, we have to suppose that a situation exists that is essentially similar to the series of conditions under which, in the history of the species, this pattern has developed and with reference to which it has become standardized. Moreover, it is presumed that different patterns have developed under different conditions, and therefore embody and express different meanings.

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[7] Malebranche, *Recherche de la Verité,* Bk. 1, Ch. 5.

If we accept this view of the neurological factors involved in behavior, is it possible to give any account, other than the one suggested, of those explicit movements, coördinated with the motor patterns, by which the organism adjusts itself to its environment? Do not these also refer to the environment in a way that justifies us in regarding them as functions of it? Whether, therefore, our perceptual situation be an old and familiar one, or a new and strange one, an analysis would seem to show that the behaviorist has over-simplified his account of behavior, and that because he has done so he has failed to recognise that at the very heart of behavior lies, for behaviorist and psychologist alike, the problem of meaning.

I shall take another illustration in order to bring out certain ambiguities that lurk in our current use of the word environment, and that affect our description and explanation of behavior. I take the infant-nurse-clock example mentioned by Weiss.[8] Weiss states that all the individuals in the room are "exposed to practically the same electron-proton conditions." This is either a theoretically harmless assertion, or it is a practically vicious one. It is harmless if it is taken to mean that a description of the room can be given that would not be altered by the coming and going of anyone, infant or adult, who at the time happened to be in the room. It is moreover harmless if we are to understand by the "exposure" of the individuals to the room nothing more than their compresence with it. The statement would become immediately vicious if, as seems to be the case, it is supposed that whatever the individual is compresent with is the situation to which he reacts. In the case of behavior, this seems not to be the fact, and we are led to suppose it is only because the mechanical point of view obscures the problem that the facts of behavior themselves define. Supreme interest in the uniformities of behavior has led the behaviorist to overlook important variations in the behavior of different individuals under what has been described as the same conditions. In a perceptual situation, is it accurate to say that an individual's behavior is a function of the total conditions, or must we not say rather that it is a function of certain of the factors that make up the total conditions? What seems to be true is that a situation develops within the perceptual complex, and that it is to this that the behavior is an adjustment. The behavior points to or means that situation.

Now when Weiss supposes that both infant and nurse respond to the clock that is an element of the total electron-proton aggregate he calls the room, he practically admits the distinction I have pointed out, but takes no account of the problem the admission raises. Indeed, he seems to be so blind to it that he says that "the infant if it responds to the clock at all will do so by cooing, crying, listening posture, etc.," although the nurse responds by preparing the infant's bath. Now with the possible exception of "listening posture" the list of specified responses of the infant does not describe anything that could be called 'behavior' in a clock-individual situation. I am entirely in agreement with Weiss when he says: "Unless the infant responds to the clock in some specific manner, there is no way for the behaviorist (or for anyone else?) to determine whether the electron-proton aggregate known (to the behaviorist) as a clock, is modifying that aggregate known as the infant." But what I contend is that "cooing "and "crying" are not "specific" responses, and that from these it is quite impossible even to say that the clock-individual situation exists for the infant in question. Is not the contrary position based on the supposition that because the infant's senses are "exposed" to the auditory and visual stimuli of the clock its behavior is therefore determined by them? In principle, this can only mean that the individual necessarily responds *in terms of behavior* to whatever electron-proton aggregate he is at a given time exposed. This surely is not the case. What, therefore, can be inferred from the instance before us is that because the infant's responses lack the character of meaning, because they do not specifically point to or mean the clock, they do not fall under the category of behavior. It would seem then that the criterion of meaning, either explicitly or implied, is necessary if we are able to determine what does and what does not belong to the subject matter of behavioristic psychology.

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[8] *Op. cit.*, 40.

III. TELEOLOGY IN BEHAVIOR

The correlated group of receptor-neuron-effector elements which underlie the responses of physiological organisms to the physico-chemical conditions of the environment mark off, according to Weiss, a distinct class of behavior, in which the organism adjusts itself to certain effective aspects of the material world. It includes all "the sensory-cerebromotor structures and processes." These structures and processes "represent, with certain extensions, the so-called 'muscle-twitch' phase of human behavior."[9] They are called the biophysical responses.

In view of these definitions, it is perhaps safe to say that the facts that have been chiefly emphasised in the two preceding Sections are of the kind that are here called biophysical. I believe that I am not mistaken in thinking that behaviorism would regard this class of responses as pre-eminently subject throughout to a mechanical explanation, and Weiss does refer to behavior of this sort as presenting "no special difficulties for behaviorism."[10] It is, he says, all of the 'muscle-twitch' variety. Three remarks will serve to bring our discussion, and the facts upon which the discussion has proceeded, into line with these contentions. I have granted, in the first place, that facts of a mechanistic sort are involved in behavior, and that it is possible to study these without having recourse to consciousness, mind, or any other term of a non-physical denotation. How much or how little behavior is of this type is not necessary to inquire. We may take, for our present purpose, a view similar to the one that Bertrand Russell[11] develops, namely, that in the field of sensation at least there is nothing that is not an actual constituent of the world with which physics deals, and this may be delivered over bodily to be dealt with in a mechanically causal manner. But, in the second place, the matter is not so simple when we come to perception, even when this is regarded not as a form of knowledge, but as a form of behavior. I have pointed out that observation of behavior of this sort leaves us very far from certain as to what will occur in a given situation, and we are by no means in a favorable position for saying what exactly the causally effective factors are in such a situation. This statement of the case does not rest upon the limitations of present knowledge. The contention is that perception is only a simple case of what may reasonably be expected to occur over and over again in an unfinished and evolving world; that conditions will arise that are novel; and that if behavior takes place it will do so not by the repetition of what is old, but by the invention of what is new. I do not press the contention. I introduce it in order to say, thirdly, that when Weiss reduces perception to the "muscle-twitch variety" of behavior, the biophysical category is stretched to the breaking point, and that, so far from causing the behaviorist "no special difficulties," perception is likely to be as perplexing a problem to him as it has been to the mind psychologist. I have questioned if an adequate analysis of perceptual behavior has been made by behaviorism, and the conclusion seems to be warranted that an element has been omitted that, for the purpose of reference, I have called 'meaning.'

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[9] *Op. cit.*, 43, 120. [10] *Op. cit.*, 120.

In order to go beyond the point at which we have arrived, it is necessary to look at the other category in which behavior is classified. Here, it would seem, are resumed all responses that have freed themselves from the anatomo-physiological conditions that are the basis of classification for biophysical phenomena.[12] It is the field of overt behavior, and comprises only those motor effects that are conditioned by the individual's responsiveness to the social organization of which he is a member. It is a wide category and includes activities that are usually regarded as belonging to "the educational, vocational, administrative, recreational, and personal forms of behavior."[13] It may with accuracy be said that the biosocial responses are intended to include all, and all the various kinds of, behavior that constitute the special subject-matter of social psychology. Weiss skillfully avoids the question between the leading schools of social psychologists, and between them and the sociologists, as to what constitutes 'social' behavior, but he does so at the cost of giving us merely a negative statement of what he means by "biosocial equivalence." It is, he says, positively not a classification of behavior on the basis of the receptor-effector mechanisms involved. This, he had already maintained, is the characteristic of biophysical classification. In this field, different sensori-motor conditions give rise to different kinds of reaction. In contrast to this, in the biosocial field, it is the behavior of individual organisms that act as "*stimuli for other individuals,*" and, as a consequence, there is said to be "a complete change in the movement groupings" in biosocial responses.[14]

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[11] *The Analysis of Mind*, 1921, 25, 37 f.

[12] *Op. cit.*, 43.

[13] *Op. cit.*, 136, 137.

There is, Weiss contends, an impermanence to change in the sensori-cerebro-motor system that underlies biophysical responses, and this may be reasonably interpreted to mean that the neurological conditions of this type of behavior have in the course of ages become relatively fixed. The stability of the nervous mechanisms can be traced for five thousand years. It is too big a question for incidental discussion, and we need a great deal more experimental evidence on the debated question of the inheritance of acquired characteristics before any opinion, for or against Weiss' view, could be said to have any weight.[15] I mention it here to say that a sharp distinction is made between the standardised responses that rest upon such a mechanism, and those that constitute the group of biosocial responses. For instance, there are many *different* biophysical changes that lead to the *same* biosocial response, and, in view of such a fact, Weiss is precluded from finding in the former a principle for explaining the latter. "The classifications which include responses that are biosocially similar need not show any biophysical similarity," he positively asserts.[16] In taking this position, he is well aware, that he is running counter to the large number of psychologists of different schools that look for advance in neurological science to throw light upon all human behavior. He would substitute for nerve physiology the investigation of "the compound-multicellular form of electron-proton organization." When we ask, however, what the denotation of this description is, we discover that it is the behaviorist's way of saying "social." But not to press the point, one does begin to wonder, in the light of the recognized discontinuity between biophysical and biosocial behavior that is positively affirmed in Weiss' denial that these two classes of phenomena are different aspects of sensori-motor organization, what mechanical explanation of the biosocial can be given that escapes the charge of being essentially teleological.

To get this question concretely before us, let us consider the illustration of an invitation to dinner from a friend in a distant city.[17] We may begin by presuming there is involved in this, as in all true questions, an element of indeterminism. In a question-situation factors are involved that make uncertain the response, and it is the recognition of this state of affairs that determines the interrogative form the situation assumes. If this were not so, such a situation would never arise; we should never get beyond the categorical, or—which perhaps is as near the categorical as the limitations of human faculty enable us to come—the hypothetical. But, it may be pointed out, it is the existence of true alternatives (disjunction) that gives weight to the illustration, and enables Weiss to argue the independence of biosocial responses of the sensori-motor conditions that form the basis of biophysical reactions. I shall, therefore, assume that there is no objection to regarding the particular techniques of the two categories as distinct, and that they are distinct because an analysis of the behaviors that belong to each shows in the one case, and do not show in the other, an element of indeterminism.[18] What explanation does Weiss offer for this state of affairs? With a good deal that he says in this connection I am in sympathy, and I think he is right in laying emphasis upon the uniqueness of the situation in which behavior, defined so as to distinguish it from, let us say, the impact of one billiard ball on another, is the cause for responses that in its absence would not take place. Behavior is a stimulus for other individuals, and the medium in which such stimuli are effective is the social group. When, therefore, it is said that it operates as a cause, I take it that we are to understand by behavior the *characteristic* responses of biological organisms, in distinction from the changes in molecular state that act as causes in the physical world. Stated otherwise, Weiss emphasises the importance of distinguishing between the region in which the equations that express our knowledge are reversible, and that in which our equations, in the nature of the case, never can be. What other distinction can be meant if the biophysical stands in such a decided contrast to the biosocial, as he says? And upon what factor can the finger be laid, in the latter case, that is the bearer of this indeterminism? If Weiss' friend in a distant city does not know if a dinner invitation can or cannot be accepted, must there not be something in the situation of that electron-proton aggregate that Weiss calls his friend upon which the answer finally rests? And whatever this factor may be, it is excluded, on the view we are examining, from the realm of mechanical causation. The conclusions then to which we come are these: (a) that behaving organisms, as behaving, affect one another in a way that differentiates them from non-behaving things; and (b) that the principle of determinism that suffices for the explanation of the latter does not apply to the former.

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[14] *Op. cit.*, p. 43. Weiss' italics.

[15] The position taken by Weiss seems to me one of those temporary and precarious bridges that behaviorists are in the habit of building in their campaign of carrying the gospel of behaviorism, in its extreme form, to the unenlightened peoples who dwell on the other side of the Tiber. I suppose I refer to something in behaviorism that Miss Edgell in *Theories of Memory* speaks of as "camouflage."

[16] *Op. cit.*, 44. [17] *Op. cit.*, 43.

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[18] I have shown in a previous Section that I would draw the line that separates the sphere of determinism and that of indeterminism at another place, and much lower down in the phylogenetic history of the species than does Weiss. That however is an unimportant detail. What matters is the recognition of the distinction.

Now this is exactly the state of affairs we should expect to find if teleology were a characteristic of behavior. In order to keep our point of view as empirical as possible, it may be affirmed that teleology is as much a matter of fact as mechanism is. In both cases, we want to know if behavior has or has not certain specified characteristics. Ultimately, teleology rests upon an affirmative answer to the question: Does the behaving individual contribute factors to the conditions under which behavior takes place so that, if all the other factors remained the same, the behavior would, in this case, be different?

I have pointed out that this set of conditions is realised in genuine questions. It is also realised in human wishes. For their fulfillment, wishes depend in part upon the individual who entertains them, and in part upon other individuals for whom they are not wishes but, what Baldwin[19] calls, 'proposals.' What for me, therefore, is psychologically a want is for you a course of action. Something like this, in simpler or more complex form, is the fundamental structure of most, if not all, social situations. Whether I realise my wish,—whether in any social relation the wish of anyone is realised,—depends, in the final count, upon the particular contribution that is made by some individual or individuals other than the one who entertains the wish. Wishes are of varying degrees of complexity, and they may last for a few seconds or endure for days or years. They may affect the structure of the society within which they arise not at all; or they may, when realised, involve its complete reorganization. These are merely incidental variations of the class of phenomena we are considering, and we have only to mention them to see how large and important a place they play in human life. They belong to the group of biosocial facts. Interpreted in behavioristic terminology, wishes are, or are expressed in, some form of behavior—speech reaction, if you please; they act as stimuli that release, or do not release, in other individuals responses upon which the realisation of the wishes in question depends. If I am correct in supposing that we have here an example that embodies factors that are present in all or most social relations, it ought not to be difficult to agree that indeterminism is a characteristic of a very large part of human life. And further it ought not to be difficult to agree that such indeterminism as exists has its basis in the non-mechanical nature of the laws that are expressed in social behavior.

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[19] *Thought and Things*, 2, 151 f.

If I have understood his position, Weiss, I anticipate, will agree that the foregoing statements and deductions are substantially correct. In that case, I should like to point out that wishes have to be interpreted, from the behavioristic point of view, as part of the conditions under which behavior takes place. They create a situation in which it is possible for acts to occur that, apart from those conditions, would not take place. But is not this exactly what the word teleology has in the main stood for in all its long history? There are no doubt reasons why the scientist regards the term with disfavor, and his nervousness apparently is not overcome by any substitute that may be offered. This is particularly noticeable among the nominalists, with whom words function too often, in our own and other centuries, as unrecognised essences. But be that as it may, and let our terminology be what it will, there is urgent need to be called to a frank recognition of facts, and behaviorists least of all should hesitate to respond to the empirical call.

I have referred to the existence of objections to the word teleology on the part of scientists, and so far as these objections are based on the fact that particular interpretations of the term obscure our insight into the nature of the physical and organic worlds, they seem to me valid. Suspicion of the word and what the word stands for took the form among psychologists of an attempt to write a psychology "without a soul," and the same attitude is seen in the criticisms that were directed against the notion that the mind was made up of a number of separate faculties. For what, it was asked, is the 'soul' but the survival within the field of psychology of the *deus ex machina* of theology, and are the so-called 'faculties' anything more than a survival in pluralistic form of the same conception? In neither case is there a description of observed facts, and used as an explanatory principle the 'soul' or the 'faculties' tempts one to indulge poetic fancy where regard ought to be had for the rigors of scientific proof.

Notwithstanding all objections and difficulties connected with the notion, teleology has never quite died, and at the present time McDougall has become the spokesman for it as the only means by which psychology can advance to the position of an independent science. In his *Outline of Psychology*, and also in his Presidential Address before the Psychological Section of the British Association for the Advancement of Science, McDougall has entered a plea for "purposive striving" as a fundamental psychological category. If I understand him correctly, McDougall is led to this position through dissatisfaction with what he regards as the one-sideness of both the behaviorists and the introspectionists.[20] He agrees with the introspectionists in saying that facts of a mental order must be included among, if they are not the whole of, the data of psychology, but, he argues, so long as the introspectionist maintains the structural-analytic standpoint he can never achieve an explanation of the

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[20] *Op. cit.*, 1923, ch. 1.

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facts with which he deals. His disagreement with the behaviorists is of a more radical sort. For he contends that when they bring over into the field of psychology the methods of description and explanation that have been developed in the physical sciences, they not only subordinate psychology to an outside control, but render themselves incapable of observing the subject-matter that has historically belonged to psychology. The situation is no better among those who try to combine the analytical description of consciousness with the mechanical explanation of the behaviorists, for they are bound to have recourse either to psycho-physical parallelism or to epiphenomenalism, and, in either case, this results in nothing but "a lame excuse for a science." It is time, he says, that psychologists took themselves seriously as scientists, and accepted the task of developing psychology as an independent discipline. And this can be done, he contends, by adopting without reserve "the conception of purposive striving as valid, useful, nay, indispensible and therefore true."

Above I called attention to Weiss' objection to mind on the ground that it involved a definition of the type "*that something which* specifically may be characterized as redness, etc." This he argues is tautology, because there is nothing in addition to the redness, sweetness, etc., nothing, that is to say, except these definite experiences, that remains to give meaning to the word mind. The mind isn't red or sweet; red or sweet, as concrete data of experience, are what must be meant by mind, if anything is meant. Now it is interesting that McDougall adopts this type of definition of mind in his *Outlines*, and insists that the mechanistic psychologist cannot escape it. "If," he argues, "we refuse to follow this modern practice (of substituting the nervous system for mind), we must go back to, or adhere to, the older practice and still speak of the mind. For what the older psychologist meant by "the mind" was that *something* which expresses its nature, powers, and functions in two ways: (1) the modes of individual experience; (2) the modes of bodily activity, the sum of which constitutes the behavior of the individual. The mechanistic psychology says—this *something* which expresses its nature in these two ways is essentially the brain, as described to us by the mechanistic psychology. There can be no doubt that we are bound to postulate this *something,* and that, if we are not content to regard it as merely the brain mechanically conceived, we must have a name for it. . . . I do not think we can find a better word to denote this *something* than the old fashioned word 'mind.' The mind, then, of the individual organism is that which expresses itself in his experience and in his behavior."[21]

It does not fall within the scope of this paper to examine McDougall's views in detail, but no consideration of teleology would, at the present time, be justified which omitted all reference to the position he has taken on this subject. The most that I shall attempt is to state two questions that are often confused, and are really distinct, and to indicate an attitude of mind in which, it seems to me, the questions may be approached.

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[21] *Op. cit.,* 35.

The first question is this: Is "purposive striving" as a fundamental psychological category necessarily connected with a substantial conception of mind? That McDougall does take such a view of mind is evident from his statement of position I have just quoted. From one point of view, McDougall's assumption of the Cartesian position places him in no worse case than, for example, Weiss is placed in assuming physical monism. But in neither case do we squarely meet the ultimate question. For it is one thing to say that a philosophical view may be adopted for its methodological serviceableness in science, and it is another to say that the content of a science is a justification of a particular philosophical view. Now the question I have raised is more closely connected with the assumption of the latter point of view than with the former. It would clear the air if we were told whether the acceptance by the psychologist of "purposive striving" committed him to the substantive view of mind. Historically, I think, there is no unequivocal justification for this contention. But that some such connection as I have indicated is implied in McDougall's statements seems to be the general impression. I feel that this is unfortunate, because it places an unnecessary obstacle to the unprejudiced consideration of McDougall's psychology.

Let us return to the quotation above. Frankly, I may say that I am not satisfied with this way of stating the case. My dissatisfaction is based upon the feeling that it confuses, or seems to confuse, logical with psychological analysis. We may, of course, say that both experience and bodily activities *imply* a subject which acts in certain observed ways; but it is doubtful if this subject ever comes within the range of the methods by which we become acquainted with the modes of experience and bodily activities that are said to manifest its "nature, powers, and functions." This objection raises in a particular form the difficulty that was expressed historically with respect to the Cartesian *cogito ergo sum.* The *sum* it was held must be either an inference or an implication of the *cogito.* In the former case, it is not a datum of experience; in the latter, it is only an indirect object of observation. For what we directly observe are sensa,—colors, sounds, tastes; and, if the question arose as to what sensa reside in, the answer would be in material objects, not in a subject of experience. The relation of the subject to its experience, however, is not a scientific question, and we can hardly think it possible that McDougall is calling upon his colleagues to take the philosophical way to the founding of an independent psychology. We can hardly think so because this course would be open to the same kind of objection he has himself brought against the behaviorists' attempt to make psychology subordinate to physics and chemistry. If, therefore, this is not what McDougall intends, there does not seem to be any particular advantage in the way in which he states his position. It would surely be enough, to start with, to say that by "mind" he means the "modes of experience and the modes of bodily activity," and that the word "behavior" is to be understood as a synonym of "mind". This is, indeed, what the passage practically amounts to when it is shorn of its Cartesian implications; and, in this form, it would seem to offer a more favorable point of departure, and to provide a broader basis of cooperation.

If this is a satisfactory solution of the difficulty implied in the first question, the ground is cleared for a consideration of the empirical evidence he advances in support of "purposive striving." The second question, therefore, to which attention should be given is this: What account, other than a philosophical one, does McDougall give of the category of "purposive striving?" What are the empirically observable facts upon which a justification of the category is shown to rest? This is a question that cannot fail to receive considerable attention, and, since this paper was written, Weiss[22] has given it partial consideration. I have done all that is possible within the scope of my purpose in this paper to indicate how teleology may be approached from an empirical point of view, and it is perhaps some service to have indicated how McDougall's position may be brought into the focus of current discussion.

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[22] *Psych. Rev.*, 32, 171 f.

THE PSYCHOLOGY OF *GESTALT*[1]

By Harry Helson, University of Illinois

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The general reader is advised to omit the small print in the following two Sections, which deal with technical material of importance principally to the specialist. The remaining Sections show how configurational principles apply to hearing, touch, simple and complex behavior patterns, memory, learning and the higher thought processes, and comprise a brief discussion of the psychology of the abnormal and the general implications of configurationism. Since Koffka's and Köhler's books have appeared in English, I have decided that more than passing reference to them is unnecessary, and that a more detailed consideration of experimental sources (which will probably never be translated into English) will be more useful and enlightening in tracing the developments and implications of *Gestalttheorie*. The nature of the original material precludes the possibility of any such systematic presentation of topics as I gave in the instance of movement, but I have tried to draw a coherent and clear picture of the important subjects touched upon by the configurationists in the various fields of experimental psychology.

(16) *The Effect of Configurations upon After-images*

The configurational effects upon after-images have opened up some interesting problems. It might readily be granted that the configuration of a physical structure will decide what is given in perception. It might also be taken for granted that, if the objective configuration is equivocal or weak, imaginal factors may play a part in determining what is perceived. After-images seem to fall between experiences which are wholly subjective and those which are determined by stimuli; in other words, they are too nearly objective to vary directly with purely internal changes and too nearly subjective, or too far removed from stimulus, to be universally determined throughout their temporal course by stimulus.

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[1] This is the third article in a series on the psychology of the *Gestalt*, the first two of which appeared in this Journal, 36, 1925, 342-370, 494-526.

Experiments were carried on by Frank in which after-images were projected on screens on which were drawings of three-dimensional figures,[2] figures in perspective, figures which represented figure-and-ground effects, letters with contours and the like. Frank found that after-images tend to accommodate themselves to the form and size of the drawings of three-dimensional bodies, just as they do when projected on real bodies in space. An after-image thrown on a drawing of a corridor appeared small when it was localized at the near end and larger when localized at the far end of the corridor. If the after-image is thrown on a drawing whose fields can be seen as figure and ground, it will be clear (*deutlich, klar*) when it is on the figure and blurred when projected on the ground, partaking in this case of the properties of each field as it forms part of each. If the fields are reversed, so that what was figure becomes ground and conversely, the after-image which was blurred on the field as ground becomes clear on it as figure.

Fuchs and Katz[3] (the latter not a configurationist) found that, when an after-image was projected on an object so that it protruded beyond the outline of the object, the part covering the object was transparent and remained so as long as the contour of the after-image was preserved. The influence of the configuration upon the after-image was shown in a positive way by Fuchs by means of the following procedure: a stimulus was presented which was composed of differently colored parts, forming two discernible configurations, each with its own color. If one figure was seen, it had a certain dominating color throughout, but if the other figure was seen, another color predominated. A circle in the center of these figures contained a mixture of both colours. If the *O* saw one configuration, the after-image of the middle circle took on the color of the dominating configuration, no matter which configuration was preferred in perception. If both figures were perceived as a single configuration, it had a uniform color in the after-image as well as in direct perception.[4]

Fuchs, contrary to the usual theories of after-images, believes that after-images may be influenced as much by central processes as by peripheral.[5] In fact, Fuchs argues for the central origin of colors as well as for the central origin of configurations and after-images, in view of the fact that, as in the example just given, the color may vary with the after-image. The central theory of after-images receives support from certain observations made with abnormal *O*s. Pathological *O*s, suffering from hemianopsia, were able to see complete figures when part of the stimulus-configuration fell within the blind portion of the retina, and were also able to obtain complete after-images of these configurations. Often, when perception failed to reveal a complete figure, the after-image would contain more of the figure than the original. Finally, when a part-circle was seen to be incomplete, a complete circle was seen in the after-image. When the circles were composed of different colors, one half being colored in one color, one half in another color, sometimes the entire circle was seen in one color, sometimes in two. But the completion of the figures takes place only when there is sufficient stimulation of the normal portion of the retina for the formation of a configuration. Fuchs' preference for central explanations, in place of explanations by means of peripheral mechanisms, is illustrative of the general tendency of the configurationists to push explanations, whenever possible, back into the central nervous system.

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[2] H. Frank, Ueber die Beeinflussung von Nachbildern durch die Gestalteigenschaften der Projektionsfläche, *Psych. Forsch.*, 4, 1923, 33-42.

[3] W. Fuchs, Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung, *Z. f. Psych.*, 91, 1922, 201 ff. Katz, Die Erscheinungsweisen der Farben. *Z. f. Psych.*, Ergänz. Bd., 7, 1911.

[4] W. Fuchs, Experimentelle Untersuchungen über die Aenderung von Farben unter dem Einfluss von Gestalten ("Angleichungserscheinungen"), *Z. f. Psych.*, 92, 1923, 288 ff.

[5] W. Fuchs, Psychologische Analysen hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter; Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker, *Z. f. Psych.*, 83, 1920, 67-169.

Many of the phenomena noted by Fuchs were observed by Rothschild in his study of the influence of the configuration upon negative after-images.[6] Rothschild presented figures made of white woolen threads on a black background. He used simple unitary curves and line figures, and was able to reproduce them with no change in form. Little irregularities in the originals were often corrected in the after-images, which showed a tendency to become "good" configurations. But line figures which were not composed of a unitary line (*Linienverlauf*) often gave different results; the whole figure would not appear at once, parts would appear in rivalry as simple unitary figures in themselves. Thus an arrow gave first the tail end, and then the head, in alternation. That these and similar phenomena were not due to position in the visual field or to attention was proved by the fact that it was possible to perceive a single, good configuration when the parts extended far into the periphery of the field of vision. Attention to a part may (as Fuchs found) cause the configuration to disappear.

Outline figures which were incomplete were completed in the after-image. This rule does not hold, however, for surface figures where other conditioning factors are too strong to allow completion to take place. Complex figures often gave alternation of different part-figures, especially when the parts themselves formed independent figures or when the original did not possess a definite, universal structure.

(17) *The Influence of Form upon Color*

Does the configuration in which a given quality appears exercise any effect upon that quality? Or are qualities unique phenomena, independent of their position within a structure? Are qualities amenable to configurational laws? The configurationists have sought to answer these and similar questions as they applied the concept of *Gestalt* to traditional problems of color, psychophysical limens and contrast. These problems concern fundamental points of view, and in dealing with them we shall have occasion to note wherein the configurationists have adopted a new attitude toward many of the old facts.

Fuchs'[7] experiments on the influence of form on the perception of color are guided by such questions as these. Does the critical part of a configuration influence the parts around it? Must the parts be in immediate contact in order to be influenced by the rest? Do assimilative effects appear in after-image and contrast colors? The first question was answered by observing an orange E exposed through a 30° opening of a blue episcotister. The E will appear either (1) in two parts, of which one part is more blue or gray than the other while the second part is orange, or (2) as a complete letter whose color is orange while the part surrounding it is a shaded orange. The experiments showed that even though certain parts may differ from the rest in color; the greater part will influence the rest; the color of the larger part determines the color, of the whole. But the color of the larger part is itself more or less changed. If any part of the figure is attended to, it keeps its own color, it is necessary for the configuration as a whole to be perceived if it is to assume a uniform color. These facts show, says Fuchs, that a configuration is a unitary surface consisting only of a single piece.

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[6] H. Rothschild, Ueber den Einfluss der Gestalt auf das negative Nachbild ruhender visueller Figuren, *Arch. f. Ophthalmol.*, 112, 1923, 1-128.

[7] See reference in Note 4.

Even though the smaller part is different in color from the rest it may possess greater intensity or impressiveness, and it will thus compensate or overcompensate for the predominance of the color of the larger part. Under these conditions the color of the whole will be a mixture of the two or will even lie wholly in the direction of the color of the smaller part. The color of the smaller part may exert a greater influence over the whole than its size warrants, if it forms the crucial part (*Schwerpunkt*) of the configuration. If a configuration has several important points of reference, the crucial point may be shifted from one to another. Thus the letter E has at least two crucial parts, the lower horizontal line and the vertical line. Some interesting facts emerge when the E is broken up in different ways. We may summarize these facts as follows: the color of the other parts influences to some extent the color of the critical part; the color of the critical part deviates more or less by assimilation of the color of the other parts; the mutual assimilation-effects of the parts are so much the greater, the more or the better the perception of the total configuration. The limiting case would be that in which every surface should be of the same color as each of its parts. Assimilation would therefore arise as a result of configurational perception, not as the result of the assimilative tendency of neighboring retinal fields by diffusion.[8]

Under proper conditions, a configuration may arise from separate elements. If five yellow circles, separated by a small interval and forming the outline of a square with a spot in the center, are observed and if a shadow has been thrown on the middle circle, it will appear like the rest in color, sometimes as shaded and at other times not. If the first, second and fourth are perceived as a square, then the middle one (the third) appears darker than the rest. If the first and second are in a single configuration and the third is made a part of it, then even though it is a darker shade of yellow than the first two it will appear like them in color, but as opposed to the fourth and fifth the first three appear to be a grayish yellow.

Fuchs found that geometrically simple figures need not be psychologically simple. The triangle is a figure in question; although it is geometrically simple it is difficult to get configurational effects from it psychologically. Factors of simplicity have yet, Fuchs admits, to be investigated more fully. The square and the circle are geometrical figures which lend themselves to configurational effects very easily, as we found above. Some interesting effects were found in the following arrangement: nine circles were arranged to form a square, the four outer being of a blue-green color, the four inner being yellow, and the center one a mixture of the other colors. It is here possible to see simultaneously a blue-green square and a yellow cross within it. In this case the middle circle is yellow. If one sees a blue-green cross, tilted toward the left, and a yellow square, also tilted, the middle circle is blue-green. If one perceives all nine circles as a single configuration, they take on the same color, which will be either yellow or gray. The same thing holds true of the after-image; here the middle circle will take on the color of the figure to which it belongs. In this manner the configuration determines the quality of its parts.

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[8] Fuchs, *op. cit.* (Note 4), 264-265. For Exner's theory of retinal diffusion see *Pflüger's Arch.*, 73, 1898, 177 ff.

Assimilative effects within configurations were also observed. Different kinds of assimilation may take place; assimilation may be between a contrast color and the color of its surroundings. An example of the first type is found in the following simple experiment: if two circles cut out of the same gray are laid respectively on a white and a black paper and the whole is covered with a piece of gauze paper, the two circles are different in color if perceived independently; but if both are perceived in a unitary structure their difference in color vanishes, apparently because mutual assimilation has taken place between them. Wundt's explanation of this phenomenon rested on the assumption that the original contrast phenomenon is compensated by a mutual associative effect.[9] If Fuchs' explanation is correct, associational factors are not in question; "nach unseren Ergebnissen kommt aber eine Assoziation nicht in Frage, sondern: unter der Wirkung der Gesamtgestalt der aus beiden Kreuzen samt Verbindungsbalken sich ergebenden Figur tritt positive Farbänderung, d. h., Aenderung der Wahrnehmungsinhalte ein."[10]

Assimilation between the contrast colors and colors of the surroundings took place between black squares intersticed with white narrow lines. The squares and lines tend to assimilate to the surrounding colors as either one figure or another is grasped; if the white lines are fixated, so that they protrude from the rest, gray spots will appear on them due to the surrounding black squares. Color assimilation takes place best when the configurations possess a strong coherence-grade and the colors merge gradually into one another. Experiments in contrast and assimilative phenomena show that with the same stimuli it is possible to get assimilative or contrast effects by changing the perception of the configuration.

Benary, Gelb and Granit have found that contrast effects are different according as the contrasting parts belong to the figure or to the ground. It may be said that the work of these men consists in getting a closer approximation to the factors influencing the quality *within* the configuration itself.

Benary places two gray patches on a black cross in such fashion that the one is within the black field of the cross and the other forms part of its ground.[11] The latter actually is surrounded by more black than the former and should therefore, according to summative theories, appear lighter than the other by contrast. But the gray in the cross actually appears lighter. Brightness therefore depends upon whether the field to be tested forms part of the figure or ground, whether it is part of a configuration or not. But there are other factors working against the figural effect; so that the gray forming a part of the figure is not always the lighter. The configurational factors may work against one another in so many different ways that it is at present impossible to draw definite conclusions from the results of all the work so far reported.

(18) *Auditory Configurations*

Auditory phenomena group themselves very readily into temporal and non-temporal configurations. A single tone or chord, perceived almost instantly, represents a non-temporal configuration. But themes, melodies and the complex patterns of sound and harmony that we hear in a choral or symphony require time for their completion, and hence may be called temporal configurations. The presence of specific form-qualities in tonal complexes was noted by Mach, von Ehrenfels and Meinong. Mach saw in the new qualities peculiar space-forms and time-forms which were merely new sensations. Meinong ascribed the properties of a melody not compoundable from its parts to a special founding process and the transformations which such a process works upon sensory contents.[12] The configurationists, rejecting the space-forms and time-forms of Mach and the "activities" of the Graz school, assert that temporal configurations simply require a time-parameter, and the changes in time which it permits, to account for the configurational properties of the completed structure. Even though dynamic processes in time are difficult to explain, they are subject to configurational laws and satisfy the criteria of true structural events.

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[9] W. Wundt, *Physiol. Psychol.*, 2, 1902, 261 ff.

[10] Fuchs, *op. cit.*, 298.

[11] W. Benary, Beobachtungen zu einem Experiment über Helligkeitskontrast, *Psych. Forsch.*, 5, 1924, 131 ff.

The problems of sound, from the configurational point of view, become almost endlessly complicated by the multiplication of facts of "higher order." Thus the problem of the resolution of qualitative patterns, whether in a single tone or a chord, difficult as it may be, is but the first of a number of even more vexing problems concerning the nature and conditions of the more complex auditory phenomena such as fusion, combinational tones, localization, distance, monotic and diotic hearing. If we also take into consideration the phenomena described under the head of "space induction" in sound, and the relations of tones and noises, then it seems an almost hopeless task, from the point of view of any psychology, to bring all the facts under a single ordering principle. Since a systematic investigation of all these problems remains to be made, I can touch upon the bearing of configurational theories on the field of audition only in so far as experimentation has yielded definite results in the most important problems.

Beginning with the simplest auditory configurations, a single tone or clang, we may describe it as a complex unity in which any modification in intensity, pitch, volume, brightness or vocality will affect the whole. According to older assumptions, the perception of unitary clangs or chords possessing super-summative properties was purely phenomenological (*Schein*), and could be dissolved into simple components as soon as the effects of analytic attention revealed the multiple nature of the experience. But granted that it is possible to analyze partial tones and this or that attribute from a tonal complex, the configurationists insist that phenomenal structures possess unique properties paralleled in the nervous system and lost in analysis. The reality of total clang as such cannot be questioned in the light of such experimental evidence as Eberhardt offers: a partial too weak to be heard may still influence the total clang color, while every partial tone in a total clang suffers in intensity as a result of its membership in the configuration,—not because of attention, weak or uncertain "apperception," or the like, but because of configurational principles applicable to the physical and physiological structures as well as to the phenomenal. Certain discrepancies between physical and phenomenal configurations or their components still remain, but these can be removed only by a systematic variation of subjective and objective conditions.[13]

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[12] *Cf.* M. Bentley, Psychology of Mental Arrangement, this JOURNAL, 13, 1902, 268-293 for the school of the *Gestaltqualität*, and the first article by this writer, *op. cit.*, 343-344.

Passing to some of the more complex aspects of hearing, we find essentially the same arguments advanced by the configurationists against summative theories of the physiology and phenomenology of monotic and diotic hearing.[14] Unitary, diotic hearing does not consist of a summation of two monotic processes; it does not build itself up out of monotic processes or parts of them. The primary processes do not unite into secondary processes or enter into them as fundaments. The processes in each ear furnish only the necessary and sufficient conditions for the unitary structures found in diotic hearing, just as the inaudible primary tones are necessary for the perception of the beating tone. Some experimental results support this statement: von Hornbostel found that beats could be produced in each ear independently of the other, and that over and above the dichotic beats could be heard beats of a higher order. This phenomenon cannot be a result of central *Ueberlagerung* of right and left processes. Nor is there a summation of intensities; for when both tones are definitely below the threshold, double hearing does not bring them above it.

The configurational theory of hearing advanced by von Hornbostel is designed to account for the phenomenology of hearing by preserving characteristic properties of total structures. Any theory of hearing, he tells us, must proceed on the assumption that double (diotic) hearing is the original form of hearing from which monotic hearing is derived. Diotic hearing cannot be derived from monotic; localization, direction and other spatial attributes of sound are largely functions of diotic hearing. Allers and Schmiedek object to this theory on the ground that monotic hearing possesses spatial attributes *per se* which are of importance for diotic hearing, just as monocular moments are conserved in binocular vision.[15] But von Hornbostel's theory is characteristically configurational in that he would explain the facts of single hearing and discrete stimuli by the unitary central processes, going, as Wertheimer would say, *von oben nach unten*.

The auditory perception of space, whether by means of monotic or of diotic hearing, becomes tremendously complicated when we consider the rôles played by vision and kinaesthesis in sound localization. Indeed, some writers assert that audition possesses no spatial characters like direction, distance, and position, except in so far as visual and kinaesthetic images are associated with the primary auditory data. The primary difficulty with associative theories in this field, as Allers and Schmiedek point out, is fatal. How can any datum acquire spatial properties if spatiality is not somehow resident in it? On the other hand, the facts described by Klemm and embodied in the following law of spatial induction or complication challenge all theories: simultaneous, disparate stimuli (*e.g.*, vision, sound and pressure) exercise an inductive effect upon each other; this effect can be characterized as a tendency toward spatial fusion. The transition from one sense modality to another and the unitary effects arising from the fusion of sense data from different modalities are inexplicable on any atomistic-associationistic theories. Some common materials and processes must be at the basis of such phenomena. The configurationists have attempted to frame a set of hypotheses to account for the common denominator in the various sense modalities; but as yet the field is an open one, and challenges investigation and new ideas.

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[13] M. Eberhardt, Ueber die phänomenale Höhe und Stärke von Teiltönen, *Psych. Forsch.*, 2, 1922, 346ff.

[14] The discussion here follows E. M. von Hornbostel, Beobachtungen über ein- und zweiohriges Hören, *Psych. Forsch.*, 4, 1923, 64-115.

[15] R. Allers and O. Schmiedek, Ueber die Wahrnehmung der Schallrichtung, *Psych. Forsch.*, 6, 1925, 92ff.

The figure-ground properties of visual fields find an analogy in hearing. Sounds arise from a background of stillness or a diffuse acoustic plenum. The sharp outline, solidity, and definite localization of diotic sounds correspond to the figure. Monotic sounds are less objective, more unstable and less predominating than diotic. The effectiveness of a sound is greater when heard with two ears than when heard with one, even though both are of the same intensity.[16] Diotic hearing is the result of the formation of strong configuration, in Köhler's sense.[17] The transition from strong to weak configurations follows this general law of hearing: diotic hearing changes into monotic when the stimulus becomes in any respect less unified. This change must be due to a change in central physiological processes which have, in time, become weak configurations. The strongest central configurations arise from a stimulus in the median plane, and are the more effective, the higher the frequency of the stimulus, the more pronounced its wave-form, the louder it sounds, and the shorter time it lasts. Monotic hearing, on the other hand, mediates the perception of distance in sound; for the nearer a sound seems to be (with certain exceptions), the more closely it approximates to monotic hearing. Attributes of distance, like "near" and "far," are as basic phenomenological qualities of sound as the so-called primary qualities, because the soft sound is always judged as far, the loud sound as near, while soft near sounds are confused with harsh, sharp far sounds.

So far we have been dealing only with non-temporal auditory configurations perceived in a specious present. Such phenomena may be called static or stationary, after Köhler, because there is no such experience of a change in time as there is, *e.g.*, in a melody, which is a temporal structure.[18] When, therefore, the components change in time and the time intervals affect the components (*cf. allegro* and *andante* tempos), we say there are dynamic, temporal configurations. The rate of change may exercise a marked effect upon the phenomenal structure: it is said that Charles Darwin was unable to recognize the melody of "God Save the Queen" when it was played very fast. Stimuli may reach each ear at different times and yet be perceived as simultaneous, transformed into stationary structures, provided that the time-interval between them is not too great. Experiences of simultaneity may be said to correspond to Wertheimer's simultaneous stage of apparent movement. If the second stimulus is too late to form a unitary structure with the first, no *Zeitspannung* can take place, and a new process is the outcome. This new process will appear as a sound which has been displaced to one side, as in monotic hearing, from the median plane of diotic hearing. Audition may thus be an instrument for spatial orientation no less than vision.[19]

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[16] von Hornbostel, *op. cit.*

[17] According to Köhler, weak configurations do not depend upon a determining topography for their form and have no real space form. The temperature of the bodies in a room (or molecules in a gas) is the same, but a change in the temperature of the one is accompanied by a change in the temperature of the others; the division of warmth is a *Gestalt*, but does not depend on the form of the objects. *Physische Gestalten*, 126.

[18] Köhler distinguishes five different kinds of configurational states, which must be kept in mind during the rest of this paper: (1) static states are represented by bodies in perfect equilibrium with no apparent motion, like a book lying on a table; (2) stationary states preserve a characteristic form though the parts are changing, as in the flow of current at a given point; (3) quasi-stationary states include those which are too slow to reveal any dynamic factors but are really changing; (4) periodically stationary configurations are found in resonating membranes; (5) dynamic configurations can be viewed in change, *e.g.*, the fall of a body in a vacuum. *Physische Gestalten*, Introduction.

(19) *Tactile Configurations*

Owing to the large number of varied qualities mediated by the end-organs within and below the skin, the number of tactile patterns must be very large. The possibilities of spatial patterns of warm and cold, pain and pressure, and other qualitative combinations have yet to be investigated. An interesting problem calling for consideration by the configurationists concerns the effect of the various tactile qualities upon each other as they blend in physiological or phenomenal configurations. The phenomena of tactile phi or apparent movement on the skin have been investigated by Benussi and Scholz, and striking similarities have been established between touch and vision.[20] On the other hand, Gelb and Goldstein doubt the existence of a tactile space except as built up through vision and kinaesthesis, so that, if their position proves tenable, a large class of spatial configurations must be denied to this sense modality.[21] Temporal configurations may be found in this domain; for any change in time may be regarded as a dynamic process possessing configurational properties. Indeed, von Ehrenfels mentioned reddening, cooling and blanching as examples of changes characterized by a peculiar form-quality.

The doubtful status of spatial configurations on the skin leaves us only the qualitative patterns to consider. Von Frey has pointed out[22] that, if Müller's theory of the specific energies of nerves is true, then sensations mediated by the nerves would be capable only of quantitative changes, while every slight change in the quality would require a different kind of sense organ,—a requirement which has not and is not likely to be met in physiology. So the view has grown that the kind of stimulus, the manner of stimulation, the temporal relations of the stimuli, and their spatial juxtaposition all affect the quality of the resulting experience. Two tactile stimuli applied simultaneously or successively may influence each other in a variety of ways, depending upon their mutual intensities, position on the skin, and the time interval between them. Contrast, fusion and compensatory phenomena are matters of common enough occurrence to illustrate the qualitative and quantitative changes which single "sensations" undergo when several stimuli combine in the nervous system.

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[19] The argument in the main follows von Hornbostel.

[20] V. Benussi, Versuche zur Analyse taktil erweckter Scheinbewegungen, *etc.*, *Arch. f. d. ges. Psych.*, 36, 1917, 59-136; W. Scholz, Experimentelle Untersuchungen über die phänomenale Grösse von Raumstrecken, die durch Sukzessiv-Darbietung zweier Reize begrenzt werden, *Psych. Forsch.*, 5, 1924, 219. An account of Scholz' work has been given in the second article cited in note 1.

[21] A. Gelb and K. Goldstein, *Psychologische Analysen hirnpathologischer Fälle*, 1920, 157-251. For a brief account see J. Fröbes, *Lehrbuch der experimentellen Psychologie*, 1, third ed., 355ff., 365f.

What has been taken to be a simple, elementary sensation, like the prick sensation of Goldscheider, is in reality a complex experience, says von Frey, for which as yet no satisfactory analysis into components has been made. Hence disagreements may arise regarding this sensation, since different investigators may have been talking about different aspects of a complex phenomenon. Even the stimulation resulting from the mechanical application of single points on the skin is attended by irradiation, which forces us to regard the resulting complex as a unitary whole and not as a plurality of juxtaposed excitations. Just as the stimuli for apparent movement are separated in space and time, so may tactile stimuli be separated and still be perceived as unitary structures, *e.g.*, tactile phi and the sensation of synthetic wetness.

The difficulty, if not the impossibility, of analysis in the field of touch is due, von Frey believes, to the fact that analysis cannot penetrate into those levels of psychic phenomena which lie below the threshold of clear consciousness (*deutliche Bewusstheit*), perhaps wholly on the physiological level. The integrative action of the nervous system as well as the supposedly electro-chemical nature of its activities implies that the physiological processes at the basis of tactile experiences possess supersummative properties.[23] [24]

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[22] M. von Frey, Ueber Wandlungen der Empfindungen bei formal verschiedener Reizung einer Art von Sinnesnerven, *Psych. Forsch.*, 3, 1923, 209-218.

[23] Köhler, in *Physische Gestalten* (244), feels that tactile space forms may be perceived without the aid of associated visual images. Gelb and Goldstein in the article cited above come perilously near an associationistic theory in trying to account for their abnormal patient who depended mainly upon kinaesthesis for his perceptions of form. If this difference of opinion is taken seriously, it means that much of Köhler's physiological theory must be changed, perhaps some fundamental assumptions in *Gestalttheorie.*

[24] There is little to be said about taste and smell configurations. It may well be doubted if they exist in any well-defined sense. In so far as all processes in time may be regarded as subject to configurational principles, taste and

(20) *The Problem of Relations and the Psychophysical Judgment*[25]

How can dynamic processes extended in time form a unity? How is it possible to judge the relations between two or more stimuli separated in time if there is no *real* basis for the judgment? These and many other questions concerning the temporal configurations call for answer. With them we shall find involved problems of memory, recognition and reproduction. Prevalent theories designed to explain the continuity of experience and behavior rest upon the concepts of association or frequency-linkage, special attentive or unifying acts (simultaneous and successive), memory images and the like. In fact it is not too much to say that every system of psychology built upon atomistic-associationistic assumptions has failed to make a logical transition from the cross to the long-section facts of behavior and experience. The attempt to build complex patterns extended in space and time out of successive phases of simple units illustrates a general tendency on the part of psychologists to cling to a simple logical scheme whose weakness is repeatedly demonstrated when it is brought to the bar of experimental observation and verification. Since the units composing experience or behavior have been present in the past, the associationistic-frequency theories ultimately rest upon the magical effects of "past experience," which can be made to explain anything in any way one likes. Yet, strange to say, the shortcomings of the associationistic theories rarely led to anything more than additional assumptions serving to bolster a simple, but inadequate logic, while the fundamental point of view remained unaltered.

There are really a number of problems calling for solution when we consider the unity of experience and the fundamental facts of behavior as we find them exhibited in processes running their course in time. The simple associationistic theories do not tell us how it is possible to say that *a* is greater than, equal to, or less than *b* when *a* and *b* are offered either simultaneously or successively. Actual observation proves that images, secondary processes and other introspectable residua may be entirely absent when a judgment is made; hence the concept of past experience does not help unless it is used in a very definite, restricted sense. We can best approach the problems of the temporal configurations by a discussion of the psychophysical judgment and of relations, since these are basic in the transition from

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smell would have to be included. Adaptation, fusion, compensation and contrast may well be configurational in character. Probably the considerations applying to the determination of thresholds, discussed in the next Section, are true of all sense modalities.

[25] This Section should be read in connection with the following three Sections, particularly 22. Here we find the basic considerations entering into all temporally extended configurations.

cross to long-section phenomena. Whatever the result of analysis, we must be able, if our psychology is to make any pretense at being systematic and complete, to deal with dynamic processes in time with no break in theory and without the introduction of assumptions *ad hoc* (memory images, memory color, unobservable acts). The solutions offered by the configurationists of the psychophysical problem rest upon fundamental divergences in theory which have important implications for the even more complicated problems of thought and action.

In this Section I shall therefore limit the discussion to several fundamental questions in psychophysics, leaving it for the later Sections to trace the consequences of the facts here established. To conceive the problems in clear-cut fashion we may formulate them as follows: (1) What is a comparison and how are relations perceived? (2) How can we explain the so-called false judgments, or rather, the lack of agreement between phenomenal and physical structures? (3) What does the differential threshold really mean?

(1) The real importance of the first question was appreciated by von Ehrenfels and Meinong, who devoted considerable attention to judgments of likeness and difference. But their solution had at least two grave defects, typical of later psychological theories: first, by attributing the comparison and the relation upon which it is based to the activity of a subject or an "ideal" process, they withdrew the whole problem from control and observation and the domain of the real,—in fact, for Meinong and his school, relations and judgments may be real or ideal, the distinctions finally merging into logic and epistemology; and secondly, they conceived the terms (or stimuli) which are related to be prior to the relations and somehow built upon or added to them, hence less essential, often capricious and optional. Thus Stumpf, in this vein, asserts that relations are perceived in the sensations, at least two of which must be given, but the relations are not as palpable as the relata,—wholly overlooking the fact that neither sensations nor isolated units of a connecting relation need be perceived as such in order to form the basis for a comparison; for a unitary structure may be perceived as a "step up" or a "step down" with no hint of the relation of "greater than" connecting the members of the configuration. Still other accessory hypotheses have been made in order to bolster the element-theories, which begin by assuming that each unit may be observed independently of the other: thus we are told that judgments may be influenced by secondary criteria in the form of secondary sensations which form transitional experiences hovering about the primary data (Jaensch); unnoticed sensations are responsible for judgments of "less" when the response should be "equal" or "greater"; or the memory image or images may have lost in intensity; finally, the "relational process" may be misguided so that psychophysical errors are due to errors in judgment (*cf.* Stumpf's distinction between relations of sense and judgments of precision).[26]

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[26] For the original sources of the various points made in this and the following five paragraphs see: K. Koffka, Psychologie der Wahrnehmung, *Die Geisteswissenschaften*, Part 29, 1913, 712-716, and 796ff; W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psych. Forsch.*, 4, 1923, 115-176.

The configurationists approach the problem of relational judgments from a wholly different point of view. Relations and relata form a single, unified structure. The judgment is usually made in terms of the configurational properties of the stimulus situation, and not upon now one element and now another, the relation supervening somehow upon both. In order to make a simultaneous or successive comparison the things compared must be put into a unitary structure. Furthermore, a judgment involving a total structure and a judgment of relations of parts within a structure may present two wholly different phenomenal problems: to compare the sides of a rectangle and to form a judgment of their relations are obviously two different matters. To make a judgment requires more than a mere perception of parts. The parts must stand out in some kind of structure of their own, independent of the totality in which they are found. Judgments of relations depend upon an even more restricted phenomenal field in which various configurational transformations are necessary, depending upon the relations judged. Every problem thus creates a new set of configurational conditions which will modify the final judgment. The instruction plays a large part in determining the phenomenal field to be judged.

(2) We are now faced with the second problem, how to explain false judgments? How can it happen that the heavier of two lifted weights may be called the lighter, the more intense sound judged to be the weaker? Leaving aside for the moment constant errors due to temporal and spatial order, we may formulate the answer of the configurationists by reference to simultaneous and successive comparisons.

It must be admitted that the perception of a relation within a structure may or may not be given, depending upon such factors as set, disposition, habit and the like, which even the configurationists have not been able to dispense with. If the judgment reveals that the phenomenal and physical structures do not coincide, then we must explain the discrepancy. To say that two lines which I falsely report as equal were not equal and to explain the report by an error in judgment or by asserting that certain sensations were not noted is to assume the constancy hypothesis and involves one in the stimulus error. There is no phenomenological criterion of the length of the lines other than their appearance at the moment the judgment is made. It is therefore necessary to account for the judgment in terms of physical and physiological principles which apply directly to the observed phenomenal facts on the simplest set of assumptions. From this it appears that a given physical structure may give rise to a number of judgments based upon the phenomenal pattern. In any case the judgment must be amenable to (configurational) laws. The first assumption therefore concerns the *paired* character of the data upon which the simultaneous or successive judgment is based. If it can be established that the physical or physiological configurations possess properties either responsible for or paralleling phenomenal configurations, then we have found a *real basis* for all judgments and the solution of the problem is no longer to be sought in arbitrary suppositions as to what must be the nature of the processes responsible for the judgment.

We are now in a position to understand what false judgments denote for the configurationist. If, for example, one is required to judge which of two stimuli is the brighter or darker, the brightness of each stimulus is not abstracted from the paper or cardboard in which it inheres, nor is each stimulus considered necessarily by itself. Both stimuli, rather, form a single configuration, releasing configurational physiological processes and a phenomenal configuration. If there is sufficient objective difference between the stimuli to form a step up or down, the judgment will be "lighter than" or "darker than." If the objective difference is not enough to arouse a step-wise structure the judgment will be "equal," a homogeneous phenomenal field being the result. A fourth possibility remains: if now one member of the structure predominates, now another, the judgment will be "uncertain." All judgments can therefore be deduced without the aid of additional hypotheses concerning the nature of conscious processes. Indeed, the speculations of Lindworsky, Jaensch, Bühler and others concerning the mental processes of Köhler's apes, far from giving us a simpler theory, actually attribute more intelligence to the animals than Köhler does. For as soon as we postulate memory images, "transitional sensations," "the noting of material relations," and the like, we must suppose that the animals consciously draw upon their past experience and thus make the required comparison.[27]

Turning now to successive comparisons we are confronted with essentially the same problems in far more difficult form. Effects of time and in time require the most painstaking research before a satisfactory conclusion can be reached regarding the temporal configurations. Time order has been found to be of importance *per se*: the differential threshold for lifted weights, auditory and pressure intensities, and taste qualities is lower when the stimuli are given in ascending order than when given in the reverse direction.[28] The duration of a stimulus may affect reaction time, the latter varying directly with the former. The length of a stimulus or the interval between stimuli is not a property (like its intensity) affecting it momentarily, but rather a factor which affects the totality and the judgment of the whole. Since the beginning and the end of a process are physiologically important points, characterized by marked changes in potential, it is important how far apart the stimuli are from each other in time. The nearer two stimuli are together, the stronger will their total effect be.

We have already dealt with the errors found in simultaneous comparisons and have now to investigate the errors made in successive comparisons. The usual explanation of the fact that more "greater" judgments are given in the ascending order, even when the second stimulus is equal to or less than the first, is that the first stimulus leaves a memory image which loses its intensity in time; and, since the comparison is made on the basis of the memory image, the weaker image of the first makes the second seem greater. But when it was found that observers gave "greater" judgments when no memory image of the first stimulus was present, this explanation had to be given up. Köhler, working with sound intensities, found that the second of two objectively equal stimuli is judged "greater" twice as often as equal; moreover, the greater the interval between the two stimuli, the larger is the number of "greater" judgments. There is therefore a true asymmetry in ascending and descending steps. The second sound is heard, not by itself alone, but as a rise or fall from the first. And since a step is more impressive than an undifferentiated field, the stepwise judgments will predominate wherever conditions are at all favorable for them. Here Wertheimer's law of *Prägnanz* may be at work: every configurational structure tends to become as sharply defined as possible. While a certain minimum of difference is necessary for the steps to be perceived, we find that an ascending order or a lengthening of the temporal interval between the stimuli is sufficient to create the proper conditions for a non-homogeneous or rising structure-function.

We may say with Köhler[29] that the tendency of configurations to be completed in a certain way, *e.g.*, in the direction of sharpness (*Prägnanz*), simplicity, or completeness, is a special case of the more general law that a given direction or end may impend in a phenomenal field; the phenomenal character of the field will include within itself certain goals or futurities (*Herkunftsorte*), although these may not be given in consciousness in concrete, definite contents. It is these configurational tendencies upon which the relational judgments are made, and not upon traditional sensations or other accessory criteria. It thus becomes evident why the investigations of thresholds and psychophysical work in general have been unfruitful: since measurements of sensory experiences were being sought, the sensory data having a more substantial existence than their relations to each other, the relations and whatever knowledge we might have gained of them were thrown outside the pale of consideration. To be sure, masses of statistics and quantitative results bear witness to the industry and perseverance of numerous investigators; but their interests did not lead them to a study of certain fundamental factors in the psychophysical problem which are of immense importance for the psychology of memory, recognition and the higher thought processes.

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[27] Further discussion of this point will be found in Section 22 below. *Cf.* also K. Koffka, *The Growth of the Mind*, 1925, 205ff.

[28] W. Köhler, *op. cit.*; K. E. Leonnard, Ueber Reaktionszeiten bei verschiedener Dauer des Reizes, *Psych. Forsch.*, 4, 1923, 205ff.

[29] Köhler, *op. cit.*, But according to Köhler comparative judgments cannot be explained from the phenomenology of the comparison; for this

In throwing out time errors from memory, when it was found that no memory image was present at the time when the successive judgment is made, the older psychology neglected an important problem of memory. For if we examine the effects left by a stimulus, either in consciousness or (hypothetically) in the nervous system, we shall find a number of facts which call for explanation and whose explanation can help us with memory, recognition, and the dynamic processes in time. On the basis of his experimental work on successive comparisons, Köhler notes the following effects of a given stimulus: (*1*) it leaves a residuum (a dynamic process not to be confused with the static residua of associationistic writers); (*2*) after a number of repetitions with stimuli of the same kind (*e.g.*, sounds), an after-effect is left which changes the fate of subsequent incoming stimuli or their residua; (*3*) a stimulus of the same kind may be recognized after a long interval of time (recognition); (*4*) an absolute impression may be left by a given stimulus; (*5*) one may call up a memory image of a stimulus even after a year's time has passed. It may be thought, says Köhler, that five different after-effects are present in each case. Although recognition and reproduction do not follow the same laws, it is probable that the same kind of after-effect is at the bottom of these two memory performances. The absolute impression, which is also an accomplishment of memory, cannot be said to be identical with the after-effect at the basis of recognition and reproduction; but to assume that it is, is a more fruitful working hypothesis than to assume the contrary. And so Köhler, while confessing that he cannot reduce all five processes to a single psychophysical process, feels that we have the basis for a common set of assumptions to bring these various fields together. Thus the problem of relations in the psychophysical judgment may be of tremendous importance for the psychology of memory, thought and action.

(3) It will be remembered that we still have a third question to answer concerning the differential threshold. What is the explanation of the differential threshold and why does it exist? From the preceding discussion it becomes clear that the Weber-Fechner law applies to pairs of

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we must go to the nervous system. Here each stimulus leaves a modification (*Spur*) which has configurational properties, an inner tendency toward *Umbildung* in the sense of increasing definiteness and therefore possessing *relativ feste Bedingungen zukünftigen Geschehens* (146). Köhler presents three physiological hypotheses and seems inclined to accept the third as the best.

stimuli and their paired character and not to single sensations.[30] The old psychophysical view, therefore, that we are judging single sensations when making psychophysical judgments, is false; the judgment concerns the relata and their relation. In other words, while it is possible to collect quantitative data concerning the magnitude of experiences in various regions of the scale, the real meaning of the differential threshold is to be found in the light it throws on the problem of relations and *das Zueinander* of stimuli and their effect upon phenomenal fields.

Some of the configurational experimentation bearing upon the paired or structure-function effects of stimuli upon differential thresholds is worthy of note at this point.

Gelb and Granit investigated the color limen for a gray field when it appeared as part of a figure and when it formed part of the ground, the intensity remaining in each case the same.[31] The figure field was chosen both when it was darker and when it was lighter than the ground field. The fields were illuminated in part by lights of various colors, and the limens were determined for each. The results confirm previous findings concerning the fact that the color limen is higher for brighter fields than it is for darker. More important is the fact that the limen is different for a field when it is figure from what it is when the field is ground, even though its brightness value is unchanged. The limen for the figure field is greater than that for the ground field. The difference between limens for figure and ground is so much the greater, the greater the difference between figure and ground. In general it may therefore be said that the figure inhibits the perception of colors since its limen is greater.

Koffka had already pointed out that the color limen does not depend merely upon the brightness of the gray with which the color is mixed but also upon the total structure consisting of the tested field and its surrounding fields.[32] The greater the structure-difference between the brightness of the field tested and its background, the higher is the color limen and the more difficult is it to form a color structure. Gelb and Granit's results rest upon differences arising from figure-and-ground, not from differences in brightness, primarily because the brightness structure in their experiments remained the same.

These authors reject contrast effects and attention as explanations of their results. While it is true that a figure makes itself more impressive and that attentive factors are more favorable to the figure, still its limen is the higher. They believe that "distraction" worked equally against figure and ground, so that this cannot either be the causal factor in the situation. The explanation must be sought, they assert, in the fundamental distinction between figure and ground. The greater energy-density of the figure and its more concrete psychological characters go hand in hand with psychophysical processes that are opposed to any change. This is only a special case of the tendency of a figure to become and remain a simple, univocal, precise configuration, homogeneous throughout, resisting change. It must be remembered, however, that the results of these experiments cannot be generalized to include all figures, *e. g.*, figures lacking clearness.

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[30] The physical theory will be found in *Physische Gestalten*, 211ff; E. Becher, in his review of *Physische Gestalten*, presents a simplified account, see *Z. f. Psych.*, 87, 1921, 34. The perception of rhythm, time, and similar phenomena should come under this Section but lack of space forbids.

[31] A. Gelb and R. Granit, Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle, *Z. f. Psych.*, 93, 1923, 83-118.

[32] K. Koffka, Ueber Feldbegrenzung und Felderfüllung, *Psych. Forsch.*, 4, 1923, 176-203.

A more exact determination of the factors influencing color limens was made by Ackermann[33] and Eberhardt.[34] Ackermann determined the limens for different colors on inner fields with different surrounding gray fields, and for different inner fields with the same surrounding gray fields. A color was added to the inner gray field until the *O* could just notice and report on the quality of the color. The general conclusion to which Ackermann comes is that the color limens increase with the difference in brightness between the inner and outer gray fields. The critical-gray explanation of Hering and various other explanations do not suffice to explain all the facts. The theory which shall explain all must take account of many variables in the total situation. A further determination of these variables was attempted by Eberhardt.

Eberhardt wished to know what other factors besides brightness influence the color limen. She first investigated colored fields on neutral grounds. Here it was found that the color limen depends simultaneously upon three factors: brightness value of the inner field, brightness difference between the inner and outer fields, and brightness distance between the inner field and the outer field. All three of these factors must be controlled if we are to account for the color limens of gray fields. These dependencies hold for supraliminal colored fields as well as for the liminal colors.

Neutral fields on colored grounds were next observed. The question here is how the three factors enumerated above influence the quality and intensity of the contrast coloring in the inner fields. It is a well-known fact that contrast effects are best when brightness effects are least and the fields are most alike in brightness value. For the appearance of a contrast color, the intensity of the inner neutral field is of no account (when the ground is colored). It does influence the color tone of the contrast color, but in itself it is neither favorable nor unfavorable to the appearance of a contrast color. A brightness difference between fields diminishes contrast colors, which appear best when the brightness difference is reduced to a minimum, *i. e.*, when both fields are equal in brightness.

The limens of color and brightness for a field surrounded by fields of various brightnesses depend, therefore, upon the brightness value of the inner field, its brightness distance from the other two, and the white value of the inner field. The brightness interval between the fields influences not only the phenomenal effect of the brightness of the stimuli but also the appearance and saturation of the color aroused by contrast on an objectively neutral field.

We have been dealing above with factors within a configuration which exert an influence qualitatively upon the parts of the structure. We may turn to a study of conditions imposed from without a configurational structure, and note how these may modify internal qualitative characteristics of the configuration. In other words, we may say, with Koffka, that this story concerns the dependence of phenomenal field properties on external configurational conditions; or we may say that any given field is a part of a more complicated configuration when certain determinants outside its own boundaries modify its internal structure.

Koffka investigated fields which are phenomenally homogeneous when objectively they were not, and fields which appear heterogeneous when objectively they were homogeneous.[35] An example of the first is found if one replace the broken black line of the Masson disc by a solid black line: a homogeneous white surface will be seen, whereas with the usual Masson disc one sees rings of varying brightness. Some *Os* do not perceive the homogeneous appearance of the pseudo-Masson disc owing to an "unnatural" attitude. It is possible to destroy the homogeneity by objective means also, namely by changing the conditions of the perception. Koffka measured the brightness of this field by equating it to the brightness of an objectively homogeneous field, and found that it approached the geometrical mean brightness of the latter.

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[33] A. Ackermann, Farbschwelle und Feldstruktur, *Psych. Forsch.*, 5, 1924, 48-84.

[34] M. Eberhardt, Untersuchungen über Farbschwellen und Farbenkontrast, *Psych. Forsch.*, 5, 1924, 85-130.

In order to make an objectively homogeneous field appear heterogeneous Koffka placed a ring between a darker and lighter field. The middle field then showed a brightness gradient throughout its whole width. Such fields are used to demonstrate marginal contrast. Koffka does not accept the Hering theory as an explanation of this phenomenon, because it is stated in terms of the single effect of each field on the central field. So, too, with Mach's theory. These theories are also rejected for mathematical reasons, since a mathematical formulation of what must occur in order for the brightness gradient to appear shows that a darkening of one side of the middle field is not independent of a brightening effect in the other. The middle field either becomes homogeneous or does not, depending on whether the boundaries on both sides fulfill certain conditions. Stated mathematically, the two boundaries must change with respect to each other; and this change can be described by means of a parameter until now neglected, the arithmetical and geometrical means of the surrounding brightnesses. Koffka thus rejects the Mach-Hering explanations as atomistic theories, because they take cognizance of the effect of only one field at a time; the facts call for a more complicated function, which can be expressed only by taking into consideration the influence of both fields acting upon the middle field as a single unitary structure.

(21) *Configurational Aspects of Response Patterns*

Behavior or reaction patterns are dynamic configurations requiring time for their completion. The varied activities of the organism from the simplest reflex to the most complicated form of purposive behavior have a beginning, a transitional, and an end stage. From another point of view, every act appears as a figure upon the general background of the total organism. Even phasic, isolated acts, like the raising of an arm or the singing of a note, involve the tonic level of the whole muscular and nervous system. Every act modifies the existing state of the organism, leaving a more or less permanent effect upon it in the form of configurational dispositions or attitudes. The sum total of configurational dispositions and capacities within an organism determines the phenomenal and reaction patterns of which it is capable. Single part-responses neither run their course independently of each other nor are they linked by chance: the past, present and future needs of the organism combine to modify every single act in the economy of the whole. We may begin our consideration of the experimental work in the field of behavior proper by considering first the simplest motor configurations and the laws which govern them, and then proceeding to the more complex patterns.

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[35] K. Koffka, *op. cit.*

Werner asked observers to draw simple figures, with eyes closed, under two different attitudes.[36] Under the non-membered attitude two lines forming a right angle may be regarded as a single notched line; under the other, as two lines enclosing a surface ("membered" attitude). What differences appear in the drawings, and therefore in the reaction patterns, under each attitude? Three possibilities are open: (1) the figure may be reproduced with little or no change, and this case we need not consider, since the given configuration is preserved. (Wulf called this the "conserving" tendency); (2) the lines may be bowed in so as to form a semi-circle (Wulf's tendency toward Nivellierung); (3) the lines may be bowed out, accentuating the angular appearance of the figure (Wulf's tendency toward *Präzisierung*). Under the non-membered attitude the figure drawn tends to become homogeneous, for here an assimilative tendency predominates: when the property of the whole dominates over the parts, the individual members lose their individuality and become absorbed in a more or less homogeneous structure. Under the membered attitude the angle is preserved and strengthened because the members are differentiated more and more from each other, in form, size and position. The membered attitude accentuates the dissimilative tendencies within a configurational structure. The configuration attains its fullest *Prägnanz* by the individualization of its members, *e.g.*, contrast effects in vision. Werner found that these general assimilative or levelling dissimilative or sharpening (*Präzisierung*) effects hold for auditory and speech phenomena.

The same laws of assimilation and dissimilation hold for the response patterns in speech and govern changes in accent, pronounciation and length of syllables. Werner's attack upon the problem of language-change stands in striking contrast to the associationistic studies made upon the same problem by other psychologists and philologists interested in the growth of language.[37] Werner considers words as structures, built about certain peaks or vantage points. Thus the word *getan* may be pronounced by accenting the *t* (simple centering) or by separating the syllables *ge-tan* (double centering). In the first case the peak is preserved and the rest of the word assimilates itself to it qualitatively. With the double centering there is a tendency to sharpen the members rather than to fuse them in a homogeneous structure. It is in these assimilative and dissimilative changes that we find the basis for changes in pronounciation, accent and tonal quality of words. These changes occur only when the parts are members of a single configuration and are not merely "neighbors,"—a view which is opposed to the widely prevalent assumption that changes in language are a result of association (frequency) with words of similar classes or categories. We thus see how the growth of language may be conditioned by configurational principles.

From the configurational point of view perception and action cannot be separated; the one flows over into the other. There is therefore no break in continuity between phenomenal and behavior patterns. In order to understand the reaction of the organism to its environments, it is first necessary to understand the effect of the world upon the organism; hence an introspective report may throw light upon explicit behavior. Since the configurational concepts are not limited to static events in the nervous system, perception may be regarded as a stage preparatory to action. If an object appears "friendly", this *Aussehen* implies my reaction to it as well as my perception of it. Any cross-section of consciousness or behavior is meaningful only as it points toward a completed whole; so that we must introduce affective, appreciative and evaluative descriptive terms if we are to account for the complex behavior patterns in actual life. The meanings which point to action are more primary than the qualities in experience, since the latter involve isolation from a total situation before they are experienced. But such terms as ugly, beautiful, sad, gay, benevolent or malevolent imply dynamic properties of situations which lead from perception to action.

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[36] H. Werner, Ueber das Problem der motorischen Gestaltung, *Z. f. Psych.*, 94, 1924, 265-272; F. Wulf, Ueber die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psych. Forsch.*, 1, 1922, 333-373. Wulf's work bears more particularly on memory and will be taken up in Section 22.

[37] H. Werner and E. Lagercrantz, Experimentell-psychologische Studien über die Struktur des Wortes, *Z. f. Psych.*, 95, 1924, 316ff.

Meaningless reflexes as the units of behavior find little favor with the configurationists, smacking as they do of the associationistic-frequency theories according to which intelligent behavior consists of nothing more nor less than combinations of unit reflexes knit by trial and error. The summative theories must assume too many complications to explain even the simplest successful act: it is inconceivable, Koffka argues,[38] that a baby is furnished by nature with all the connections necessary for fixation within two or three days after birth. Since every fixation would require a different system of connections, if the object fixated were slightly displaced, the number of pre-determined mechanisms for successful fixation early in life would have to be enormous. Instead of the "pathway" hypothesis, Koffka regards the sensory, motor, and phenomenal areas as a single system wherein *what is to be seen* determines the eye movements as well as the afferent and efferent impulses. "The alteration of conditions (within the sensory and motor parts) must therefore take place in accordance with the greatest possible simplicity and equilibration of forces: and the principle of the greatest horopter will be found in harmony with this requirement."[39] The simplest reflex acts are therefore not mere mechanical products, the result of predetermined connections, but rather organic activities tending in certain definite directions.

The view that instincts are merely chained reflexes or blind compulsive activities is also given up by the configurationists. The facts upon which the mechanical theories of instinct founder are several: (1) there is persistence with varied effort until the goal or end is attained; (2) the movements continue in the direction of the goal to be attained and cease only when it is attained; (3) that each part-activity attains meaning only when viewed in the light of the whole or of the consummatory response. The problem which confronts the configurationists is to steer between the blind mechanism of the frequency and conditioned-reflex theories and the vitalism of Bergson, Driesch and others. The configurational solution consists in describing instinctive activities in terms of dynamic processes whose formal properties can also be found in the inorganic world. Thus the problem of instinct becomes a special case under the more general problem of the dynamic temporal configurations. We must regard all temporal configurations as possessing a beginning, a transitional and an end stage. Since the inner, emotional behavior fits the external behavior, it follows that "the emotions are dynamic phenomena conditioned in time" and can be treated with the instincts. "So long as the instinctive activity is incomplete, every new situation created by it is still to the animal a transitional situation; whereas, when the animal has attained his goal, he has arrived at a situation which to him is an end-situation.—Instinctive activity then becomes an objective mode of behavior analogous to such phenomena as rhythm, melody and figure."[40] If we seek for the nature of the nervous processes responsible for these activities and if we desire a parallel for these modes of behavior in the inorganic world, we can turn to Köhler's concept of "closure," which is defined as follows: "In every process which issues at all in an end-situation, independent of time (although it is possible to apply this to events occurring in time), the mode of distribution (of energy) shifts in the direction of a minimum of configurative energy."[41] The laws of simplicity, *Prägnanz,* and completeness, therefore, would apply to behavior patterns in time. Koffka's final word on the relation of configurationism to vitalism may clear up any misunderstanding which a superficial reading of the *Gestalttheorie* may inspire: "Although instinct is still a riddle, at least it is no longer one which forces upon us the acceptance of psycho-vitalistic principles."[42]

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[38] K. Koffka, *The Growth of the Mind*, 1925, 71ff.

[39] *Ibid.*, 80.

Voluntary activity does not separate very readily from non-voluntary activity, instinctive from intelligent behavior. It is probable that in voluntary activities, as distinct from non-voluntary, phenomenal fields undergo transformation, so that insight or understanding governs the part-activities in the light of the total pattern or situation. Thus Köhler's apes would fabricate a tool when the object and the goal were brought into the tool-goal configuration. But before this could occur, it was necessary for the object of perception to undergo a complete alteration, to be *seen* as the proper object to fill a certain gap or to satisfy a certain need.[43] It is not a question, then, of explaining each part-activity in and for itself and then of asking how the various responses become knit into a meaningful whole, as the associationistic and chance theorists do, but rather of explaining the solution and the sense it has for the animal. When the goal can be reached only in a roundabout way, as in Köhler's experiments, and each act has no meaning except as seen in the light of what the animal is after, we must infer that the end is the controlling factor in everything that the ape does.

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[40] *Ibid.,* 102f. [41] *Ibid.,* Koffka quoting Köhler's *Physische Gestalten,* 250.

[42] Koffka, *op. cit.,* 106.

[43] An example showing how necessary a perceptual transformation is before an adequate structure-function reaction can take place may be found in the following: if a table is needed for a certain purpose it may be entirely neglected by the ape if it stands in a corner, fitting closely into it. The table and the walls form such a firm structure that the ape is unable to separate the one from the other, and the table is neglected. The ape will tear a board off a box if there is a space between it and the other boards, otherwise the top forms a closed whole unsuitable for the solution of the problem. More will be said later concerning the transposition from one configuration to another.

The development of rational behavior consists, then, in the ability and choice of more and more adequate and intricate structure-function responses according to the general configurational principles enunciated above.

(22) *Recognition, Learning, Memory and Reproduction*

Current explanations of recognition, learning, memory, and reproduction rest upon the concept of the mental element and the mechanism of association. Repeated experiences are regarded as the causes of association, and the resulting association then becomes the moving force in recognition and reproduction. Sometimes the word 'habit' is used to denote both the mechanisms and effects of associative learning. Subsidiary aids in learning and memory performances reveal how association and the mental element go hand in hand: a given sound, taste or form is recognized or reproduced because memory images from past experience or other secondary processes accrue to a present perception and modify the percept or reaction accordingly. To be even more specific, let us take the problem of color constancy: colors maintain their pristine hue and saturation under illumination that should give wholly different effects; thus, a white sheet of paper looks white under illumination that should make it appear yellowish or reddish. The explanation commonly advanced for phenomena of this kind is that a "memory color" (Hering) of the object overlies the sensational presentation and preserves its erstwhile appearance. The image and the sensation fuse by "assimilation." Assimilation is enhanced by frequent repetition of experiences. Mental processes thus condition each other through frequent association. All memory performances reduce to the reproduction of elements which have frequently been attached willy-nilly to one another.

It is clear that "associations from past experience" can be made to explain anything in any way that one pleases; the treasure house of the past contains an infinite variety upon which one may draw. But memory images and past experience explain either too much or nothing at all; for they can be made, if manipulated with sufficient dexterity, to support any proposition or its contradictory. That we must penetrate behind the linkages formed in past experience is attested by experimental observation, seeing that two items associated a thousand times in the past may not be associated the next time one of them is presented. Why? Pure frequency theories afford no answer,—not so long as we remain faithful to them.

What have seemed to be the simplest assumptions (for did they not operate with the "simplest units" of experience or behavior and the simplest mechanisms?) really prove to be gratuitous in the face of the facts and hardly capable of experimental observation and verification. Let us revert once more to color constancy. Tests made with chicks only a few months old show that when trained to pick the white (brighter) of two objects they still choose the (really?) whiter when it is in the shade and actually reflects less light than the darker nearby. It can hardly be said that these animals have a memory color of the white object as it appeared in normal sunlight. Even as able and careful a thinker as Bühler (I omit here the speculations of Jaensch, Lindworsky and others) is really forced to speculate more about the mental processes and memory capacity of the ape than Köhler does in his doctrine of "insight." Bühler hopes, by means of traditional associationistic-sensationalistic assumptions, to show that the ape possesses no structure-function reactions. Let us see what Bühler offers in place of Köhler's description: the ape is able "to feel itself towards the end-situation of attaining its goal, which is 'not difficult to explain theoretically as a memorial after-effect of successful pursuits of a goal in the past.'" Moreover, the animal is able to "note" material relations in the problems he is called upon to solve, just as he notes sensory contents![44] Both Koffka and Köhler have repeatedly emphasized the fact that to note relations and to perceive a configuration are two entirely separate affairs, involving different phenomenal fields and structure-functions,—probably in the case of the relation perceiving attitudes, very much more complicated perceptual patterns. Furthermore, it is a matter of pure speculation what the animal remembers from past situations, what he notes among the relations present, and how his mental processes work. The memory image and the associationistic assumptions formulated *ad hoc* have yielded few implications for further research, factual verification or prediction.

The problem of recognition reduces to a more simple scheme in the configurational psychology. Neither identical elements, nor old pathways nor former functional centers have to be recalled to life in order that objects, persons, or situations may be recognized. By hypothesis the configuration is a transposable form; only similar structures need be present, and not identical elements, when recognition takes place. Since experience follows configurational laws we should expect to find structures in all experience, and hence to find the formal conditions for recognition present quite universally. Both everyday observation and experimental results confirm this supposition. The tendency to identify nonsense syllables may be so strong as to make useless all attempts to induce a neutral attitude on the part of the observer or to get a purely mechanical response from him. To be sure, past experience plays a rôle in present perception and action,—but not by means of elementary residua associated to other units through frequent repetition. Configurational attitudes, dispositions and tendencies still play a large part in *Gestalttheorie*, but in radically different fashion from the older mechanisms. We are told that, once configurational structures have appeared in experience, their reappearance in the future is easier even though the objective conditions are less favorable. In accordance with the laws of completeness,

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[44] Quoted from Koffka, *The Growth of the Mind*, 207. This whole section in Koffka's book, 205-230, should be read by everyone interested in animal psychology and the fundamental concepts of general psychology; some of the most acute criticism of traditional concepts will be found here.

Prägnanz, and perfection (to mention only the more important), a part of a configuration may facilitate the whole. Phenomenal and behavior patterns tend to "become as good as possible" and do not follow the laws of chance.

There are other factors, besides the form of a structure, which are important in recognition. Stated generally, they concern the properties belonging to the configuration as such. I can give here only one or two illustrations. Rubin found that a field is more apt to be recognized if it is seen twice as figure (or as ground) than if perceived now in one way and now in another.[45] Often, if the field had been seen as figure and later as ground (the stimuli being ambiguous figure-ground configurations), it was not recognized as the same. The difference between figure and ground is therefore not one of degree (frequency of association) but one of kind; figure and ground possess wholly different phenomenal properties. Numerous experiments show that when the formal properties of different sense modalities do not coincide, identical stimuli appear as different objects and may pass unrecognized.[46]

We may now turn to the phenomena of memory and learning. Here, too, chance-frequency theories have dominated experimental procedure and interpretation of results. If an animal cannot survey the whole situation in which his problem is enmeshed, the result will be a chance performance and a smooth learning curve. But trial and error, frequency, and associationistic theories do not explain the sudden drops or rises in the learning curve, nor for that matter do they alone explain any learning whatsoever; for additional hypotheses, such as the stamping-in effect of the pleasant consummation, must be brought to the support of the original theories.[47]

We come now to an important factor in learning, memory or retention. We may regard memory from the standpoint of what is or is not retained, and we must also consider the changes which occur in retained material after a lapse of time. The typical explanation here is also associationistic: frequent associates tend to strengthen one another, while the rest drop out. Köhler has shown that the apes retain structure-function reactions after a lapse of several weeks, while the absolute choices in which unit elements within a situation are learned last only a few days. The work of Lewin on association proves that mere repetition of associates does not play an important part in memory performances.[48] The laws of retention must be reformulated in terms of structure-function activities.

We have next to inqure concerning the changes in remembering or learned material. Müller's theory of memory changes may be contrasted with the configurational interpretation.[49] According to Müller, the tendency or direction of ideas is a result of a law of memory by which remembered things lose their determinateness and become more alike as time progresses. He asserts further that images belonging to the same sense modality but to different objects converge toward a state of unclearness in which their differences are obliterated. This law, the "convergence principle," is supposed to possess universal validity. Wulf's investigation was designed to test both this theory and the associationistic structure upon which it rests.[50]

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[45] E. Rubin, *Visuell wahrgenommene Figuren*, 1, 1921, 28.

[46] *Cf.* von Hornbostel's findings, this JOURNAL, 36, 1925, 525f.; also Section 24 below.

[47] For the proof of this statement see Koffka, *op. cit.*, 153ff., 234ff.

[48] Lewin's work has important consequences for all the facts so far discussed in this Section; but I have reserved discussion of it until p. 50 below, when it can be brought in at length.

Wulf exposed simple figures for 5 sec. and more complicated figures for 10 sec. After intervals ranging from 20 min. to 24 hrs. the observers were asked to draw what they had seen. In comparing the reproductions with the originals, the fact was noted whether special properties of the figures were preserved, omitted, or sharpened. These configurational tendencies were designated as conserving, levelling (*Nivellierung* or *Niv.*), and sharpening or accentuating (*Präzisierung* or *Pr*). Extensive introspective reports accompanied the reproductions. The results show that with the exception of 8 cases there is always a clear deviation from the original in the direction of *Pr.* or *Niv.*, while comparisons of reproductions of the same original made at different times indicate a continuous change in the direction evident in the first reproduction. The quality of *Pr.* or *Niv.* could be recognized more or less strongly. At times the change was so marked that a new configuration was drawn, and the observer reported that he had no memory of the original. (The observers were good, bad and indifferent visualizers). Nor were the reproductions based on imagery: for (1) the copies were not copies of images; many of the observers had poor, fleeting or no imagery; and (2) often the image and drawing showed no correspondence when both were drawn.

The changes in content which appear after a lapse of time may therefore be said to be subject to configurational laws; *Pr.* and *Niv.* manifest uniform directional changes. Both may appear within the same reproductions, for they are not antithetical. And in view of the fact that these tendencies were not accompanied by affective states, they cannot be identified with Müller's affective transformations. Nor can the changes in the reproductions be ascribed to Müller's convergence principle, according to which ideas tend to become more alike as they lose in clearness during temporal intervals. The laws of *Pr.* and *Niv.* are special cases of the more general law of *Prägnanz*: configurational changes in memory, as well as in perception, tend toward the sharpest and best form possible, *i.e.*, every configuration tends to become as "good" as possible. In other words, two configurations either approach each other in symmetry and form (or the members of a configuration do this, in which case we have what Werner calls "assimilative" changes tending toward homogeneity), or their differences become accentuated so much that two new configurations are formed, (Werner's "dissimilative" tendency). We find thus that the accentuating, levelling and preservative tendencies may be regarded as exhibiting typical configurational changes in memory functions.

What an individual can reproduce represents his highest memory performance *par excellence.* Reproduction in one form or another is the test of the amount of memorization, retention, learning, and accomplishment. Since it was earlier assumed that these functions are nothing more or less than the result of association, it followed that the capacities behind them could be measured by excluding all meaningful factors and isolating the associative mechanisms. The upshot of all the work on memory based on these theories has been to collect statistics having little or nothing to do with actual learning, since in daily or professional life people do not memorize nonsense syllables. The most disheartening fact in the whole situation is that it now appears that, when all meaningful factors are ruled out and the other conditions required by the associationistic assumptions are fulfilled, no learning at all takes place, no associative bonds are established. The absolute failure of associational mechanisms (if there are any) demonstrates what it means to take the atomistic principles seriously and to evaluate them under exact experimental conditions. The work of Lewin shows that all of the so-called laws of learning depend upon more than frequency-linkages of separate items in mechanical fashion. Before beginning a discussion of Lewin's two papers, I quote his general conclusions based upon experimental evidence: the law of association as hitherto conceived is untenable, since the effects demanded by it are absent even though the conditions called for by it are present; and the law is stated in altogether too general terms (1) in giving the conditions necessary for the formation of an association, and (2) in leaving out of account the conditions necessary for the effectiveness of the so-called reproductive tendencies when they actually function in reproduction.[51]

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[49] G. E. Müller, Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, *Z. f. Psych.*, Erg.-Bd. 8, 1913, 509.

[50] Wulf, *op. cit.*, note 36.

We must first refer to the work of Ach, which was repeated by Lewin. The preference for doing a certain thing Ach called the determining tendency. Thus the stimulus *a* may leave an after-effect determining how we react to *b*. It was thought that the strength of this determining tendency can be measured by the so-called "associative equivalent," which is obtained as follows: *O* associates two nonsense syllables a given number of times; he is then instructed to rhyme or invert when one appears. *O* may then either carry out the given instruction or may reproduce the syllable formerly associated with the one which appears; thus the strength of the various tendencies may be measured. A reaction in which the association succeeds in the face of the rhyming instruction is called an "intended failure reaction." The strength of the association which is just sufficient to bring about the intended failure is the associative equivalent. In other words, the strength of the association can be measured by the number of repetitions necessary to establish a connection that will bring about an intended failure reaction.

Two kinds of activities may be called forth by a stimulus or instruction after a series of syllables has been "learned." After two syllables have been associated, the instructions may require that another syllable be reproduced instead of the one associated; this awakens a "heterogeneous" activity which works against the associative activity. Heterogeneous activities are supposed to cause a reproductive inhibition (*Hemmung*) and to lengthen the reaction time. A so-called "homogeneous" activity is aroused when the instructions and the associative tendency established by the learning of the two syllables work in the same direction. Homogeneous activities are supposed to produce shorter reaction times.

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[51] K. Lewin, Das Problem der Willensmessung und das Grundgesetz der Assoziation, *Psych. Forsch.*, 1, 1922, 191-302; 2, 1922, 65-141. Ach's work will be found in his *Ueber der Willensakt und das Temperament*, 1910.

The *O*s were shown syllables and told to learn them. Two series of 16, two of 8, three of 4, and six of 2 were given. From these two series two similar sets were chosen. Every set was reported six days out of seven, so by the end of the fifteenth day every series had been repeated 270 times. Meaningful aids were not used in learning, although rhythm and complex building were not avoided. The *O*s preferred the series of 16 and 8 syllables because they could be put more easily into rhyme. A series of neutral syllables was used for comparison purposes. This series was memorized in haphazard order, the learning being done for the purpose of avoiding the effects of "strangeness" in a neutral set.

The tables show that, in spite of 270 repetitions and against the expectation of all, there was no heterogeneous activity, no determining tendency established for an association to persist; the reaction time for syllables learned together was approximately equal to that of the neutral syllables which had no associates. The *O*s repeated the associated syllables, to show that they knew them as well as before. The exceptions to this general statement were due to the fact that the *O*s tend to identify syllables, to see if they can be recognized, hence to put them into a structure. The identification process cannot be said to be the result of association, because it might occur with syllables which had not been associated. Under express instructions to the *O*s to be neutral, there was no difference in reaction-time between syllables which had been reproduced together and new syllables. The law of association, according to which an appearing associated syllable tends to call up its non-presented pair, as well as the concept of associative equivalents, falls to the ground.

The lengthening of time, reported by Ach and ascribed by him to the effect of determining tendency, was absent. Even when asked to repeat "the first thing that came into their heads" after seeing one of the associated syllables, the *O*s did not give the syllable which had been associated with it several hundred times. Where then is the binding effect of association pure and simple? Syllables rhymed with syllables beginning with gutterals were rhymed just as quickly with labials as were syllables practised with labials. With passive instructions, in no case did a single response give a rhyme learned 260 times with the stimulus. No tendency toward the reproduction of the second on the sight of the first was present!

It is beyond the scope of this article to enter into the refined experiments, the checks, and answers to objections cited in Lewin's paper; for all this the reader must go to the original, which is more convincing than any abstract can be. We have now to determine what, if any, are the real conditions under which so-called associations are established and become functional. To determine this Lewin tried to produce associations that would be effective in manifesting themselves as reproductive tendencies, retarding the heterogeneous activities, and accelerating the homogeneous activities. Eight series of syllables were used: 4 series were constantly rhymed, 4 were inverted constantly. They were exposed visually and read aloud, after which the *O* gave the response. The results in favor of "association" are now as startling as those previously against it: although the constantly inverted syllables had been repeated only 8 times, the first time they were called upon to rhyme them there was a failure reaction, and the second time they were asked there was considerable increase of the reaction time. And with only 11 repetitions of the constantly inverted syllables, the exposure of a constantly rhymed syllable led to an intended failure reaction and was not noticed as such. It was possible to get an intended failure reaction with only 4 associative repetitions. Hence the effects demanded by the laws of association were obtained: intended failure reactions, inhibition or facilitation of the heterogeneous activity and acceleration of the homogeneous activity, all appeared after a small number of repetitions.

The causes for the difference between the results of the first and the second paper were revealed by the introspective reports of the *O*s, which show that mere frequency does not suffice to explain the so-called "associative" effects. The instruction, attitude and motor response form a single configuration which must be analyzed separately. The instructions for a given reaction may be framed in divers ways and carried out in divers ways. It makes a difference whether an *O* is asked to "rhyme or invert" a given syllable or to "name the usual or earlier (rhymed or inverted) syllable which follows the given stimulus." The former instruction sets into operation a configurational pattern in which the end or goal releases and controls definite activities, while the second really leaves it to chance whether or not the associated syllable will be reproduced. *In this way the response can be controlled, and not by the frequency concept of the associationists.* The difference does not therefore consist, as the associationist would say, in the strength of associations, but in the fact that in the one case the *Aufgabe* is to *reproduce* in a certain way, in the other it is *to build a new syllable.* The number of associations after a few repetitions is of little account; rather, certain readinesses-to-respond in a certain direction determine what will be reproduced. These tendencies toward response need not be conscious; they may be related to the stimuli in very definite ways; and they form a relatively firm structure with the actual performance.

A comparison of neutral activities with definite ones also shows that association *per se* is not sufficient to bind together two contents. Admitting that it is impossible to put oneself in a wholly neutral attitude (which works also against the associationists!), it is possible to give neutral instructions and then to determine by introspection how far the *O* has been able to comply. In this way something approaching a neutral attitude may be appromixated; and the results show that the mere repetition of syllables sets up no reproductive tendencies whatsoever.

It appears, therefore, that out of the dross of the associationistic experiments some very important knowledge can be gained concerning recognition, memory, learning and reproduction, provided that we approach the facts with sufficient critical and constructive implements. Whenever the atomistic theories are taken seriously at their face value and without bolstering hypotheses, it is found that they do not suffice to explain the most elementary facts of perception, memory, or behavior. The supposed linkage of meaningless elements through mere repetition simply does not occur; there must be some structures already present in the organism which are ready to respond mechanically if nothing else is required of them. It is necessary to introduce long-section, functional concepts for the facts of insight, true learning and successful accomplishment; and this must be done with a minimum number of assumptions concerning the memory processes, the intellectual abilities, and the mental equipment of the human or animal subject. Only thus can a satisfactory approach be made to the problems of capacity and function, whether in perception, recognition, memory, or the higher thought processes.

(23) *Configurationism and the Higher Thought Processes*

One cannot approach the higher thought processes without bringing to mind the work of Watt, Marbe, Bühler, Messer and other members of the now defunct imageless-thought school. What at first promised to be a new mode of attack on the problem of thinking finally proved to be little more than the old association psychology supplemented by *Aufgabe*, determining tendencies, relations, and a new mental element joined in the usual fashion to the old. The potential *Wissen* of Bühler, Messer's predicative relation and transcendent meanings were all entities incapable of psychological demonstration or control. The use of unobservable entities and mechanisms in no way helped to support a theory which by its very nature must fail to explain orderly, rational thinking since it reduces all to the chance connection of meaningless units.

As Titchener and Bühler have pointed out,[52] the problem of the higher thought processes contains a number of subsidiary problems, many of them having little to do with one another. Judging, inferring, comparing, abstracting, willing, understanding, believing, valuing and appreciating have all been included under the rubric of thinking. It will therefore be necessary to restrict this discussion to what may be called logical thinking, thinking in which a certain goal or end is deliberately sought. We may put the problem thus: "How do we reach our goal in logical or creative thinking?" The typical answer of the associationists may be gleaned from Müller's constellation theory. According to this theory the goal is merely a constant in the chaos of reproductive elements and tendencies. The product of thought finally emerges from the reproductive tendencies because this particular item has been the most frequently associated in the sought-for connection. But on the basis of this theory it is impossible to explain how a new problem is solved at the first attempt, how a new product may be the result of thought, or why the significant term or relation which holds the key to the solution is seized upon.

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[52] E. B. Titchener, *Experimental Psychology of the Thought-Processes*, 1909; K. Bühler, Tatsachen und Probleme zu einer Psychologie der Denkvorgänge, *Arch. f. d. ges. Psych.*, 9, 1907, 297.

The logical analysis of the associationist is responsible for the plight he gets into; for, having begun with elements having no relation to one another, he must by some means connect them. The configurationists answer the question of thinking by asking how thought progresses from the beginning to the end of a problem. The historic antitheses of sense and intellect, sensation and thought, do not exist in configurational psychology; hence there is no need to build one on the other or to derive one from the other. Memory and thought saturate perception, and perception implies memory and thought. For thought to progress along logical lines, it must proceed within a structure, and this structure is very often furnished in perception. The relations of objects to one another are given in phenomenal configurations won in perception. A change in the object of perception may mean a change in the whole course of thinking, since rational insight demands the perception of the structure of the whole. Fruitful thinking consists, therefore, in approximating, either in perception or thought, to the *sachlicher Zusammenhang* of configurational structures. Configurational connections are not the results of frequent associations or blind juxtapositions, but are inner bonds imposed by the demands of the total structure. Once this structure is seen, the parts must follow one another in a certain way.

The concept of closure can be applied to thinking. Questions demanding thought arouse configurational processes which are incomplete and call for closure.[53] The answer thus "sticks" in the question. A good question and a good answer are not matters of chance: they fit each other as the key and the lock. Once the form suggested by the question is apprehended, there is a sudden *Einschnappen*; the inner bond appears, baring the structure, and the configuration is completed. It is this sort of process which is responsible for the sudden drop in the curves of animal learning,—a fact which trial and error, and frequency theories have never been able to explain.

It must not be thought, however, that all thought configurations are direct functions of perception. Experience leaves configurational dispositions which may be set into operation by part-situations. Again, the inner configurational conditions may change the organism so as to modify perceptual configurations and one's response to them. Thus puzzle pictures attain new perceptual properties and meanings when new inner configurational dispositions are aroused. To speak in such cases of mere associative-linkage is to neglect the specific properties by virtue of which the total situation is transformed both in thought and perception. The extent to which configurational transformations may affect our phenomenal and rational worlds is almost unlimited. The universal causality of the savage, compared with the restricted and refined concept of causality held by the modern man of science, amounts to a re-ordering of the whole perceptual, emotional and rational worlds of the latter in a way unknown to the former. The relations of facts within firm thought structures are based, not upon the savage's frequent associations and his (Humian) view of causality, but rather upon laws which modify and order the successive data of perception and thinking in the light of the structures to which they belong.

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[53] For a simple explanation in English of Köhler's concept of closure, see Koffka, *The Growth of the Mind*, 103.

The question still remains concerning creative thinking, or if there is an original product in thought. This problem (that of the petitio in the mode *Barbara*) has become a classic in logic, which asks: Does the conclusion of the syllogism involve anything new and not contained in the premises? Logicians generally answer both No and Yes. Logically there is nothing new, psychologically there is a new product. The logical problem has thus been transferred to psychology, which must explain how thought can tease something new out of existing propositions. Wertheimer has endeavored to give an answer in terms of configurational theory.[54] His solution may be summarized as follows: Creative thinking consists in the transposition of a member of one configuration to another. Thus members of two different configurations (the premises) are identified as belonging to a common (new) configuration (the conclusion). Membership within a new structure possessing its own peculiar properties sets the transposed members in a different light. Insight depends upon the ability to break up existing configurational structures and to make fruitful transpositions from one configuration to another. A concrete illustration may make this statement clearer. When in need of a tool for reaching food, the ape will use whatever objects are within his ken, putting food and stick or branch within the same configuration, the stick becoming a new phenomenal object once its tool-properties have been recognized. But if the stick or branch is part of a tree, *i.e.*, has not been separated from the tree of which it is a part, although the ape is familiar both with sticks as tools and branches as parts of trees, he will not tear the branch from the tree and use it as a stick-for-getting-food. Certainly the animal sees the branch, but the phenomenal field of which it is a part presents too firm a structure for the ape to break up. The existing configuration must be changed before transposition can take place and the branch be put into the tool-food structure. In other words, to see the branch on the tree as a stick-for-food is quite beyond the ape's visual and thought powers.[55]

Thinking necessitates not only the perception of a given structure but also the destruction, often, of old custom-bound configurations in order that new structures may become dominant. Many of the stupidities and "foolish" mistakes of students in the classroom and of adults without may be due to the inability to transpose from one situation to another or to see things as fitting and proper members of new configurations. The ability to transfer members of one configuration to another may be a definite structure-function which in pathological cases often degenerates and which probably never develops among those of inferior intelligence.[56]

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[54] M. Wertheimer, *Ueber Schlussprozesse im produktiven Denken*, Berlin, 1920.

[55] The resistance of configurations to change is a fundamental fact of "good" structures. Factors working for or against the preservation of structures must be further investigated, since it is not wholly clear even in the simple phenomena of movement.

Finally, the fact must be mentioned that thought structures are real in every sense of the word. Thinking logically is not the result of blind chance, of frequent associative connections, or a matter of caprice. For the results of thinking depend not only upon thought, says Wertheimer, but also upon the objects thought about. If all connections were matters of chance or caprice, objects could never mean anything definite. The inner necessity pervading the entire structure does not come from the mere relation of part to part, since the whole is more than a sum of parts in relation, but rather springs from the peculiar nature of the whole which is more or less autonomous. The course of ideas is therefore determined by configurational laws effective throughout the entire range of human thought.

(24) *Contributions to the Psychology of the Abnormal*

It has been found in studies made with pathological subjects that many grave defects in the ability to perceive, think, remember and act may be due to lesions which affect the configurational processes necessary for the normal functioning of the organism. The configurationists insist upon two fundamental propositions with regard to the principles upon which a psychology of the abnormal must proceed: (1) configurational laws govern the deviations from normal conditions, while, conversely, pathological states must be regarded as the results of a breakdown in the mechanisms responsible for configurational phenomena; (2) in cases of partial degeneration, normal and abnormal do not function summatively side by side, but rather new structures are formed which obey configurational laws and function "para-normally." Thus the normal and abnormal facts of human behavior must be deducible from a single set of assumptions linking both fields. Much light has been thrown upon the operation of normal configurational processes as a result of the investigations by Gelb and Goldstein, Fuchs, Benary and others in the field of the abnormal. But since the results are by no means capable of univocal interpretation, I leave a critical discussion of the issues raised concerning the very foundations of *Gestalttheorie* until the next paper, and shall discuss here only what seem to be the main contributions of the configurationists to the psychology of the abnormal.

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[56] *Cf.* the argument of the next Section, where the results of experimentation with pathological subjects are quoted. An application to the problems of mental testing and intelligence will be found in *Naive Physik* by O. Lipmann and H. Bogen, Leipzig, 1923.

Experiments with pathological subjects reveal gross defects in the ability to perceive, and to react to configurational situations.[57] Evidently the mechanisms responsible for the reactions to finer and more intricate structures cease to function, and the patient seems to have retained only a capacity for the simplest and most elementary (primeval) perceptual and motor configurations. One of the main factors in recognition appears to be form. If the form or structure of an object is not apprehended in perception, the object will not be recognized. If the patient cannot transpose the form either from past to present by means of memory, or from one sense modality to another, some of the apparently most simple functions may be lost: the patient cannot name or pick out common objects, fails to understand the significance of relational situations, and seem generally deficient in intelligence. One patient could not recognize forms by means of vision, owing to lesions in the visual areas of the brain, with the result that if he was to read or name a simple figure he was obliged to build the forms by means of head movements which follow the outline of the parts. But he could not make the simple transposition of form from one sense modality to the other: an object recognized by means of head movements (eyes cooperating) was not recognized when laid on the skin.[58] It appears, therefore, that the criterion of transposability, while not necessary, is an important criterion of configurational responses.

It has also been shown that pathological defects may involve only specific configurational responses and not the general intelligence of the patient. Indeed, some configurational mechanisms may remain intact, while others owing to the injury may function in an altogether new manner,—all of which would remain inexplicable on the basis of associational and atomistic theories. In this connection Benary has found even such mechanized responses as the operations involved in counting require configurational dispositions which may be disturbed. His subject could not begin to count in a series unless he began with one; he could tell that 1000 is greater than three, because the step between these two numbers is great, but when asked if seven is greater than three he had to pause before giving an answer. Every step in the ordinary arithmetical operations had to be painfully carried out and noted before he could proceed. He could not see that two times six and three times four, in giving twelve, had anything to do with each other. His ability to perceive identity and difference in different examples of the same kind was gone. He could not immediately perceive a total structure; usually it was a matter of inference from the parts.[59]

The patient cited above and other patients showed repeatedly the importance of structure for the processes of thinking, recognition, memory and action. If, owing to some pathological defect, the patient was unable to make use of a set of *Anhaltspunkte* by which he could orient himself, he set about searching for such support before attempting the real solution.[60] Without definite configurational structures the patient is lost, unable to perceive, think or respond clearly and adequately. Transformations of complicated configurations into simpler configurations of which the patient is capable are often found. Thus one patient, upon being asked to put all objects together which belonged together, made a collection based upon apparently no ordering principle. When he explained that he put all wooden objects in one pile and all iron objects in another, his response became intelligible to the experimenters.

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[57] The chief contributions to the psychology of the abnormal have come from Gelb and Goldstein and their co-workers. Their papers appear under the general title of the book quoted in note 21, *Psychologische Analysen hirnpathologischer Fälle.* In this series will be found papers not only by Gelb and Goldstein but also by Fuchs, Benary and others.

[58] This patient has been made the subject of intensive study and has been described in the book cited above and in a paper by W. Benary, Studien zur Untersuchung der Intelligenz bei einem Fall von Seelenblindheit, *Psych. Forsch.*, 2, 1922, 209-229.

[59] *Ibid.*

[60] See Ueber den Einfluss des vollständigen Verlustes des optischen Vorstellungsvermögens auf das taktile Erkennen, in *Psychologische Analysen.*

Where some normal functions are retained, the normal and abnormal do not function summatively or independently of each other. The normal and abnormal portions of the nervous system and receptor-effector organs show a mutual interdependence which may amount to a new structure-function.[61] This new structure-function will obey configurational laws of a more simple or a more complex nature, depending upon the particular facts involved. Fuchs, Gelb and Goldstein and others have found that objects exposed so as to fall within normal and hemianopic areas may be completed, seen as wholes (*totalisierende Gestaltauffassung*). The objects may be displaced toward the normal portion of the retina, or rivalry may appear between the normal and pathological areas with accompanying changes in the phenomenal data. Such facts as these, the configurationists assert, cannot be gleaned or explained by either a point-to-point investigation or a summative theory of vision.

One more important point remains to be mentioned regarding the configurational theories. If perceptual fields are important for normal behavior and cannot be omitted from a complete description of organic activities, a knowledge of the phenomenal world of the abnormal subject is even more important for the understanding of his behavior and of the defects underlying it. Gelb and Goldstein found that pathological subjects could hide gross defects in behavior which were revealed only after eliciting careful phenomenological reports on what the subject experienced. These investigators point out that there has been too much inference regarding the phenomenal world of the abnormal, with the result that the descriptions and the theories have suffered. In order to know how and why one often reacts as one does to a particular stimulus it is necessary, as Koffka points out, to know how the stimulus affects the individual. Certainly the behavior of the insane and of many borderline cases would be inexplicable did we not penetrate beneath the external phases of their behavior. "That a patient can react to a given object and is even able to give its right name does not yet prove that his perceptual processes are normal. Explicit behavior may cover gross defects which will appear only under the refined conditions of the tachistoscopic experiment."[62] Thus Gelb and Goldstein, whom I have just quoted, discovered that one of their patients could read only so long as he could make head movements. Phenomenal visual forms were entirely lacking. Only experiments designed to test the patient's phenomenological data and reports revealed the nature of his defects.[63]

(25) *General Implications of Gestalttheorie*

The fundamental concepts of any science have important implications for life and for philosophy. It has been impossible in these articles to enter even into the several fields of psychology and to point out what configurationism has there to offer, to say nothing of any thorough treatment of the fields outside psychology proper,—which would be beyond the competence of the present writer. The translation into English of Koffka's and Köhler's books has made a detailed discussion of comparative, genetic and educational psychology unnecessary. The implications of the configurational psychology for the various applied psychologies have already received the attention of those interested in mental testing, in intelligence, in group behavior, and the like.[64] I propose in this Section to review very briefly what seem to be the main points of contact between configurationism and the physical sciences and philosophy.

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[61] The evidence will be found in Fuchs' articles in *Psychologische Analysen* and in *Psych. Forsch.*, 4, 1923, 38ff.; 6, 1925, 187ff., from which the following facts are taken.

[62] Gelb and Goldstein, *op. cit.*, 7.

[63] D. Usnadze has proposed a configurational theory of paranoia which I omit since it does not readily fall within the outline of this Section. See Versuch einer Theorie der paranoischen Eigenbeziehung und Wahnbildung, *Psych. Forsch.*, 5, 1924, 1-23. This article also contains some interesting hypotheses for social psychology.

It is a fact worthy of comment that the most thorough single theoretical presentation of configurationism should bear the title of *Physische Gestalten*, should make an appeal to biologists and physicists as well as to psychologists, and should undertake to furnish a single set of assumptions for three different sciences. Such a work does not fall far short of being philosophical in its import (I wonder if philosophers are aware of its existence?), and cannot help but heal many breaches both within and without psychology. Certainly if physiologists and biologists accept the argument of the book, the behaviorist will be unable either to pride himself on possessing the only set of scientific assumptions for psychology or the only point of view in harmony with the "objective" sciences.

The new physical and physiological theories bring psychology and the "objective" sciences closer together at points where the former must needs lean upon the latter. The deduction of Weber's law by Köhler on the basis of physico-chemical principles, the derivation of the properties of the figure from the conditions obtaining in the physical fields, show how psychology may emerge from the slough of the private datum in a manner acceptable to all. Yet the psychologist can deal with phenomenal data without having resort to the circumlocutions and arbitrary decisions of the behaviorist. The insistence upon actual observation and experimentation by the configurationists must result in a righteous impatience with all the logicizing, philosophizing and hypostatizing now rife in our science. And if, as Planck is reported to have said, the real in science is the measureable, the configurationists have demonstrated anew that psychology is a real science, dealing with real phenomena, and possessing data subject to real laws.

But it is not psychology alone which may profit by the new point of view—physiology, biology and philosophy have ghosts of their own to lay. The two sciences are still haunted by spectres of atoms in meaningless conglomeration. Biology and biologists are divided on the problems of mechanism and vitalism, the inheritance of acquired characters, and other questions which might well be attacked from a fresh point of view. Koffka has given us new light on the question of nativism and empiricism, so that the problem, *e.g.*, of inherited or acquired eye-movements bears a more inviting aspect.[65] Similiar problems in the other sciences might be revivified and might acquire new significance if a more fruitful attitude could be taken toward them. Köhler has pointed out some of the implications of his theories to physiologists and biologists.[66] They are, briefly, as follows: Local processes may be influenced by the needs of the total organism; parts function with respect to, and in the economy of, the whole. The almost providential adjustment of organisms to environment does not require either vitalistic entelechy or blind mechanism, provided one admits the reality of dynamic processes extended in time and governed by definite laws. The conditions which affect the total organism must receive their due consideration just as the minute factors within it do. Certain concepts current in physics find fruitful application in the biological sciences (including psychology). Organisms tend toward equilibrium, usually represent minimum energy distribution systems, and possess several degrees of freedom,—these and other physical descriptions enable us to envisage total organic processes in scientific terms free from metaphysical bias.

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[64] *Cf.* F. Wagner, Das Problem der psychischen Strukturen, *Z. f. päd. Psych.*, 24, 1923, 193-208; Lipmann and Bogen, *Naive Physik*; R. M Ogden, The Need of Some New Conceptions in Educational Theory' *School and Society*, 18, 1923, 343ff.

When it comes to the implications for philosophy one must tread warily; for fundamental assumptions are always, in last analysis, based upon certain philosophical presuppositions. But I take heart in approaching this field because even philosophy itself must bow to experimental findings, and philosophers themselves are often not averse from accepting a pragmatic position with regard to certain problems. Strangely enough, the philosophical aspects of many of the configurational doctrines were noted before they were brought into the laboratory,—Meinong's psychology merges into logic, epistemology, and theory of value; Husserl and Dilthey, and more recently Driesch, have discussed the importance of total, organized wholes. At least certain aspects of *Gestalttheorie* (in their logical form) are not unknown to students of philosophy. Gelb went back to Plato in his discussion *Ueber Gestaltqualitäten.*

It is perhaps not too much to say that a good deal of the associationistic-atomistic psychology has been handed down from the *arm-chair* psychology of the English empiricists and those whom they influenced. The easy, common-sense analysis found in Locke, Berkeley and Hume has influenced the laboratory and the textbook of psychology far, far too much. The fault is not all to be laid at the door of the empiricists—Kant divorced matter and form and introduced the categories to get them together again! But the work of Sherrington on the integrative action of the nervous system, changed conceptions in physics and chemistry, and the latest developments in mathematics and modern experimental psychology all attest the fact that the philosophy of the past has not been altogether right in its scientific foundations. On the other hand, many of the sciences, and psychology among them, have much to offer philosophy if her world view is to be adequate to the newest discoveries. The configurational psychology has swept away the psychological foundations of several current theories of space and time and cognition, and some logical and ethical superstructures.

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[65] K. Koffka, *The Growth of the Mind.*, 1924.

[66] W. Köhler, Gestaltprobleme und Anfänge einer Gestalttheorie, *Jahresb. ü. d. ges. Physiologie*, 1922.

I can point out only one or two concrete examples of what is meant above. Reversing the Kantian position, the configurationists assert that forms or complexes are real, constitutive, exerting effects which can be predicted, controlled and measured. They can no longer be regarded as the result of special mental acts (or categories). The fact that phenomenal and physical configurations do not always coincide is not cause for attributing all forms to a mental creation or making the *reductio ad absurdum* of the idealistic logic,—solipsism. The number of possibilities for correspondence or lack of correspondence between phenomenal and physical structures is finite (just four), and we are beginning to know something of the laws governing them: a physical aggregate may be truly structured and arouse a phenomenal structure; physical structures may be paralleled by phenomenal fields lacking structuration; an unordered physical aggregate may be paralleled by either phenomenal structures or phenomenal fields lacking structuration. Correspondence depends upon a number of conditions both within and without the organism. In any case, whatever is found requires no special mental categories or acts whose locus is at present unknown to science.

Hume's problem does not exist for the configurationists. Indeed, one is tempted to believe that it was created by the assumptions with whichhe started, and that hence his logic has never been impugned. But the configurationists do not begin with the Humian assumptions or the psychology upon which many of them are founded. There are no absolutely unordered data in perception which must needs be associated in order to bring lawful, meaningful connections out of chaos. Where did Hume find these isolated bits of experience which for him constituted the world? The Humian psychology and philosophy lead to the nihilistic position that there are no real phenomenal or physical systems or structures. Observation disproves Hume's assertions: associative and productive processes as he conceived them do not form the basis of the world order. It is, of course, admitted that in no case do structures arise without necessary and sufficient conditions. On the other hand, the opposed view that all events are so intimately related that no division can be made within the universe is equally untenable; for science is interested in part-systems which can be isolated for purposes of measurement and control. An universal causality is as unfruitful as the Humian denial of any causality.[67]

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[67] An excellent discussion of this problem of isolation in science will be found in *Physische Gestalten,* ch. IV. on the problem of independent systems.

There still remain the laws of science. Wertheimer would say that they more or less embody real connections which are not arbitrary and which are founded upon *sachliche Zusammenhänge*. The quantitative laws and formulations of science cannot be juggled in any way one pleases and still remain true to the facts. Once the equation of a curve is given, every point in it is lawfully determined, and interpolation and extrapolation must follow the law of the whole if it is to be preserved. We may change our units of measurement, the mathematics we apply to our data, but this is not a matter of caprice on our part; for, after all, what we are trying to do is to find out "how the dice are loaded." True, no theory absolutely encompasses the facts, and hence the best we can do is to get approximations, —but there are bad, good and better approximations, and the progress of science consists in formulating better approximations on the supposition that there is something to approximate.

So configurational systems, wherever found, have their laws. No stark mechanism, pre-established harmony, vitalistic principles or mental impositions are necessary to explain these structures, whether found in the phenomenal or physical domains. The whole possesses an autonomy of its own which is nevertheless subject to definite laws. Mind is thus not set over against the external world, but rather finds its place in the world. It is possible, therefore, to have a monistic science without throwing the baby out with the bathwater as the behaviorists have done, or accepting the extreme idealistic position which makes of mind an unique phenomenon.

THE VISUAL APPREHENSION OF MOVEMENT UNDER SUCCESSIVE RETINAL EXCITATIONS[1]

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I. INTRODUCTION

A. *Historical setting.* Within the past decade psychology has witnessed the development of the doctrine embodied in the *Gestalt-Theorie.* The immediate origin of the doctrine and likewise the occasion for many of the experimental studies dealing with it was Wertheimer's *Ueber das Sehen von Bewegung* (**49**). Since the appearance of this study most of the publications upon the *Gestalt* have attempted either amplification or substantiation of Wertheimer. The contribution of Wertheimer himself to *Gestalt* and to the literature of visual movement lies not so much in his descriptive expositions as in his physiological assumptions regarding the cerebral explanation of the experience of movement.

The perception of movement is the most investigated and the most important, at least from a methodological standpoint, of all the perceptual *Gestalten.* It has furnished most of the experimental support of the theory in the writings of Wertheimer (**49**), Koffka (**20, 21, 22, 24**), Kenkel (**16**), Korte (**26**), Lindemann (**28**), Cermak (**6**), Stern (**22**), and others. We shall duly consider these and other studies in so far as they are relevant to our own investigation.

To Stampfer (**43**) must be attributed the construction (1832) of the first stroboscope and the first artificial production of movement independent of the actual displacement of a physical object. His apparatus, which he called a "stroboscopic disc", was quite simple. Upon one side of this disc an object was drawn in various successive positions. When viewed in a mirror through radial slits in the periphery of the whirling disc this object was observed as in continuous natural motion. Almost simultaneous with, but totally independent of, Stampfer, Plateau (**37**) constructed a similar apparatus which he variously called the "Phänakistoskop," "Phantasmaskop" and "Phantaskop." By means of it Plateau was able to produce illusory movements of the same character as those produced by Stampfer's disc. An instrument more significant for experimental purposes was constructed by Horner (1834) and named the "Daedaleum" (**15**). A hollow rotating cylinder with vertical slits was mounted on an upright axis. Representations of animals in various postural sequences were placed inside around the walls of the cylinder and below the slits in such a way that when viewed through the slits and under suitable rotational speeds they were no longer apprehended as independent objects but were seen as momentary phases of a single object in motion. In 1846 Plateau invented his "Anorthoscope," which consisted of two discs, one slightly in front of, and parallel to, the other. The anterior disc was opaque with open radial sectors, and the other, a transparent disc, bore upon it representations of various objects. Both discs rotated about a common axis, which was so controlled as to allow direction and rate of rotation of the posterior disc to vary independently of the one in front. In this manner Plateau produced stroboscopic movement in connection with the figures outlined on the second card. During the following years a large number of devices of various types were built for the purpose of producing apparent movement under successive retinal stimulations. Most of these early devices were not primarily constructed for scientific uses. Thus neither careful control of objective conditions nor accurate description of the movement-experience appears from their employment. Furthermore, no elaborate explanations are advanced. Most of the investigators rest content with a statement of the immediate physical conditions under which the 'illusory' motion appeared. It was not until Exner became interested in the problem of visual movement that we find a serious attempt at control and analysis.

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[1] The author makes his acknowledgments to Professor Bentley under whose guidance the research was carried out.

Exner (9) was early impressed by the apparently simple and irreducible character of visual movement, and he marshalled as proof of its elementary character a large amount of factual material. Along with the fact of the negative after-image, he pointed out that the minimal temporal interval necessary for the apprehension of movement is less than that which is required for the perceived succession of two successive lights. The shortest time for the apprehension of two lights in sequence he found to be about 45 σ. When the two, however, were the beginning and the end of a movement, real or apparent, the temporal interval might fall as 14 σ (**10**). The fact that an object when moving may be apprehended when the distance travelled has less extent than that which is necessary to perceive as discrete two stationary objects was also demonstrated by Exner. A much more important contribution and one later verified and cited by Wertheimer (**49**) as testimony to his physiological theory was the obtaining of movement through the rapid successive stimulation of the two eyes separately. This fact, it is alleged, substantiates the theory of central conditions as over against peripheral or retinal. We have thus the important statement from Exner that the immediate neurological correlate of the movement sensation is a specific physiological process which runs its course in a hypothetical movement-center in the central nervous system. Following Exner[2] Vierordt believed that visual movement was purely sensational in character. "Ich habe . . . . die Essentialität der Bewegungsempfindungen nachzuweisen gesucht und werde in den nachfolgenden Erörterungen den

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[2] For a general discussion of Exner's position in regard to visual movement, see Lasersohn (**27**) and Wohlgemuth (**53**).

Beweis anstreben, dass die Raumsinnsorgane (Cutis und Auge) uns von den bewegten Objekten immer nur Bewegungsempfindungen verschaffen" (**47**, 227). Aubert also could find no other way out. He differed, however, from Exner in his emphasis upon peripheral conditions. "Die Bestimmung, welcher Raum auf der Netzhaut in 1 Sek. durchlaufen werden muss, wenn das Objekt bewegt erscheint. . . . gestattet nun weiter eine Berechnung der Zeit, welche mindestens zwischen zwei Erregungen verschiedener Netzhautelemente vorgehen muss, damit wir eine Bewegung empfinden. Bei einer Winkelgeschwindigkeit von 1′ ist der in einer Sekunde zurückgelegte Weg auf der Netzhaut = 0.00436 mm = 4.36 μ, und da der Durchmesser eines Zapfens in der Fovea, 0.6 μ (= 10″) beträgt, so würden in 1 Sek. etwa 7 Zapfenspitzen in Zeitintervallen von 1/7 Sek. erregt werden. In früheren Beobachtungen hatte ich die Grösse eines Empfindungskreises der Fovea zu etwa 45 Sek. bestimmt: es würden also innerhalb einer Sekunde etwas mehr, als ein Empfindungskreis passirt werden müssen, um die Empfindung einer Bewegung auszulösen, in dieser Zeit aber eine Anzahl von 7 Zapfenspitzen in unmittelbarer Aufeinanderfolge erregt werden. Die Zeit von 1/7 Sekunde für ein anatomisches Netzhautelement, von etwa 2/3 bis 1 Sekunde für einen Empfindungskreis der Netzhaut ist nach anderweitigen Erfahrungen zu gross, um eine Verschmelzung der Bilder, zu klein, um ein vollständige Discontinuität der einzelnen Erregungen eintreten zu lassen—es bleibt eine Beziehung zwischen dem zur Zeit wirkenden und dem 1/7 Sek. vorher dagewesenen Reizzustand bestehen, und der Ausdruck dieser Beziehung ist eine Empfindung *sui generis*, die *Bewegungsempfindung*" (**2**, 357-358). In the same year with Aubert, Fischer (**11**) published.[3] The appearance of this article precipitated a discussion concerning the true character of the phenomenon and its correct explanation which has been continued to the present time, although with much less emphasis in recent years. Fischer, believing the after-image essential to stroboscopic movement, states that, "um die Bewegung eines Gegenstandes künstlich nachzuahmen, ist nothwendig und hinreichend, dass man in kurzer Aufeinanderfolge von verschiedenen, nahezu in gleichen, nicht zu grössen Abständen liegenden Phasen der Bewegung desselben kurze Lichteindrücke bekommt, und dass das Auge in der Zwischenzeit durch keine anderen Lichtreize afficirt wird" (**11**, 149). He thought he proved that there was an optimal time for the clearest apprehension of the apparent movement of an object from one position to another; but he pointed out that, once movement was seen, it tended to be perceived under conditions which would otherwise give no movement. When the time approaches the minimal or maximal, due to an increase or decrease in rotational rate the movement obtained under optimal time disappears. Here we find (1886) one of the earliest statements concerning the relation between the optimal, minimal and maximal temporal intervals and the experience of visual movement.

A clever experiment in visual movement and its dependence upon the number of phases and the temporal interval between them was conducted by Grützner (**12**). In character it differs little from some of the more recent investigations. His demonstration that "das Auge etwas sieht, was es strenge genommen, nicht sehen kann" was very simple. He covered certain phases of a serial and stroboscopic representation of one boy leaping over the back of another, so that the observer had given only the "Absprung und den Niedersprung" yet one clearly saw that "der eine Knabe über den anderen hinüberfliegt" (p. 519). "Man sieht ihn auf das deutlichste hinüberfliegen. Das geistige Auge ergänzt also eine Bewegung, von der das leibliche Auge nur den Anfang und das Ende sieht, zu einer vollkommenen Bewegung." Grützner's results were carefully investigated by Dürr (**8**), who found that the movement-experience might be gradually destroyed by progressive elimination of successive phasal postures, so long as no corresponding change in rate of presentation was made; that is to say, so long as the time between the objects remained the same. By decreasing the temporal interval he was able to remove many more intervening phases than was possible under a slower sequence.[4] Prior to Dürr's study, Marbe had published his results on the "stroboskopischen Erscheinungen" (34). Here we are told that "Auf grund unseres Experimentes mit den Bildern des Reiters bin ich nun zur Ansicht gelangt, dass der Ausfall[5] von Bewegungsphasen in Folge centraler Ursachen unbemerkt bleibt. Das Experiment mit dem Reiter gab uns ein Beispiel dafür, dass eine Reihe von Bewegungsphasen unter Umständen ohne Nachtheil ausfallen kann" (34, 399). In Marbe and Dürr, then, we find a partial return to the physiology of the central nervous system as an explanatory basis for the visual apprehension of movement. Only 'partial', however, inasmuch as ocular activities also play a part in their theories; namely, muscular functions, *Anklingen* and *Abklingen* of retinal processes, and the fusion of retinal excitations. An analogous case may be cited in the visual quality objectively conditioned by the rapid rotation of a disc composed of two differently colored sectors.[6] It is well known, Dürr says, "dass successiv und periodisch wirkende Gesichtsreize unter Umständen eine constante Empfindung erzeugen können" (8, 501). From Marbe we have the statement that the stroboscopic phenomenon arises when the eye is presented with successive single phases, which are integrated under the influence of the central nervous system. Linke observes that "Marbe hält die 'Verschmelzung' für die eigentliche Grundbedingung, eine *conditio sine qua non*, der stroboskopischen Erscheinungen" (31, 450). In the work of Stern (44) we find a recognition of the perceptual nature of the movement-experience which depends, as he thinks, upon three factors: modification in stimulus, the flow of after-images, and eye-movements. All appear to be necessary for the perception of visual movement; but they may exist in different patterns. Stern refers to the perception of a particular movement which closely resembles the '*gamma*-movement' named and experimentally investigated much later by Kenkel. "Jede Erhellung erscheint als eine Vergrösserung, jede Verdunkelung als eine Verkleinerung des erleuchteten Objektes. Freilich gehen ja Gestalts- und Helligkeitsveränderungen oft Hand und Hand; aber selbst da, wo jede objektive Grössenveränderung des hellen Gegenstandes absolut ausgeschlossen ist, hat man oft den unverkennbaren Eindruck, als 'rühre sich etwas im Sehfeld' " (44, 329).

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[3] Fischer used a modification of Stampfer's and Horner's instruments.

In the experiments of Linke we find a series of investigations which have considerable historical significance for the *phi-phenomenon.* Linke successively presented (by means of a modification of Horner's stroboscope) two opposed right angles in such a fashion that two vertical lines, for example, coincided and two horizontal lines extended in opposite directions. Under these conditions, together with a proper rate of presentation, there was an obvious turning[7] movement of the horizontal lines around the vertical 'axis'. He also presented a vertical line followed by a horizontal with the result of an "Umklappen" of the vertical toward the horizontal

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[4] "Ein ziemlich grosser Abschnitt einer stroboskopischen Bewegung kann ausfallen, ohne dass die Unterbrechung bemerkt wird, wenn nur die Dauer der Unterbrechung eine hinreichend kurze ist" (p. 512), a fact to be attributed to the "Dauer der Netzhauterregung und der Thatsache der Augenbewegungen."

[5] "Die stroboskopische Bewegungserscheinungen beruhen ausserdem auf der Tatsache dass wir in Folge rein centraler Verhältnisse den Ausfall von Bewegungsphasen nicht bemerken."

[6] "Diejenigen stroboskopischem Erscheinungen bei welchen es sich nicht um das Sehen bewegter Bilder handelt, beruhen im wesentlichen lediglich auf den Thatsachen des, 'Talbotschen Gesetzes' " (34, 398).

[7] "Am meisten dürfte aber folgendes für die Beteiligung zentraler Faktoren sprechen: die Richtung der Drehung wechselte in einer von objektiven Bedingungen gänzlich unabhängigen Weise: oft sogar während eines und desselben Versuches. Die 'Windmühlenflügel' schienen sich bald im Sinne des Uhrzeigers zu drehen, bald entgegengesetzt" (29, 217).

(**31**, 526). "Diese Drehbewegung wurde aber nicht erschlossen, sondern—das ist das Wichtigste—unmittelbar 'gesehen'. Dieses 'Sehen' eines objektiv nicht gegebenen Tatbestandes wird sich am ungezwungensten als Assimilations-prozess auffassen lassen . . . . Waren überhaupt nur zwei Phasen geboten, zwei aufeinander senkrechte Kreisdurchmesser, abwechselnd als liegendes und als stehendes Kreuz, auch hier sah man Bewegung. Die Durchmesser schienen wie Windmühlenflügel um ihren Schnittpunkt zu rotieren" (**29**, 217). Linke stresses central conditions, since attention and identification must, of necessity, be matters resting primarily upon cortical, rather than peripheral, functions. His position is made clear in the following quotation. In order for a movement to be apprehended "ist zunächst nötig, dass mindestens zwei Gesichtswahrnehmungen nacheinander bestehen, die in ihren räumlichen Bestimmungen wenig genug voneinander abweichen, um identifiziert, d. h. auf einen einzigen Gegenstand bezogen werden zu können. Zweitens aber muss diese Identität oder Einheit unmittelbar erlebt werden, und dazu ist nötig, dass die beiden fraglichen Wahrnehmungen rasch genug aufeinander folgen, um als ein einziges, einheitliches Ganze im Bewusstsein zu wirken" (**31**, 544). Unable to agree that visual movement is either a sensation or a perception, Stratton (**45**, **46**) discovers in the experience something that is neither. "The perception of a motion seems to be nothing more or less than the perception that a sensation is changing its space relations, the motion itself furnishing a decidedly favorable, but by no means unique, set of conditions for appreciating such differences of space relations. This does not imply . . . . a deliberate comparison of positions; for the discrimination often occurs at a single psychic stroke," which, however, is very complex. There is a "relational activity of the mind which interprets and gives character . . . . to the incoming sensations, so that they . . . . mean movement" (**46**, 443). It appears that "mind becomes accustomed to this manner of interpreting all rapid change" (referring to motion); "it interprets it instinctively as motion" (**45**, 288). "Where no physical 'thing' is caught" we might call the consciousness of motion, he tells us, "infra-percepts or aesthamorphs" (**45**, 292). Visual movement lies between sensation and perception, for "consciousness of motion or rapid succession is not a mere sensation, nor is it a mere judgment, nor again is it a perception. . . . It is rather a sensation or a group of sensations consciously organized and yet not organized into anything so concrete as a thing or substantial object" (**45**, 293). Stratton's labored phrasing here reminds us of Wertheimer's contention that "Es ergab sich in vielfacher Beobachtung immer wieder dieses Charakteristische: klare, zwingende Bewegung um 90° war da, das spezifische 'Hinüber' ist klar zu beobachten; . . . . nicht als Hinüber des Streifens; bloss ein 'Hinüber', eine 'Drehung' " (**49**, 224), and again of Kenkel's statement that "hat der Beobachter einen deutlichen Bewegungseindruck, ohne ein sich bewegendes Objekt zu erblicken, so nennt man dies reine Bewegungserscheinung" (**16**, 362).

Much of the setting for the *Gestalt-Theorie* must be indirectly attributed to Schumann who, in 1902, studied the apprehension of movement in connection with the Müller-Lyer illusion. Long before Wertheimer, Schumann, presenting simple geometrical figures in his tachistoscope, obtained clear movement of the first object from its position over toward the position of the second. In 1907, when referring to Linke's work, he intimates that Linke did not realize that the "interessante Tatsache, dass schon zwei Phasen genügen, um den Bewegungseindruck zu geben, ist nicht neu." "Sehr schön," he adds, "kann man sie konstatieren . . . . wenn man, z. B. zuerst den senkrechten Balken eines Kreuzes für einem Moment exponiert und unmittelbar darauf den horizontalen; dann tritt schön der Eindruck einer Drehung des vertikalen Balkens auf" (**42**, 218). At times, however, the 'object' seems to 'drop out' and there is only an "Etwas" which "sich hinüber bewegt." By way of explanation Schumann says "Wenn ich von einem sinnlichen 'Etwas' sprach, das bei stroboskopischen Versuchen zu den Wahrnehmungs-bildern hinzukommt, so habe ich damit nicht einen Bewusstseinsinhalt gemeint, der durch Prozesse in der Netzhaut hervorgerufen wird . . . . . nur ein zentraler Vorgang in Frage kommen kann. Er liess von zwei rasch aufeinanderfolgenden, räumlich benachbarten Gesichtseindrücken jeden nur auf ein Auge einwirken, und doch war der deutliche Eindruck der Bewegung da" (41, 183). Lasersohn, too, reports a relevant experiment carried out in Schumann's laboratory. "Wenn nämlich zuerst ein kleinerer und unmittelbar darauf ein grösserer konzentrischer Kreis exponiert wird, so kann man noch dem inneren Erlebnis dadurch Ausdruck geben, dass man sagt: 'Es schien ein kleinerer Kreis sich zu einem grösseren auszuweiten" (27, 121).

We have tried to indicate the more significent aspects of the various contributions, experimental and non-experimental, to the literature on the experience of visual movement prior to Wertheimer's investigation. We have traced the development of objective conditions from the simple whirling card with its mirrored pictures to the highly complicated apparatus of the modern psychological laboratory. We turn now to a brief consideration of Wertheimer's study.

With the work previously done at Frankfort behind him and with Schumann's experience as a guide, Wertheimer set about his task. He took as his principal objects two lines set parallel or in an angle. Many variations in the size, color and position of these figures were made to determine their effect upon the character of the movement-experience (49, 262-265). The objects were presented to 3 *Os*[8] in a definite temporal sequence, by means of the Schumann tachistoscope. The temporal interval proved—as Wertheimer anticipated—to be of great significance for the determination of a certain type of movement. Concerning the importance of this temporal interval for stroboscopic movement, Wertheimer writes: "Man zeichne . . . zwei Objekte einfacher Art. Bei relativ sehr langsamer Rotation . . . . erscheint zuerst die eine Horizontale, dann die andere; die beiden treten klar sukzessiv und dualiter auf. Bei sehr schneller Rotation sieht man sie simultan übereinander; sie sind gleichzeitig, zusammen da. Bei einer mittleren Geschwindigkeit sieht man bestimmte Bewegung: ein Strich bewegt sich klar und deutlich von einer oberen Lage in eine untere und züruck" (p. 165). The spatial as well as the temporal relations of the two objects appear thus to depend upon the length of the pause between the two exposition periods. "Im extremen Simultanstadium sind sie zusammen gegeben, man sieht einen Winkel. Im Bewegungsstadium, zwischen den bieden Extremstadien, dreht sich eine Linie aus der Schräglage in Horizontallage und umgekehrt" (p. 165). Kenkel has given us a more concise statement. "Wenn ich wenigstens zwei Objekte in richtigem Zeitabstande und in verschiedenen Lagen dem Auge darbiete . . . das erste Objekt bleibt eine Zeitlang sichtbar, aber nicht in Ruhe, sondern geht in die Lage des zweiten über . . . . Sukzessivstadium, in dem beide Gegenstände nacheinander in Ruhe erscheinen, entsteht bei längerer Pause zwischen der Darbietung der beiden Objekte. Ist die Aufeinanderfolge sehr rasch, so erscheinen beide Gegenstände gleichzeitig in Ruhe, höchstens mit etwas Innenbewegung im Objekte selber; es ist dies das Simultanstadium, dazwischen liegt das Optimalstadium, das gute sinnliche Bewegung gibt . . . . Biete ich, z. B., tachistoskopisch zwei Linien in gleicher Lage nacheinander dar, die in ihrer Richtung einander decken, aber verscheidener Länge sind, so sehe ich im Optimalstadium nur eine Linie, die länger oder kürzer wird, je nachdem, ob die zweite Linie länger oder kürzer ist" (**16**, 361ff.). With regard to the exact length of the temporal interval which is said to condition these three stages, simultaneous, optimal and successive, we learn that "Unter den gegebenen Umständen führten die Expositionen in der Regel zu optimalen Bewegungseindrücken bei einer Grösse der Zwischenzeit von ca. $60\ \sigma$; ruhige Simultaneität erschein bei $t=30\ \sigma$; ruhige Sukzession in der Gegend von $t=200\ \sigma$" (**49**, 178). We see, then, that for Wertheimer the pause is of very great significance. It is, then, with surprise that one reads from Hillebrand that optimal movement may be obtained when $t=0$. "Wertheimer meint, das Optimum der stroboskopischen Bewegung finde bei einer gewissen mittleren Grösse der Zwischenpause statt; sei diese zu klein ($30\ \sigma$) so sehe man die beiden Punkte A und B streng gleichzeitig: sei sie zu gross, so sehe man sie hintereinander, aber ohne vermittelnde Bewegung. Das letztere ist nun richtig und wir haben den Grund bereits erkannt. Das Erstere ist aber durchaus falsch; selbst bei $t=0$ kann man die schönste stroboskopische 'Ganzbewegung' erzielen" (**13**, 243). Hillebrand is emphatic "dass man selbst ohne Zwischenpause Ganzbewegung erzeugen kann" (p. 244).

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[8] The only formal instructions recorded were "Fixieren Sie den Scheitel, aber achten Sie besonders darauf, was an dem Orte des oberen Endes der schrägen Linie da ist, geschieht, vor sich geht, so dass Sie darüber genau berichten können" and "konzentrieren Sie Ihre Aufmerksamkeit dauernd fest an dieser Stelle" (49, 204).

We have spoken of 'types' of movement, thus indicating that movement is, for Wertheimer, not always of one sort; that it may assume different forms under different conditions. He describes several specific kinds.[9] In one type, only one of the objects moves.[10] He contends that slight quantitative changes in the temporal sequence may result in a shift of type. These shifts bring out the following forms. Full-movement, in which a figure appears to move all the way from its position to the position occupied by a second object; part-movement, in which one object moves back and forth within a more or less restricted area while the other object remains at rest; dual-movement in which each figure is apprehended in motion over an area limited to the space immediately surrounding it; and finally movement within-a-given figure. The experience of movement between two objects, where the movement appears after the presentation of the second figure but is usually conjoined with the first and where nothing in the object corresponds to the experience of movement, Wertheimer called the *phi*-phenomenon. It may occur either in definite conjunction with a specific object or it may exist independent of any particular 'object' meaning. In this last case it may be spoken of as the pure *phi*-phenomenon.[11]

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[9] Linke claims priority in the descriptive account of these movements. He intimates that Wertheimer has only changed the terminology. "Meinen 'unmittelbaren Bewegungseindruck' nannte Wertheimer das *Phi*-Phänomen, meinen 'stroboskopischen Effekt' das Optimal stadium, meinen 'thaumatropischen Effekt' das Simultan-, meinen 'Ruheeffekt' das Sukzessivstadium" (**30**, 262).

[10] Wertheimer criticizes all theories which make movement involve both, since one figure may be at rest while another is moving, or both may move independently. Thus movement has to be something introduced, so to speak, into the apprehension of a particular spatial pattern.

[11] "Gegeben sind sukzessiv zwei Objekte als Reize; diese werden empfunden; zuerst wird *a* gesehen, zuletzt *b*; zwischen ihnen war die 'Bewegung von *a* nach *b* gesehen;' ohne dass die entsprechende Bewegung resp. die raum-zeit-kontinuierlichen Zwischenlagen zwischen *a* und *b* wirklich als Reize exponiert gewesen wären. Der psychische Sachverhalt sei—ohne irgendeine Präjudiz—mit *a Phi b* bezeichnet; *Phi* bezeichnet, was ausser der Wahrnehmung von *a* und *b* da ist, was zwischen *a* und *b*, in den Zwischenraum zwischen *a* und *b* vor sich geht; was zu *a* und *b* hinzukommt . . . . *Phi* ist etwas, *a* und *b* einheitlich betreffendes, sich auf ihnen aufbauendes, sie beide fassendes und verbindendes. Der phänomenale Inhalt von *Phi* ist durch subjektive Ergänzung (oder auf Grund subjektiver Ergänzung) der objektiv nicht vorhandenen raum- zeit-kontinuierlichen Zwischenlagen gegeben. Man müsste demnach sagen: *Phi* ist eine Angelegenheit, die schlechthin *a* und *b*, und zwar einheitlich betrifft, einheitlich verbindet; und *a* und *b* müssen als notwendige, als die irgend fundierenden, tragenden Inhalte für *Phi* gedacht werden; endlich: *Phi* entsteht, indem zwischen den Lagen *a* und *b* die Zwischenlagen subjektiv ergänzt werden" (**49**, 185f). Discussing the pure *phi*-phenomenon, he asks "Was ist psychisch im Bewegungsfelde gegeben? Die . . . . These sagte (p. 221): die Zwischenlagen des Objekts werden subjektiv ergänzt . . . . das Wesentliche des Hinübergehens resp. des Drehens nichts mit sub-

We obtain a further clue to this "grin without the cat" when, referring to pure *phi*, Wertheimer says: "Dieses *Phi*-Phänomen. . . . war etliche Male so stark da, dass der Beobachter nichts über die Objekte selbst angeben konnte; 'was für Objekte da waren, ist nicht zu sagen; ich habe eine starke Bewegung gesehen, ... von Objekten weiss ich nichts, weiss nichts von Objekten gesehen zu haben' " (49, 225). In the writings of Koffka, too, we find emphasis laid upon the unitariness of *phi*. Under proper experimental conditions "das Erlebnis der einheitlichen Bewegung ist ebenso gesetzmässig und unvermittelt gebunden wie an die Wirkung eines einzigen Reizes die ihm entsprechende Empfindung. Die einzeln nacheinander dargebotenen Reize sind, ... lediglich Reize für das Bewegungserlebnis, nicht es fundierende Inhalte" (21, 354). Koffka seems here to assume that, where two spatially distinct objects are presented with a temporal interval, the temporal and spatial phases of the peripheral excitatory processes bear a one-to-one relation to the temporal and spatial aspects of the physical object;—"die einzelnen Teile einer objektiv gegebenen Figur stehen zum Erlebnis der Gesamtgestalt in direktem Reizverhältnis." We must remember, however, that the visual field under normal conditions is always a totality. The retina is being constantly stimulated. Any shift in the retinal pattern must, of necessity, involve the whole retinal outspread. The various parts of the retina do not functionate independently; rather do the facts indicate a functional integrity of the whole. Any change in the energy relations at one part will be reflected in all other parts by a change in functional activity.[12] To explain the apprehension of movement where there is no physical movement Wertheimer assumes a "short-circuiting" between two cortical regions. The peripheral conditions are of minor importance; instead it is "bestimmte, zentrale Vorgänge, physiologische 'Querfunktionen' besonderer Art, die als das physiologische Korrelat der *Phi*-phänomene dienen." More specifically stated "Wird die Stelle a gereizt in bestimmt kurzer Zeit nachher die nahe Stelle b, so träte eine Art physiologischen Kurzschlusses von a nach b ein: in dem Abstand zwischen beiden Stellen finde ein spezifisches Hinüber von Erregung statt; ist der Grad der Umkreiswirkung von a z. B. an dem Höhepunkt seiner zeitlichen Verlaufskurve angelangt und bietet sich nun Umkreiswirkung von b, so flute Erregung hinüber, ein physiologisch spezifischer Vorgang, dessen Richtung dadurch gegeben ist, dass a und die Umkreiswirkung um a zuerst da ist. Je näher die beiden Stellen a, b einander sind, desto günstiger die Bedingungen für ein Entstehen des *Phi*-Vorgangs. Ist t, die Zwischenzeit zwischen dem Eintritt der Erregung in die beiden sukzessiv gereizten Stellen a und b, zu gross, so ist die Umkreiswirkung um a schon erloschen, wenn die von b eintritt; ist die Zwischenzeit kürzer, so, das die Umkreiswirkung von a da ist, ev. am Höhepunkt ihrer Verlaufskurve sich befindet, wenn die von b eintritt, so ergibt sich das Hinüber von Erregung; ist t sehr kurz, so treten die Umkreiswirkung von a und b zu gleichzeitig auf, um den gerichteten Kurzschluss zu ermöglichen" (49, 247 f).

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jektiven Zwischenlagen zu tun hat: Es gibt Fälle, wo *Phi*, die Hinüberbewegung, die Drehung, klar und deutlich gegeben ist, ohne dass im Bewegungsfelde der Striche irgendwie vorhanden war; die Anfangs- und die Endlage war da, zwischen ihnen die Bewegung, aber im Bewegungsfelde nicht optische Ergänzung, Sehen oder Vorstellen der Zwischenlagen des Striches. Bei allen Beobachtern ergab sich dieses spontan; in Versuchsanordnung konnte dieses 'reine' *Phi* ohne Ergänzung von Zwischenlagen in einfacher Weise demonstriert werden" (p. 222). "Diese Fälle zeigten sich so, dass auch nicht etwa der Gedanke vorhanden war: ein Objekt habe sich hinüberbewegt; was von Objekten vorhanden war, war in den zwei Lagen gegeben; nicht eines oder eines von ihnen oder ein ähnliches betraf die Bewegung; sondern zwischen ihnen war Bewegung gegeben; nicht eine Objektbewegung . . . es war einfach Bewegung da; nicht auf ein Objekt bezüglich . . . in diesem Felde, wo nichts von den sonstigen optischen Qualitäten zu sehen war (ausser dem blanken Grunde)" (p. 223).

[12] It is claimed that the "subjective" visual field is never homogeneous even under conditions where the objective conditions are everywhere the same (22).

Hillebrand comments caustically upon Wertheimer's cortical theory. "An den Namen Wertheimers knüpft sich eine pseudo-physiologische Theorie (man nennt sie 'Kurzschlusstheorie'), die sich schon als unhaltbar erweist, wenn man ihre verschwommene Fassung durch eine klare ersetzen will. Sie ist tatsächlich nichts anderes als eine Übersetzung des zu Erklärenden in eine noch dazu mangelhafte physiologische Terminologie." A little later he speaks of "hypothetische Vorgänge, die *ad hoc* konstruiert werden, wie dies bei der Kurzschlusstheorie der Fall ist" (**13**, 212f). Watt (**48**) is only less severe. As he sees it Wertheimer formulates a physiological hypothesis which "makes no contribution to psychological theory" (p. 26). Koffka, in complete sympathy with Wertheimer, contends that "We must abandon all attempts to explain it [movement] by one of the customary psychological theories, completion or elaboration of sensations by the so-called higher functions" (**22**, 269).

We see, then, that, correctly considered in its proper historical setting, the work of Wertheimer was not so startling as one might gather from the apparent impression made upon certain psychologists.[13] The descriptive facts were not wholly new; neither was the insistence upon the unity of experience. It is, then, to his explanatory concept that we must attribute, as also does G. E. Müller (**36**, 105), most of his influence. The probem had been previously studied by many individuals throughout years of research upon the movement-experience. There were many contradictory theories; some stressing peripheral, others central, conditions. Exner's demonstration that the central conditions appeared to play a very important role must, as we know, have left its mark upon those who knew their literature.

With this rich historical background Wertheimer approached the problem of explanation. His speculative cerebral explanation has, to be sure, its attractive features. It is novel, and it is partially adequate to the facts. Furthermore, it aids one in thinking, inasmuch as the simplicity of the theory conduces to an easy transliteration into visual terms of unknown physiological events. Let us indicate specifically what appear to be the essential points in the explanation of movement in the doctrine of the *Gestalt*. Assume point *A* in the cortex, stimulated by an excitation from receptor organs; and adjacent to it point *B*, likewise stimulated, either simultaneously or successively. Now, with these two points given, let us assume further that the cortical process arising from the stimulation of point *A* or *B* is not confined or limited to the particular area determined by the stimulus but that it radiates from the original point of arousal in all directions, becoming as is said a spatially and temporally extended process. Streaming out, not unlike rays from a luminous body, or waves from a central disturbance certain portions of the neural process, provided the intensity of the stimulus is of a given degree, pass completely from *A* to the "region" *B*. If the interspatial cortical distance is traversed by the excitation from *A* without intercepting a similar process from point *B*, we have the so-called successive stage. *A* runs its course before *B* appears. But if we assume that *B* is stimulated after a temporal interval such that the excitation from *A* has not completed its course to *B*, then the two processes will meet provided the interspatial cortical distance *AB* is not too great. Assuming simultaneous arousal and similar rates of outspread for the two, the point of juncture will be half way between the two regions and we will have experientially the stage of simultaneity and neurologically the simultaneous *phi*-function. We may represent this event in this simple way: *A*——m——B. When the two excitations meet at m (half way) we apprehend the figures simultaneously and at rest with no movement between them. Assuming, however, that the cortical process from *A* has just reached m when point *B* is stimulated, then with equal rates of radiation, the meeting point may be represented by O where O stands half way between *m* and *B*, or ¾ the cortical distance from *A* to *B*. We represent it thus: A——m——O——B. Here we have represented the physiology of the optimal stage. In all cases the rate of outspread is determined by the intensity of the cortical excitation which in turn depends upon the intensity of the objective conditions. If the objects are too widely separated, the two excitations will be unable to form a unitary whole. So there will be no movement. If the pause is too long, the same result will follow. And, finally, even though the objects are fairly close together and the pause short, there may be no movement-experience because the intensity is too weak to effect a short circuit. Thus we have three sets of physical conditions; *viz.*, intensity of the stimulus, interstimulus distance, and length of pause to which correspond, on the physiological side, intensity of excitation, rate of flow, and distance of flow. The significance of the pause for the movement phenomenon now becomes clear. During the temporal interval the process of short-circuiting is complete, thus furnishing the neurological basis for movement. During the pause the rate of neural outspread is assumed to be four times as great as during the period of exposition and stimulation.[14] In the latter period, the radiation is assumed to be relatively inhibited. Toward this alleged central filling-out during the interval Hillebrand is skeptical. Referring to certain of his experiments, he says, "Der Hauptsache nach kann man also sagen, dass die stroboskopische Bewegung vom zweiten Punkt gemacht wird und es wird schon dadurch der Gedanke nahe gelegt, dass es sich nicht um eine irgendwie geartete zentrale Ausfüllung der Strecke während der objektiven Dunkelpause handelt (wie dies z. B. die Kurzschlusstheorie annimmt), sondern dass es das zweite Objekt ist, welches während seiner Sichtbarkeit eine lokale Umwertung erfährt" (13, 233).

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[13] Koffka finds that Wertheimer's contribution presents three outstanding phases; "(1) Descriptive: Diese Gestalten sind in keiner Weise weniger unmittelbar als ihre Teile, 'oft wird das Ganze erfasst, bevor überhaupt die einzelnen Teile ins Bewusstsein kommen.' Die reine Deskription der Erlebnisse kann daher nicht mehr am Empfindungsbegriff orientiert werden, sie wird von der Gestalt und deren Eigenschaften auszugehen haben; (2) Functional: Die typische Form der verbindung Reiz-Erlebnis ist nicht mehr die Empfindung. So wie die Gestalten deskriptiv nicht weniger unmittelbar sind als ihre Teile, so sind sie, funktionell, auch nicht weniger ursprünglich. Der Versuch, das Ganze aus seinen Teilen abzuleiten oder es über ihnen aufzubauen ist sehr oft verfehlt, das Ganze ist nicht durch Zusammensetzung der Stücke entstanden, sondern als der dem Reizbestand gerade korrelate Erlebnisbestand ebenso direkt aufzufassen, wie dies bisher nur bei den Empfindungen üblich war: And finally (3) Physiological: Die typische Form des dem Erlebnis korrelaten Hirnvorgangs ist nicht mehr die Einzelerregung einer Hirnstelle plus Assoziation, sondern es sind Gesamtprozesse und in erster Linie deren Ganz-(nicht Summen-) Eigenschaften, die für die weitere Hypothesenbildung benutzt werden müssen" (**23**, 58 ff.).

Taking Wertheimer's work as a guide, Kenkel (16) investigated the movement-experience as given under conditions where the objects were of the so-called illusory class. His principal figures (pp. 448-449) were various forms of the Müller-Lyer illusion, also circles, sectors and triangles. From the successive presentation of these Kenkel obtained three district types of movement, namely the *Alpha*, the *Beta* and the *Gamma*. The *Alpha*-movement is obtainable only under illusory conditions, inasmuch as it is the movement which comes from the successive apprehension of two lines which are physically alike but appear as spatially dissimilar. Thus the long line in the Müller-Lyer appears to contract or to expand depending upon the order of presentation. The *Beta*-movement is the common type obtained from objects which differ in spatial dimensions or in spatial location. The wings of the Müller-Lyer *e.g.*, fly around giving pronounced

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[14] Wittman points out a curious fact which appears, under certain conditions, to be connected with the temporal interval between the two expositions. For a practised *O*, he says, "tritt die Dunkelpause immer mehr zurück, das soeben exponiert gewesene Bild besitzt eine noch erhöhte psychische Präsenz bei Eintritt des neuen Bildes, so dass der Eindruck entsteht, als beginne die 'Bewegung' schon während der Exposition des ersten Bildes und dauere während der Dunkelpause an; dieser Eindruck wird noch verstärkt bei Verkürzung der Dunkelpause, wenn sich die Bewegung dem sog. Optimalstadium nähert; jetzt ist die Präsenz zweier Bilder bei gänzlichem Uebersehen der Dunkelpausen noch wesentlich erhöht' (52, 23).

Beta-movement. To this general class belong Wertheimer's various types of movement. The *Gamma*-movement[15] is that unique experience which appears under short and rapid expositions together with continued observation of a single object or figure. The object appears to contract and expand, particularly under repeated and continuous observation. It is to be noted that the *Gamma*-movement possesses peculiar significance for certain advocates of the *Gestalt-Theorie*. It affords, as it is claimed, indubitable proof of the validity of certain of their hypotheses. How is it possible to apprehend a single unmoving object as in motion? As expanding under illumination and contracting under reduction of the light? The expansive movement is said to be conditioned through the radiation or outspread of the neural excitation from the given stimulated cortical region. When the illumination is quite intense the phenomenal increase is proportionally great, and conversely. The contraction of the figure, on the other hand, is due to the flowing in upon itself of the cortical excitation. Koffka says: "Die Ausdehnung (*Gamma*-Movement) haben wir schon auf die Erregungsausbreitung zurückgeführt, entsprechend deuten wir das Zusammenziehen als Zurückfliessen der ausgestrahlten Erregung in ihren Ursprungsort" (**20**, 270). More recently the *Gamma*-movement has been made the object of an extended investigation by Lindemann (**28**), who has sought in particular the relative significance of the length of exposition time, temporal interval, intensity of stimulus, practise and the particular configuration of the object. He points out the curious fact that when the figure is apprehended as an object (or thing) it tends to remain at rest, but when it is apprehended as a geometrical oval or surface pronounced extension and contraction may be observed. Wingender's (**51**) principal objects were the Hering, Zöllner, and Müller-Lyer illusions. He reports upon the unitariness of movement, and especially upon the fact that the movement is apprehended as temporally disjoined from the form of the objects. "Die Deformationen der Hauptlinie sind beim Hinzuschliessen der Nebenlinien als Bewegung sichtbar: wenn die Nebenlinien schon an ihrem Platz stillstehen, bewegen sich die Hauptlinien mitunter noch weiter" (p. 24).

The discovery of still another type of movement was left to Korte (**26**), who found that under certain conditions of attention and fixation, together with a proper relationship between the intensity of the illumination, the magnitude of the objects, distance between the objects and length of pause, movement could be apprehended with its direction from b to a although a appeared first in the visual field. He calls this the *Delta*-movement and indicates a great number of conditions under which it may be apprehended and the various factors which affect it. Attention and point of fixation he found to be very important. Under a shift in attention in the visual field *Delta*-movement could be made to disappear and to reappear. He sought to express the relationship obtaining between the three major physical conditions (*viz.*, intensity of objects, spatial separation, and length of pause). Where i stands for intensity, s for the distance between the figures, and p for the temporal interval, we have, according to Korte, the following laws.

s (optimal) varies directly with i
i (") varies inversely with p
p (") varies directly with s.

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[15] "Anfangs erschien der Gegenstand in Ruhe, kam als Ganzes und verschwand als Ganzes; bei gesteigerter Geschwindigkeit kam zunächst unbestimmte Unruhe in den Gegenstand, die sich bald zu einem bestimmten Zucken, dann zu einem Stossen, Schlagen, Strecken, Zusammenziehen, Klappen usw. verdeutlichte. Die Bewegungserscheinung steigerte sich also mit der grösseren Geschwindigkeit. So verhielt es sich innerhalb der hier gezogenen Grenzen, doch gibt es eine Geschwindigkeitsgrenze, bei der ein Maximum von Bewegung auftritt. Darüber wird der zweite dieser Beiträge berichten. Zuerst verbindet sich die Bewegung mit dem Erscheinen und Verschwinden der Figur, beherrscht aber bei maximaler Geschwindigkeit, die sich mitunter bis zu 8 σ Exposition steigerte, die ganze Erscheinung, wenn auch Erscheinen und Verschwinden noch oft stärker bewegt sind, und zwar ist im allgemeinen das Erscheinen mit Vergrösserung und das Verschwinden mit Verkleinerung verknüpft" (**16**, 402).

The temporal, spatial and intensive characteristics of the objects as so determined were further investigated by Cermak (6), whose results tend to show that the laws have a wider applicability and a greater significance for the *Gestalt-Theorie* than Korte indicated.

In 1920 experiments upon the *phi-phenomenon* (*Beta*-movement) were reported from the psychological laboratory of Cornell University. Under a 'process' instruction the mental correlate of Wertheimer's experience was discovered to be a 'flash of gray'. In his final description of this gray, Dimmick (7) writes: "It must not for a moment be assumed that it is a simple text-book gray, the gray that lies at the center of the color-pyramid, or the gray of a piece of gray paper. The temporal attribute is beaten up with the quality into an integration of the first order. All *O*s describe the experience as a 'flashing,' 'live' gray, ... a 'curtain' or 'film' " (pp. 331-332). Under a perceptual attitude, created by a 'meaning'-instruction, the *O*s apprehended various degrees of movement. There appeared full-movement, including the pure *phi*-phenomenon, bimembral, and unimembral movements, as well as the intramembral variety. On the whole, Dimmick's investigation tended to confirm Wertheimer's descriptive facts. Thus all of Wertheimer's movement-types were reported from the Cornell laboratory. Dimmick further agrees with Wertheimer that there is a stage approaching simultaneity (ca. 30 σ) an optimal stage (ca. 90 σ) and a stage of succession (ca. 150 σ). The investigations differed with respect to the interval necessary for full-movement, the instruction and the report, and the qualitative filling in the field of movement.[16]

In a still more recent investigation of visual movement, in connection with an analytical study of the Müller-Lyer illusion, we find emphasis placed upon the simple and elementary character of the movement-experience. Referring to the nature of visual movement, Bates (3) says that movement is actually "seen in sensory terms," and is "a definite experience distinguished from displacement and change in extent—a sheer experience of the order of mental process" (p. 60). A critical and analytical study has lately been made by Wittman who claims that, although Wertheimer really regards the sensory character of movement as simple and irreducible (when he refers to the "sinnlich klar and deutlich gegeben Eindruck der Bewegung"), his optimal movement is never sensory; but only the "Phasen sowie das Feld in dem sie erscheinen" (52, p. 15). According to Wittman, even under these conditions which give pure *phi* there is still objective meaning (p. 16). Concerning the alleged lack of any spatial or qualitative characteristics in Wertheimer's phenomenon, Wittman has to say of the first that the apprehension of movement between two things "als solche ist[es] zunächst ein Raumerlebnis" and about the second he remarks that of movement "als ein selbständiges Phänomen, das ohne Bezug auf ein Objekt gar mit sinnlicher Qualität erlebt würde, dem als physiologisches Korrelat ein physiologischer Kurzschluss zugeordnet werden könnte, darf er nicht bezeichnet werden" (p. 23). Wittman's own explanation is quite simple. "Mit dem Bewusstsein der Lagenverschiebung verbindet sich sodann das Bewusstsein des Lagenüberganges, und das Erlebnis dieses 'Lagenüberganges' is mit dem Sehen einer Bewegung identisch" (p. 22).

We have come now to the end of our historical survey. Although much ground has been covered, it represents, after all, but a relatively small part of all the work done on the visual apprehension of movement. We have attempted to review here merely the outstanding experimental investigations of a certain class of visual movements and to indicate the nature of the contributions, historical, descriptive and explanatory, to the general problem. We have sought especially to trace the antecedents of the *phi-phenomenon,* the phenomenological treatment of which has directly led, as we have indicated, to the origin of the *Gestalt-Theorie* and indirectly, by way of the development of this doctrine, to many important studies in perception. We now turn to a consideration of the problem of the present investigation.

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[16] Whitchurch has recently investigated the cutaneous apprehension of movement in the Cornell laboratories. She concludes that "movement is, after all, a gross meaning" (50, 489). *Cf.* H. Burtt, Tactual illusion of movement, *Jour. Exp. Psych.*, 2, 1917, 62-75; 371-385.

B. *Statement of the Problem.* We are not concerned with the visual apprehension of movement at large and still less do we attempt a general survey of all forms and varieties of this mode of perception. Confining ourselves to vision, we disregard all those perceptions which rest upon an actual motion in the object as well as certain experiences in which no physical object acts immediately upon the visual receptors. Thus we omit the apparent movements due to prolonged fixation, the movements incident to vertigo, illusory dislocations of movement, movement due to the externally turned eye-ball, and the like. All these forms of visual perception involve the organism in a manner unlike that by which successive, resting, retinal stimulations condition the apprehension of movement. It is perceived movement under these last-named conditions that is to be considered in the following pages.

Through the employment of an apparatus devised, perfected and tested throughout several preliminary months an attempt has been made to approach the fundamental problems involved in the studies already reviewed. *Our* problems are four; (1) to discover the significance of the temporal interval, (2) to write a descriptive account of the several aspects of the movement-experience, (3) to scrutinize the explanatory hypotheses of the *Gestalt-Theorie* (so far as these hypotheses relate to visual movement), and (4) to examine the claim that the psychological correlate of visual movement is a certain "gray, fairly brief in duration." As to method, we have tried so to modify objective conditions as to give us a greater control over the extra-organic factors than has been obtained in most of the previous investigations. To this end we have eliminated from the visual field everything except that which was absolutely necessary to the perception.[17]

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[17] Improvement in technique is always possible after the work: A semi-transparent membrane over the slits in the stimulus-cards would have reduced irradiation of the light as it passed through the lines.

II. The Experiments

A. *Apparatus.* Our exposure-apparatus was a mirror tachistoscope of the Dodge type. We embodied in it, for our special needs, several modifications, as shown in Fig. 1.

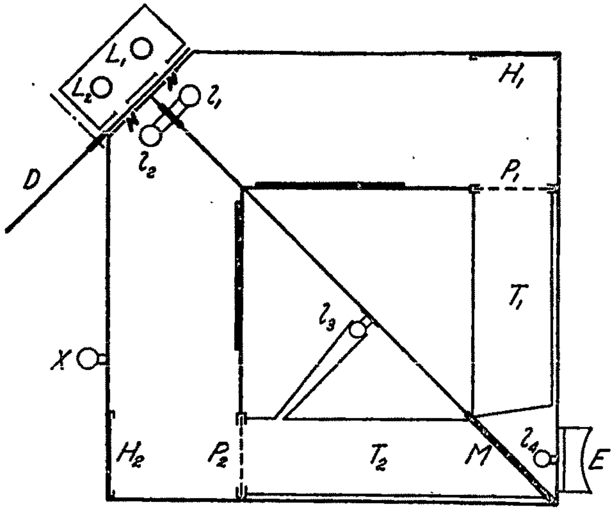

Figure 1.

At P_1 and P_2 frames were constructed to hold the cards carrying the stimulus patterns. At M was placed an adjustable platinum-iridium mirror so made as to reflect at E (the eyes) as much light from P_1 as it transmitted when illuminated from behind, at P_2. Anchored to the bottom of the box and connecting the frames at P_1 and P_2 with the mirror (M) were 4 in. black tubes (T_1 and T_2).

A small light l_3 attached to the diagonal wall was joined by a ½ in. tube to an aperture in T_2. Thus a weak light (too weak for after images) could be thrown upon the anterior side of the card at P_2 for fixation. Another light of variable intensity (l_4) directly over *O*'s eyes reduced flicker. Two moderate lights (l_1 and l_2), controlled independently by a series of 5 keys, presented either object for any desired duration and intensity. Two large, variable 150-watt Mazda lights (L_1 and L_2), housed in a lightproof box in front of the windows (N) of the tachistoscope served as primary source-lights in Series I-IV and VIII.

Exposition times and temporal intervals were controlled by means of the 3-ply wooden disc D (diam. 88 cm.), supplied with two open radial sectors, rotating between the light box and N. One of the sectors was adjustable over a graduated arc of 110°. Before N was placed a card with openings identical with those in the disc. The rate of rotation, and accordingly the temporal interval, was widely and accurately varied by a reduction cone of 16 grooves. Additional variation was obtained by use of a 10 speed d. c. motor to drive the cone. Constant check upon speed was made during experimentation by means of a voltmeter across the terminals of the motor.

B. *Method.* Our general procedure was to present 2 straight lines similar to the principle objects used by Wertheimer and Dimmick. These objects were slits in dark cards (with certain exceptions noted later) placed at P_1 and P_2. They were apprehended under experimental conditions as bright lines upon a dark or lightless field. We shall call the oblique-horizontal arrangement of the lines ($\angle$) "A", and the parallel-horizontals (=) "B", to designate the two principal object-patterns.

In both A and B the lines were 25 mm. long and 1 mm. wide. In A the angle was 45°. In B the interlinear distance was about 15 mm. The stimulus objects were presented either in sequence, with a definite temporal interval, or else (in some series) simultaneously. 8 series of experiments were conducted with A and 8 with B.[18] Every one of the first four series consisted of 20 observations for every *O*,[19] and they covered the 10 temporal intervals 15, 30, 45, 60, 75, 90, 120, 150, 180, and 225 σ. In Series I-IV and VIII the exposition time was 50 σ. These 10 intervals range, according to Wertheimer and Dimmick, from simultaneity to succession. Every one was presented twice during every period of observation. In this way we were able to check possible variations in report under the same objective conditions during the course of a single sitting. The order of the temporal intervals was determined by a chance distribution to avoid cumulative effect or the occurrence of an observational set which might appear under a sequence in which there was a gradual increase or decrease in the duration of the interval.

In Series I-IV, A and B were presented on alternate periods of observation, B following A in each series. Thus, in Series I and III, the oblique was followed by the horizontal (object A) and the upper by the lower horizontal (object B). In Series II and IV the order of presentation of the two lines was reversed. In this way we were able to determine practice effects contingent upon repetition, the effect of one stimulus-pattern upon another, and possible differences in the movement-experience due to a reversal of sequence.

In Series V, a study was made of the visual apprehension of movement under the mere addition of one member to another *without interval*, to form, in the one case, (A), an acute angle and, in the other, (B), two parallel lines. The method was simple. One light (l_1 or l_2) was turned on; *O* attended for 5 sec., and then the other light (l_2 or l_1) was added. Both lines were then simultaneously removed after 5 sec. more. 5 observations were taken for every *O* where the oblique was added to the horizontal, 5 for the reverse order and a similar number for each of the lines in B, making a total of 50 for everyone of the four cases.

Series VI was similar to Series V except that one member was subtracted or removed from the total configuration. Both lines were together presented for 5 sec. and then one line was removed by turning off one of the lights. The same number of observations was made as in V.

In Series VII the temporal interval between the two successive expositions was zero. One object was given for 5 sec. (l_1 or l_2), followed then by the other (l_1 or l_2) at an intensity such that the instantaneous appearance of the second object effected the simultaneous disappearance of the first (although the first light was unchanged). In order to obtain the degree of intensity required to wipe out the first figure it was necessary to change the character of our stimulus-objects, since the slitted lines would not transmit sufficient light energy. We accordingly drew the objects in black upon a light grey card and placed them against the outer wall at H_1 and H_2. The slitted cards were removed at P_1 and P_2 leaving a rectangular opening 3x2 in. through which the objects at H were apprehended. Using this arrangement we found it very easy to present one figure for any desired time and then to shift instantaneously to the other again. 5 observations were made by every *O* in every one of the four possible cases.

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[18] An additional series of 200 observations was made with stimulus-pattern C-o two adjacently placed circular holes 1 mm in diam. Since there was no difference obtained with this and with patterns A and B, the results are not included.

[19] 10 persons observed throughout the entire experiment. Members of the departmental staff: Dr. C. R. Griffith (G), Dr. P. T. Young (Y), Dr. E. A. Culler (C), Dr. Alice Sullivan (S), Dr. H. L. Kingsley (K), Mr. Donald Rothschild (R), assistant in psychology, and the writer (H); graduate students in psychology: Mr. W. Mikesell (M) and Mr. W. Zuschke (Z), and Mrs. Ethel Higginson (Hn). Mrs. Madison Bentley (B) and Miss Helen Hopkins (Hs), assistant in psychology, observed in Series I-IV. When *E* acted as *O*, Hn took his place at the apparatus.

Series VIII differs widely from all the preceding, where only 1 exposition of each figure was made before *O*'s report. In this last series continuous observation (5 exposures) was made with every temporal interval. 3 reports were made by every *O* for every one of 11 intervals (o-interval added to the 10 above.)

The entire experiment was conducted in a dark room with one small unenclosed light (X). *O*'s seat was completely enclosed by black cloth on three sides and the top. Under these conditions there was complete removal of all stray light.[20]

O sat within the curtained embrasure with the head maintained firmly in one position by an adjustable support. At the signal "Ready" the control key was lifted giving the fixation-light. Immediately following the signal "Now" and just after the sectors in the disc had passed the light-box, the key was pressed down, removing the fixation-light and turning on the large lights (L_1 and L_2).

These lights always had more than ½ sec. in which to attain full brightness before the radial sectors again reached the light box. In this way all effects of inertia in the lights were eliminated. After the openings had again passed N giving one exposition of each member of the object (except in Series VIII), the key was released. During the moment that *O* took to organize the report, the experimenter released the motor, stopped the disc, reset the movable sector or the motor for a different time-interval and restarted the motor. After the report was taken the next exposition was immediately given. This was the general procedure for A and B in Series I-IV and VIII.

All the *O*s worked under formal instruction which was given before every sitting.

"Fixate steadily the small lighted area in the box. Close first the one eye and then the other to make sure that you see the light with both eyes. When the lighted area disappears, continue to observe the field without change in fixation.

Observe carefully whatever comes and report orally to *E*. Be sure to include in your descriptive account particular and precise statements as to the order and nature of the appearance and disappearance of the phenomenal object or objects.

At the end of each report, indicate by the number 1, 2, 3 or 4 the degree of assurance concerning the fullness and accuracy of your report. The numbers 1, 2, 3 and 4 stand as follows: 4 represents a high degree of assurance; 3, moderate assurance; 2, small assurance; and 1, complete or almost complete lack of assurance."

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[20] Every *O* was allowed 2 min. for adaptation; although it was soon found that adaptation had little, if anything, to do with the matters in question.

Our preliminary reports were not sufficiently standardized. The demand for a "descriptive account" proved to be ambiguous, as it obviously as in Wertheimer's experiments. In our regular experiments we specified as follows:

"Do not speculate upon the apparatus or upon experimental conditions. You are not to guess at or to reconstruct the conditions in the apparatus. This is not a test of alertness or of intellectual acuteness. You are to omit all assumptions, inferences or associative reflections, as well as all analogies and outside comparisons. Strive to translate as precisely as possible into verbal terms your actual visual experience. Nothing else. Make it photographic. You are not depicting an event in its natural setting. You are describing a phenomenal, not a natural, object. Do not make your reports figurative. Confine them to qualitative, temporal and spatial properties and relations."

III. THE RESULTS

Series I-IV; Object A. The results of this series are summarized, on the quantitative side, in Table I. The upper horizontal line of figures after every *O* gives the total number of each type of movement apprehended in Series I and III (a-b sequence); the lower line, in Series II and IV (b-a sequence). There were always four trials.

The most obvious fact in the Table is the prevalence of cases of full movement (FM); *i.e.*, movement from the position of one line to that of the other. Of 81 cases of "no-movement", all but 26 are from two *O*s (M and Z).[21]

It must not be assumed that 'full-movement' always meant that a line was apprehended to move, as a line, across the field; although this is the usual meaning of the term in previous studies. The *O*s instead reported at times that the experience *meant* that one line moved from one position to another; but that the actual movement of a line across the field intermediate to the two perceived positions was not seen. At other times, it seemed to be just bare movement over the field. Description of this movement was very difficult. It was insistent yet baffling. It was an approximation, apparently, to Wertheimer's 'pure *phi*-movement'. At least this is indicated by such reports as the following:[22]

"Two bright lines; the oblique slightly first. Movement referred to the whole region between the lines but not the movement of a line. (Perhaps a judgment in terms of eye-movements)" (G, 4).

"A horizontal line, broken in the center, appeared in the lower part of the visual field. The line was not sharply defined. Then a clear oblique appeared. The two formed an angle. The horizontal then disappeared and the oblique persisted for a moment. Between the lines and filling the space was a yellow haze of low chroma. I saw very rapid movement from the lower to the upper position. I am sure of movement, but not sure it was a line" (Y, 5).

"A horizontal line appeared and disappeared; then an oblique line appeared. There was no qualitative change in the field between them. There was, however, an impression of movement from one position to another, but not of a line" (Hs, 10).

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[21] M had difficulty in adjusting himself in the beginning to our experimental conditions. He frequently complained that he failed to focus. He likewise observed that the total exposition was so rapid that he was unable clearly to perceive the object. It was often necessary to repeat in order to obtain a report. Z appeared at times to work under the self-instruction that the change was not true movement unless it approximated the nature of a physical object in motion.

[22] The numeral after the letter for the *O* shows the temporal interval from which the report was taken.

Table I, Series I-IV; with Object A

	1					2					3					4					5					6					7					8					9					10				
	15					30					45					60					75					90					120					150					180					225				
	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I
G		4					4					4					3	1				4					4					2	2	1			4						4				2	2		
	1	1	2				3	1				3	1				4					4					3	1			1	2	1				3	1				1	1	3			2	2		
Y		4		1			4					4					4					3	1				4					4					4					2	2				3	1		
		2	2				4					4					4					4					3	1				3	1				4					4					4			
C		2	1	1			4	1				4					4		1			3		1			2	2				3	1				4		1			3	1				4			
		2		2			2		2			4					4					4			1		4					4					2	2				2	2				2		2	
S		4					4					4					4					4					4					4					4					4					4	1		
		4					4					4					4					4					4					4					4					4					4			
B		4					4					4					4					4					4					4					4					4					4			
		4					4					4					4					4					4					4					4					4					4			
K	2	2					4					3	1	1			4				1	2		1			4					4					4				2	2					4			
		4					4					4					4					4					4					3	1				2	2		1		4					4			
Hn		4					4					4					4					4					4					4					4					4					4			
		2		2			1		3			1	1	2			3		2			3		2			4		4			2	1	1			2		4			2		4			3		1	
H		4					4					4					4		1			4					4					4					4					4					4			
		4					3	1				4					4					3		1			4					4					4					4					4			
Hs	1	3					4				1	3				2	2					4				1	3				1	3				2	2					4					4			
	1	3					4					4					4					4		1			3		1			4					4					4				1	3		1	
R		4				1	3					4					4					3	1				4				1	3					4				1	3					2	2		
	2	2					4					3		1		2	2				1	3	1				3	1			1	3				1	3					4					2	1	1	
M	3	1				1	3					4					4				3	1					4				3	1				4					3	1				3	1			
		4				2	2					4					4				1	3				2	2				2	2				2	2					4				1	3			
Z		3		1	1	2	2				1	2			1	1	1	2			4					1	2	1			2				2	3				1	1				3	2				2
	2	2					4				1	2			1	1	3					3			1	1	3					4				2	1	1				4					2		2	

O = no movement; F = full; P = partial; D = dual; and I = inner movement. Full movement followed by a partial return is entered as FP; followed by a displacement of the end-position is entered as FD.

TABLE II, Series I-IV; with Object B

	1					2					3					4					5					6					7					8					9					10				
	15					30					45					60					75					90					120					150					180					225				
	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I	O	F	P	D	I
G		4		1			2	2				3	1				4					1	3				4					3	1				4					1	3				2	2		
		4					4					3		1			2	2			1	2	1				2	2				3	1				4					2	2				2	2		
Y		4					4					4					4					4					4		1			4					4					4				1	3			
		4					3	1				1	3				2	2				4					1	3				1	3				3	1				2	2				2	2		
S		4					4					4					4					4					4					4					4					4					4			
		4					4					4					4					4					4					4					4					4					4			
C		2		2			4		1			4					3	1	1			3	1				2	2	1			2	2				2	1	1			4					4		1	
		4					4					4					4					4					4					4			1		3		1			4					4			
Hn		4					4					4					4					4		1			4					4					4		1			4					4		2	
		4					4					4					4					4					4					4		1			3	1				4					3		2	
Hs		4					4					4					4					4				2	2				3	1				1	2	1				1		3		1	3			
		4					4					4					4					4					4					3	1				4					4				2	2			
M	2	2				2	2					4				1	3				1	3					4					4				1	2	1				3	1				3	1		
	1	3				2	2					4				1	3					4					4					2		2			4				1	2		1			4			
B		4					4		1			4					4					4					4					4					4					4					4			
		4					4					4					4					4					4					4					4					4					4			
Z		3			1				1	3	2	1	1			2	2					4				1	2		1		2	1			1	3	1								4	3	1			
	2	2					4				3	1				2	2					4				3	1				2	2				1	3				2	1			1	1	3			
H		4					3	1				4					4					4					2	2				4					4					4					4			
		4					3					4					4					4					4					4					3	1				4					4			
K		4				1	3	1				4					4					4					4					4					4					4					2		2	
	2	2					4					2		2			4					4					4					4					4					4	1				4			
R	1	3					4					4					4					4					4					4					4					4					4			
		3		3			4		1			4					4					4					4					4					4					3	1			1	3			

TOTALS	15		30		45		60		75		90		120		150		180		225	
Tables	I	II	I	II	I	II	I	II	I	II	I	I	I	II	I	II	I	II	I	II
O	12	8	6	5	3	5	6	6	10	2	5	6	11	7	14	6	7	3	7	9
F	73	82	83	83	85	83	86	85	79	89	84	80	75	78	73	82	72	75	73	77
P	5	0	3	5	3	5	3	5	3	5	6	9	7	8	6	6	10	10	9	7
D	7	6	5	4	4	3	4	1	6	1	5	3	2	3	5	3	7	4	7	7
I	1	1	0	3	2	0	0	0	2	0	0	0	2	2	2	0	3	5	2	0

"A horizontal line appeared followed immediately by an oblique. There was vague movement over the field between the lines. Something moved from the first to the second positions" (Z, 8).

"Both lines appeared, distinctly separated and sharply defined. There was a pronounced impression of movement over the whole sector although each line was clear. There was no sensory blur" (C, 6).

"Distinct impression of two lines given practically together in time. There was movement from the upper to the lower position, but I saw both lines clearly" (K, 2).

In those cases of FM (full-movement) in which an object was apprehended as actually moving across the field, *O* often reported a line in various intermediate positions. The successive apprehension of these objects appeared to condition the perception of actual movement, or rather it meant that a line was seen moving across the field and occupying these successive positions. All *O*s reported these intermediate lines at one time or another. Occasionally 1 only would be reported; again 4 or 5. These lines were frequently observed to be quite like the initial and final lines. In fact, some of the reports indicate that the *O*s did not realize that only 2 lines were presented; instead they saw "several similar lines or the same line in several positions."

"A diagonal line appeared and moved to a lower position. Movement was smooth and uniform in rate except at a middle point in the descent where the line paused for a moment. At this point, I saw the line stand out. The remainder of the field was qualitatively like the background" (S, 8).

"The movement of a line across the field in three stages. The stages were clear. It was as if a line of light were emitted in sequence, or rather, as if a slit were being moved down and light came through the slit at these points" (B, 8).

"Movement very much as before. The lines were of the same length or perhaps it is the same line exposed clearly in four different positions. Movement was fairly smooth and uniform" (B, 10).

"I saw definite movement downward of a line; *i.e.*, I saw a line in different positions as it moved from an upper position to a lower position. The four intermediate positions were not so clear as the end positions" (K, 7).

"An oblique line appeared and moved to a horizontal position. There was a series of three intermediate positions between the upper and lower positions. Very rapid and continuous movement" (R, 5).

"I saw a line move from an angular to a horizontal position. There was first a bright yellow line against the black background. After it had persisted for a time it was cut off. After it was cut off it moved down smoothly to a horizontal position. I saw the line in various positions. I saw it there, and there, and there, as it moved. I saw nothing more" (Y, 9).

"I saw an angular line which moved from an upper to a lower position. I actually saw the line assume against the black background two or possibly three intermediate positions before it reached the horizontal position" (Y, 5).

In other cases, FM of a line across the field without intermediate positions was apprehended. Sometimes the bare apprehension of two lines appeared to carry the movement meaning. Quite often the field between the lines was reported to be illuminated in such a way as to aid in conditioning the movement perception; *i.e.*, as if a line moved across so rapidly as to leave a blur or trial of light.

"Both lines well defined but closer together, spatially and temporally, than before. Obvious movement of the top line as it moves quickly into the bottom position. Very little, if any, qualitative change in the field between the two positions" (C, 8).

"An oblique line emerged and moved into a lower position. As it moved it changed gradually from an oblique to a horizontal direction. Nothing else was to be observed" (M, 6).

"An oblique line appeared followed by a lower horizontal. Both clear. The upper moved rapidly to the lower. The field was unlighted between the lines" (Hs, 5).

"A line appeared in an angular position. It appeared to drift slowly to a horizontal position. The horizontal was broken in the middle. A faint haze which spread out from the lines appeared to cover part of the field between" (Y, 10).

Another outstanding fact as shown by Table I is the lack of any dependence of FM upon any particular temporal interval. Almost as many cases of FM occur at 225 σ as at 75 σ, the interval midway between the determined 'optimal' times of Wertheimer and Dimmick. At an interval where phenomenal simultaneity without movement occurred for Wertheimer, we found very insistent movement. At 225 σ where, prior to the development of our method, we had assumed that discrete succession would occur, 76 percent of all reports gave FM and less than 9 percent no-movement. Our results for this series indicate that it would be unwise to attempt to state that full-movement is to be obtained most often with any certain temporal separation of the lights. Slight variations in self-instruction might result in a complete shift of a small percentage of FM reports in favor of one interval or another. In fact our *O*s might see FM with an interval which 10 min. later gave 'no-movement.' There may be, it is true, slight qualitative differences in the character of the movement apprehended under rather widely separated intervals due to slight differences in the organic resources called into operation; but the predominant meaning with all of our intervals appears to be that movement occurs over the field between the two positions.

There appeared, moreover, to be no one interval or group of intervals which tended especially to condition either dual- or partial-movement. Dual-movement was reported in some cases under complete phenomenal simultaneity. Both lines appeared in the form of an angle which in turn moved up or down. At times, however, the two lines seemed to move toward each other,

"An oblique line appeared followed by a horizontal. Together they formed an acute angle filled with a dim yellowish light. The whole sector seemed to shift as a unit rapidly downward" (G, 7).

"I saw an acute angle. Both members of which were moving; *i.e.*, the top moved downward and the lower moved up as if to meet it" (Hn, 2).

"Two lines, an oblique and a horizontal shot out to the periphery and then moved slightly toward each other. A halo of light surrounded each" (Z, 1).

"Two lines given together. The spatial interval was dark. Both lines seemed to disappear almost simultaneously. The experience meant that an angle shifted up a few degrees and back again" (Y, 1).

"Both lines practically together in time. The field clear between. The oblique less intense than the horizontal. Both lines moved slightly up and then down" (K, 3).

On the other hand, dual-movement was sometimes reported where the interval was quite long. Here the movement assumed various forms of which the most common was a completion movement of the second line as it moved away from the position of the first and into its final position. Dual-movement easily became partial-movement, depending upon the interpretation placed upon the last stage of the movement. At times *O* saw a line move down and disappear and then suddenly reappear lower down and move to a final position. It meant that the same line was moving ahead. In such cases, we can scarcely speak of dual-movement as it was originally used by Wertheimer. Rather it is a 'continuation' or a 'partial'-movement of one line.

"An oblique line appeared. It was moving rapidly downward when I saw it. After covering about ¼ in. of the field it disappeared into the blackness of the background. Then immediately another line appeared where the first had disappeared and went on down to a resting position. There was nothing in the field other than the lines unless it was a faint glow surrounding each line" (S, 10).

"A horizontal line appeared and immediately moved up about 5 degrees, and disappeared. Immediately I saw an oblique come in above where the horizontal disappeared and move on up to the final position. Field between the end positions was dark except for the intermediate lines" (R, 10).

Partial-movement of one object, the other remaining at rest, was frequently perceived. It appeared indifferently in connection with either, though possibly the second may have been more often observed to move.

"An oblique line appeared but stationary. It started to move downward just as I saw it. Then a horizontal line appeared below. The upper line moved only part (¾) of the distance between the lines. I saw nothing more" (Y, 5).

"The oblique appeared but remained at rest. Immediately following its disappearance, a line nearer the horizontal than the oblique position came in and moved rapidly down to the position of the horizontal" (Y, 10).

"An oblique line appeared followed by horizontal. The horizontal persisted a trifle longer. Distinct movement referred to the horizontal line as it moved into its place over about ½ of the intermediate field" (G, 9).

"Two lines came successively. There is movement here but only in the upper part of the sector as if the oblique appeared after the horizontal and about ⅓of the way up (I saw a line here) and moved on up to the final position. The oblique seemed, then, to swing up over ⅔ of the sector. There was nothing visual between the lines" (K, 8).

Movement within a given figure was seldom observed. Most of the reports were given by Z. The movement was usually experienced as a flowing, a shooting, or a twisting within the line.

"Two lines appeared in succession; a horizontal and then an oblique. The first was dim; the second was clear. There was a flowing movement within the second line,—perhaps back and forth. I am unable to state direction with certainty" (Z, 10).

The effect of repetition upon the apprehension of movement is to be seen (Table I) in connection with the reports of M and Z. By the time Series IV was reached each *O* had had considerable practice. In Series IV (A) only 2 reports of 'no-movement' out of a possible 40 were given by these *O*s.

This fact is obscured by the combination of Series II and IV; but the total of these two shows less 'no-movement' than Series I and III. Practise appears, then, as the factor to which, with the greatest degree of assurance, we may attribute to these two *O*s this difference in visual apprehension at earlier and later periods. It has frequently been mentioned that it requires time and training in order that some individuals may apprehend stroboscopic movement. This was not true of our other *O*s. As may be seen, most of them immediately experienced movement. This is not to be ascribed, either, to a neural set or predisposition created by a particular experimental atmosphere. Some of our *O*s knew nothing of the character of the problem. The same holds true for those persons who happened to visit the laboratory. Out of a score or more who observed (without formal instruction) not one failed to report movement.

No significant difference appears where the direction of movement is upward. There are, it is true, fewer cases of FM; but there are, likewise, fewer cases of 'no-movement'.

It would be interesting to compare a large number of cases of movement in these two directions with various distances intermediate to the two lines to determine whether there exists any real difference in the fullness of movement or of the apprehended distance between members.

If such a difference in movement should exist it would be quite significant for those theories of visual movement which stress eye-movement. Since the eyes move downward with greater ease and rapidity, one might expect a larger percentage of FM reports in connection with the a-b presentation than with the b-a sequence.

Series I-I V; with Object B. The quantitative results for this series are given in Table II. There is a very close similarity between this set and Table I. The experiments with B (parallel horizontals) tend merely to substantiate the facts ascertained in the 4 series with A. FM was apprehended in a large percentage of cases at all intervals and by all *O*s. The one arrangement gives as clear FM as the other.

We find our results in this connection in close harmony with those of Wertheimer and Dimmick. They, too, discovered no significant difference between the two arrangements. There is, of course, nothing to lead one to assume that any real difference in visual movement should accrue to the use of either stimulus-pattern so long as the apprehended distance between successive members of B is not excessive.

Thus far we have not dealt specifically with the qualitative aspect of the movement-experience. Throughout the entire experiment the predominant appeal was to the visual resources of the organism. Owing to the method of stimulation very little qualitative variety of a visual character was to be expected. Quite often merely two yellowish streaks, with or without a halo of dim light, were observed. Various *O*s reported from time to time that the second line appeared a little brighter; as if the first had been added to it, increasing its intensity, or as if the line had suddenly stopped, allowing its spatial character to be better seen. At times, the field between the lines was distinctly different in quality from the background. It seemed partially or wholly filled with a 'hazy,' 'cloudy,' 'dusty,' 'filmy' light. This light might be uniform in distribution; again it might be very patchy, even streaky with decided intensive differences. The light was often unstable and shifty, due to pronounced changes in intensity. It would shoot, dart, stretch out or contract in such a way that it served directly to condition the perception of movement. At other times it was just *there*, resembling Dimmick's visual grey. In so far as it had to do with movement in these latter cases, it appeared as if it were the result of retinal displacement, the blur that comes from a rapidly moving object or the cloud raised by some actual movement. It was, then, one of the visual resources directly aroused on occasion by the stimulus-objects and apparently utilized at times by the organism in apprehending or in qualifying visual movement. We have tried in the general discussion of our results to indicate the exact importance for the apprehension of movement of this visual filling.

Supplementary to the visual qualities, and playing, as we believe, no insignificant part in the perception of movement may be mentioned (*1*) verbal kinaesthesis, (*2*) fairly specific pressures around the eyes and face, and (*3*) a certain rhythmic activity of bodily members, particularly of the feet and hands. Some *O*s report that the expositions were subvocally accented, as 'ta-*ta*', 'there-*there*.' Others find tensions in the feet, as if the heel were put down, followed by a sharp slap of the rest of the foot. Non-verbal lingual movements not unlike a 'pursuit' appear at times with the perception of movement. Whether these various kinaesthetic factors are important for the experience is difficult to determine with certainty. Reports of them seemed chiefly to occur when only the bare lines were apprehended.

A report from Professor Bentley runs as follows "Successive lines. General apprehension of downward movement. Whether it is a meaning built up out of strains I get around shoulders and arms or whether I translate the strains into movement, I don't know" (8).

"Distinct verbal judgment 'It moves. Oblique moves into its place.' There was no phenomenal visual basis" (G, 10). "Verbal judgment 'the upper line jumped into its position' " (G, 7). "Movement was in terms of eye-movements (*i.e.*, nothing visual)" (G, 4).

"I accent the 2nd member. Movement comes, I believe, kinaesthetically. Movement is quite insistent without visual basis" (C, 8). "Very good movement. Kinaesthetic factors were largely responsible for this" (C, 7). "Visually there was no basis. Good movement, however" (C, 8). "Definite movement over the whole angle but not given visually" (C, 6). "I tend to follow the whole performance with my tongue. My tongue moving helps to give me clear visual movement" (C, 10). "Kinaesthetic factors persist right along" (C, 4). "There is a decided tendency to vocalize, to sing two tones of different pitch 'high-low,' 'high-low' " (C, 7). "Distinct eye-movements. I follow the line. Movement comes in other terms than visual in which I get very distinct impression of two lines, but first moves to second" (C, 10).

"Very strong kinaesthesis around eyes and head which meant 'there, there;' with movement from one line to the other" (G, 10). "The experience is accompanied by very clear verbal kinaesthesis and diffuse pressures about the head and chest meaning 'there, there.' These factors, I believe, are largely responsible for the perception of movement" (G, 7). "Definite movement from below upward; little or no visual basis; given, no doubt, kinaesthetically" (G, 4; G, 3). "Rhythmic appearance of three horizontal lines "ta-ta-*ta*' " (G, 3). "I observe a great deal of kineasthesis around eyes and neck. There is constant verbal kinaesthesis meaning 'there-there;' 'moving up;' 'going down' " (G, 1).

"At times, I am sure that as much as I try I am unable to keep my eyes on one spot. I feel my eyes jump when the line comes on" (Z, 2). "Movement over whole sector. Given kinaesthetically" (C, 5). "Two lines in definite sequence and each with a slight halo. Definite movement from one to the other carried largely in non-visual terms" (G, 10). "A number of times I have the feeling of putting my heel down followed by a sharp slap of the sole" (C, 4).

Once more, the visual movement appeared to differ in a qualitative way under different intervals. With short intervals the movement was 'vigorous,' 'lively' and 'insistent.' It seemed to be smoother and more uniform in rate; there were fewer reports of secondary positions. The movement was carried more fully, as it appeared, by visual processes. It was more pronounced, but, we may say, less the movement of a 'real line' than with the longer intervals.

Nevertheless, there was more of a 'turning movement around a pivot' (with object A) reported with the shorter than with the longer intervals, where the lines usually failed to meet at an apex. The movement of the longer intervals was irregular and jerky. There were more stages to it; that is to say, there were more intermediate lines or positions than in the shorter intervals. Such lines generally meant a slight pause in the course of the movement. Where the interval was short with few, if any, intermediate lines, there was usually a leaping movement, a short, quick jump, or a rapid sweep from one to the other position. Where the intervals were long, and intermediate lines absent, the *O*s reported a drifting, floating movement or a slow, easy, flowing movement across the field. This type was never reported in connection with the shorter times. As we have just said, in connection with the longer but seldom with the shorter intervals, all *O*s reported now and then that the two lines (arrangement A) did not meet, or that the oblique had been changed to a vertical. At times, the oblique even bisected the horizontal line.

From our observations in these 8 Series we feel safe in asserting that full visual movement appeared to be conditioned by the successive apprehension of two lines in different positions with or without a qualitative intermediate filling, or by the subjective re-enforcement of the intensive aspect of the second member with the meaning that the first was added to it, or that the second had now come to rest and thus could be clearly seen. Along with these visual resources must be mentioned general kinaesthesis, which constitutes at times, as we believe, an important if not a dominant resource of the organism in the apprehension of movement under our conditions.

What is to be said of dual and partial-movements? Under the same physical conditions that gave FM, *O*s occasionally reported (a) full-movement of one member with slight movements of the other, (b) partial-movements of both, or (c) of one member. Dual-movement, as we have said, appeared under simultaneity, complete succession, or full-movement.

It was difficult to find any specific factor to which it could be attributed. The phenomenal basis was very elusive. When both lines were clearly apprehended, with the intervening field uniformly lighted, there might be distinct movements of each line. This same thing happened when the lines were much the same with the field dark between them. Some cases of dual-movement were reported with the meaning that the upper line moved down knocking the lower line out of its position. Here we have a combination of full- and dual-movements. The dual-movement in these cases seemed to be chiefly conditioned through the apprehension of two lines close together at the bottom, much as would occur if one object thrust another aside and assumed its position.

A form of dual-movement or a modification of full-movement occasionally occurred with the meaning that, because of rapidity of movement, the moving line overshot the position of the second member and was forced to move back to get into its final place.

"Upper line went down beyond its final resting position only to withdraw to it" (Y, 6). "I can't distinguish whether the top line moved part way past the bottom and then back to it or if the lower moved, or if both went down and back after the first is joined to the second" (C, 4). "Movement of lower line beyond the upper line and back again to it" (G, 1). I saw the line in 5 positions in its downward course. It did not stop with the bottom line but went past it and then came back" (B, 2). "Upper line moved past the bottom and back to it" (C, 1, 2, 4, 6, 10).

Partial-movement differed little from FM, except in extent. It was usually described as a swinging, a darting, a jumping, or a jerking movement of a line. At other times it was a smooth, slow, flowing movement over a part of the field. At still other times, it was just movement, often vague and unlocalized, around some part of the total figures. Very seldom was there a report of a 'trembling or shaking' of one line. This sort of movement generally occurred in dual form.

Concluding our discussion of the first 4 Series with the angle and the horizontals, we shall have to consider for a moment the apprehension of FM under conditions in which phenomenal simultaneity seemed to predominate. According to Tables I and II, full movement was reported in a large number of cases with intervals of 15, 30, and 45 σ. But the Tables fail to show that, as a rule, the figures were *seen together* in the form of an angle or parallel lines, but with movement; and often the movement of a line, although the line might not always be seen in intermediate positions. It is to be noted that some *O*s reported most insistent and pronounced movement where there was simultaneity or approximate simultaneity. In the light of former investigations upon the visual apprehension of movement this fact is of peculiar importance[23]. Its significance will be considered when we come to our general discussion. At present, we wish merely to call attention to its general character as given in a few of our own reports.

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[23] Professor Koffka, who has done much fruitful work in this field of perception, visited the laboratory during the course of the experiments. Upon observing for us, he was immediately impressed by this form of movement. He suggested that since it had never before been observed, it should be distinguished in some way from the other types of movement previously described by Wertheimer and by his own pupils.

"Very strange, indeed. I saw an angle almost whole, but the upper line moved down to the lower." (Professor Koffka).

"Two lines together. The upper disappeared followed immediately by the lower. There was a weak yellowish light between the lines. I saw very definite movement downward of the oblique as it swung to the horizontal position" (Y, 1).

"A photographic representation of an acute angle with just a smudge of light yellowish light of low intensity at the innermost corner of the angle. The diagonal swept rapidly and smoothly to the horizontal" (S, 1).

"Two lines clearly given forming an angle. The field between dark. Very fast and continuous movement from upper to lower position. The whole experience is highly unified" (C, 3).

"Two lines at an angle. The lower line dim and narrow. The upper clear and wider. Field between clear. There was definite movement from below upward over the whole field of the horizontal line" (Z, 1).

"An angular line with a horizontal below it. The field was a little hazy around and between the lines. I saw very rapid movement of a line from one position to the other" (Y, 2).

"Two lines together forming an angle of about 45°. Very nice movement (just as if a fan opened up). I saw a line swing up over the field. The field between was qualitatively unlike the background. A weak light surrounded the lines and filled the angle" (K, 1).

Throughout Series I-IV the whole period, including the exposition times and the interval, was extremely brief. In some cases the entire presentation occupied little more that 1/10 second (115 σ). The longest time was only 325 σ. Under these conditions accurate apprehension was so difficult that the reports were often made with considerable difficulty. Even with the shortest times, however, two lines or rather two positions were always apprehended. Along with these lines the intermediate field might be partially or wholly filled with a 'hazy,' or 'filmy' light. But the field might also be dark, with just as insistent an impression of movement as where it was qualitatively different from the background. With the very short intervals the perception of movement appeared to come almost before the lines were distinctly apprehended, or else the lines were given with movement just at disappearance. Aside from the contribution to the perceptual performance of kinaesthetic factors, we believe that the slight sequence that must necessarily have occurred in the presentation of the figures accounts for the apparent perception of movement before the lines were clearly apprehended. Thus *Os* sometimes spoke of movement after which the whole figure stood out clearly; or movement occurred and was followed by a faint 'after-image' of two stationary lines. The movement that came simultaneously with the disappearance of the figures was due partly, as we surmise, to the fact that the first, being presented a little earlier than the second, disappeared a few sigma before the second; but owing to the extreme rapidity of the whole presentation these slight differences were unobserved. The effect upon visual apprehension, however, was important. It meant that the first line did not simply disappear, but that it moved down to the position of the second member, going out by way of this member. Assuming this to be true, we sought a more satisfactory method of testing this whole matter of the apprehension of movement under conditions in which the disappearance of one object was by way of the remaining, or in which the appearance of a second object was by way of the first. To this end we proceeded with the method and the conditions of Series V and VI (already described) where observations could be made with a greater degree of assurance and accuracy.

Series V; addition of one member to another (*Objects A and B*). In this phase of the experiment we sought to determine the nature of visual movement in connection with the addition of one figure to another. No preliminary series were given. The *O*s were unacquainted with the setting. There was no interval. *O* was presented with one figure and then the other was instantaneously added to it while the first remained. The results are given in Table III.

TABLE III
Series V; Addition

Order	*Object A*						*Object B*					
	a——*b*			*b*——*a*			*a*——*b*			*b*——*a*		
O	O	F	P	O	F	P	O	F	P	O	F	P
G	1	4		1	4			5			5	
C		5			5			5			5	
Y		3	2		5			2	3		1	4
Z		5		2	3			5			4	1
R		5		1	4			5			5	
S		5			5			5			5	
Hn		4	1		5			5			5	
K		5			5			5			1	4
H		5			5			5			5	
M		1	4		5			5			1	4
	1	42	7	4	46			47	3		37	13

Here we find but 5 cases of 'no-movement' reported out of 200 observations. Most of the *O*s apprehended FM in all cases. There were no reports of dual-movement or of movement within a member, and only a small number of partial-movements occurred. The conditions in this and the following series were almost ideally suited for the perception of visual movement. Several *O*s commented on the ease with which observation could be made in this as contrasted with the former series, and on the insistence and clarity of the movement. The outcome was not wholly unexpected; yet there was an element of surprise. There is little in the literature on visual movement to lead one to assume that the mere addition of one member to another, to give a particular object-configuration, would be accompanied by experiences such as described in the following reports.

"I saw an oblique line on dark background. Then suddenly a bright line separated from the oblique and 'fell' down to a horizontal position forming an angle. The angle persisted for a moment and disappeared. The field was clear between the lines" (Z, 1).

"There was a horizontal line on a black field. Then movement downward. Then another lower horizontal line. Then upper line reappeared. It was not there during movement. Then both lines disappeared. Nothing visual here in the field between the lines to give me this movement" (Y, 3).

"A horizontal line appeared. Then a line suddenly detached itself from this line and moved up to an oblique position. Then I saw an angle which soon disappeared. There was nothing visual other than this" (S, 1).

"An oblique line appeared. Then it dropped rapidly and smoothly to a horizontal position. Then I saw an acute angle which disappeared. There was nothing more visually" (K, 1).

"I saw an oblique line on dark ground. It then moved all the way down to a horizontal position, but strangely enough when I saw the line

> in the horizontal position I again saw the oblique forming an angle with the horizontal. (It struck me as odd that the oblique could get down and then back again so quickly and without my seeing it go back)" (R, 1).

We have here pronounced FM without any sequence of objects in the physical sense of one line appearing and disappearing, to be followed by another similar object, which in turn disappears. Yet under these new conditions we obtain approximately the same kind of movement as we secured under the method of successive presentations. We cannot say 'exactly' the same kind because movement was clearer under these conditions, with particular emphasis upon FM. This movement of a line from the position of the first to form another line is exactly what we obtain under the usual method of stimulation where one line is followed by another which appears to come from the region of the first and to move to its final position. This series sheds considerable light upon the movement that is reported as coming before the objects are clearly seen and also of the movement that has been so often reported in some such way as the following; "A line appeared and then another moved from it to form a second line."

Certain reports in connection with Series V are very interesting from a theoretical point of view. I give the following reports from Y.

> "A reddish-yellow line appeared on a black ground. Then a bluish-green line appeared just above it when it disappeared and moved quickly to an upper position. Then two yellowish lines stood out for a second and then disappeared." "A yellowish horizontal line appeared on a black field. Then a low chroma bluish-green line started just above it and went on up to its final position. Both lines then disappeared."

The implication appears clear to us. It means that here an instantaneous shift upward occurred in the point of fixation at the time the figure was added and during this change the negative after-image of the first line appeared. This change in fixation also might serve to account for the experience of visual movement. As we know, there would be sufficient time for this shift to occur, as Mach proved with his work under instantaneous exposition of adjacent objects.[24]

Series VI; subtraction (Objects A and B). Here one member was subtracted from the total configuration A or B. The quantitative results are summarized in Table IV. An inspection of them shows the very close similarity to those of the preceding Series (Table III). Under the new conditions the *O*s did not, curiously enough, see a line disappear into the blackness of the background, or merely fade out; they saw it go out by way of the remaining member. It is either a case of no-movement, full-movement or partial-movement.

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[24] Mach exposed instantaneously two small squares of colored paper (2 x 2 cm.), 8 cm. apart with the instruction to attend to the right. The *O*s reported that the square directly attended to was red, but that the other was greenish. It was pointed out that a shift in attention from the one to the other might account for this difference in hue, although the whole period of exposure was very short (that of the electric spark). Mach, E. *Beiträge zur Analyse der Empfindungen*, 1906, 205-6.

"Both lines given in the form of an angle. The oblique seemed to fall into the horizontal. There is some change in the field but I am unable to describe it" (G, 3).

"Two parallel lines on black field. Then the upper line suddenly moved downward to the lower. Nothing given visually to give me this movement so far as I am able to observe. The line just moved" (S, 1).

"I saw two horizontal lines with a gauzy light between them. The lower then moved up to the top line and was gone. It seemed to 'swoop' up the film of light as it moved up" (C, 1).

"I saw an angle and then the oblique 'clapped' down to the horizontal. Very good clear movement. The upper line just moved down and closed up. Nothing between the lines filling the field" (Z, 1).

"Two horizontal lines with a faint grayish haze between them. Then there was a peculiar dropping of the upper horizontal line to the lower in a quick smooth manner" (K, 1).

Table IV

Series VI; Subtraction

	Object A						*Object B*					
Order	ab——a			ab——b			ab——a			ab——b		
O	O	F	P	O	F	P	O	F	P	O	F	P
G		3	2	1	4		3	2		2	3	
Z		5			5			5			5	
Y		2	3		5			5			5	
C		5			5			5			5	
S		5			5			5			5	
K		1	4		5			3	2			5
Hn		3	2		5			5			5	
M			5			5		5			5	
R		5		2	3			5			1	4
H		5			5			5			5	
		34	16	3	42	5	3	45	2	2	39	9

Summary of Series V and VI. So far as we are aware visual movement under the precise conditions outlined in Series V and VI has never been reported or described. Under all previous conditions objects have been presented in sequence with well-known results. Here the conditions were different. *O* had an opportunity to attend fully to one member of an object while the other was being added, or to a whole figure while one member was removed. There was no temporal interval between the addition or the subtraction of members. The whole procedure was relatively slow, except for the sudden appearance or disappearance of the member.

Possibly this was the reason why the *O*s expressed greater satisfaction in observing. There were no reports of uncertainty and no need of repetition. In Series I-IV the *O*s often complained of the extreme rapidity of exposition and the general feeling of uncertainty concerning the accuracy of report. In these series there was neither objection nor uncertainty.

The apprehension of movement under the above conditions is significant. The change in the hue of the lines, together with reports upon the momentary disappearance of the first object during the apprehension of movement, throws further light upon the functional resources of the organism utilized in the perception of movement. We hesitate to advance a theory or to develop explanations. We recall the reluctance of Exner, Wertheimer, Koffka and others to explain visual movement by any reference to ocular shifts. Yet it does appear from our work with the addition and subtraction of members that very clear and insistent visual movement might arise from rapid eye-movements.[25] This would account for the fact that one line disappeared for a short time when movement occurred. It would also explain, as we said, the qualitative change in the line of light. Furthermore, it would account for the failure of the oblique and the horizontal to meet at the proper point. Finally, it would explain the apprehension of the line as moving beyond, and returning to, its true or final position. If this interpretation is correct, it means a great deal to current theories of visual movement. Our conditions in these two Series (V and VI) are in effect similar to these involved in the usual method of successive presentations, but they permit us to ascertain with greater clarity the character of the visual movements.

Series VII; 0 σ interval (Objects A and B). Earlier in this paper was pointed out Hillebrand's claim that movement can be obtained where the interval is less than Wertheimer believed necessary for the apprehension of movement. In the present series we sought to determine the character of perceived movement (if it existed) with 0 σ interval and successive members.[26] The quantitative results are given in Table V. Here for the first time we fail to find any report of 'no-movement.'

TABLE V
Series VII; 0 σ Interval

	Object A				*Object B*			
Order	a——b		b——a		a——b		b——a	
O	F	P	F	P	F	P	F	P
G	4	1	3	2	5		5	
K	5		5		5		5	
M	5		4	1	5		5	
Y	5		5		5		5	
Hn	5		5		5		4	1
C	3	2	1	4	5		3	2
S	5		5		5		5	
Z	5		5		5		5	
R	5		5		4	1	5	
H	5		5		5		5	
	47	3	43	7	49	1	47	3

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[25] R. Dodge, Participation of Eye-Movement in the Visual Perception of Motion. *Psych. Rev.*, 11, 1904, 1-14.

[26] Here we used the same sort of cards and drawn figures as Dimmick employed.

Only full- and partial-movements were observed. The first sort predominates; 9/10 of all reports indicate FM. Moreover, the experience was just as insistent and as clearly apprehended as under the intervals employed in Series I-IV.

Hillebrand is the only person, unless we may except Benussi, who has heretofore reported full-movement under 0 σ interval. Wertheimer, as we know, found that simultaneity without movement occurred as the interval approached 0 σ. Intervals of about 30 σ could be counted upon to give resting figures or, at least, with slight unrest only in each member. Partly to account for this alleged fact he desired his cortical theory. We do not wish now to discuss the significance of our results in the light of the current theories; that is reserved for our general discussion. We wish merely to present at this time a few reports with 0 σ interval to be considered in connection with what we have just said about the significance for Wertheimer of the temporal interval.

"I saw a yellowish field with a dark grey oblique line on it. The field suddenly became much brighter and there was a horizontal line in the field instead of the oblique. I saw a single line move rapidly from an upper to a lower position. There was nothing between the two positions visually to give me this movement" (Y).

"An upper diagonal line moved down, or rather swept down to a lower position. The field at the close was brighter than at the beginning" (C).

"A horizontal line appeared. Then, after a moment, there was rapid upward movement referred to the region above it and to the horizontal line. The line stopped in an oblique position and then disappeared. At the moment that movement occurred the field grew much brighter. There was nothing between the lines of a visual character" (G).

"The only thing I can say is that the oblique 'clapped' down to the horizontal" (Z).

"A horizontal line appeared. Suddenly it moved up to a diagonal position and then disappeared. Along with the movement the whole field was much brighter. There was nothing visual to give me this movement across the field, *i.e.*, there was no particular visual filling between the lines. The whole rectangular area was lighted" (S).

"A black horizontal line on a dim yellowish field. Then a sudden increase in the intensity and I saw a horizontal line above the former position of the first. Just at the moment that the field brightened, I saw the lower line rise smoothly up to its final position (2 cm.) above. There was nothing in the visual field between the lines to give this" (Y).

Series VIII; continued exposition (*Object A*). It has been claimed by various investigators that the best visual movement is often obtainable under recurrent stimulation with the same object-pattern. Individuals who were unable to perceive movement under single expositions readily saw full-movement under repeated exposures. To ascertain the nature of movement under these conditions we conducted the present series.

For the 0 σ interval we used the same method as in Series VII, except that the weaker light was not removed.[27]

After the proper signals the control key was pressed and released at 1 sec. intervals, giving in this fashion 5 expositions of each figure. For the remaining 10 intervals the slitted cards were placed at P_1 and P_2, the disc set to give the desired interval, the motor started and the control key pressed while the disc rotated 5 times.

The results are summarized in Table VI. Here, as in the previous series, we find FM predominating. In fact, in no case was there an absence of it. It is quite evident that under otherwise similar conditions the effect of repeated exposures of a stimulus-pattern tends to make for very clear perception of movement. In the preceding Series, the *O*s reported two lines. One appeared and moved up or down to a position where it could be seen as a resting line or a line in a given position, or both lines were seen with movement between them or in connection with one. But in the present series, the meaning was, as a rule, slightly different. More often it meant that a single line moved up and down, or a line recurrently appeared and

TABLE VI
Series VIII; with Object A

	0	1	2	3	4	5	6	7	8	9	10
O	0	15	30	45	60	75	90	120	150	180	225
	F D	F D	F D	F D	F D	F D	F D	F D	F D	F D	F D
S	3–	3–	3–	3–	2– 1	2 1	2– 1	3–	3	3	3
Z	3	3–	3–	3–	3–	3–	3–	2– 1	2 1–	3	3
Hn	3	3–	3–	3–	3–	3–	3–	3	3	3	3
C	3	3–3	3–	3–	3–	3–2	3–	3–	3	3	3
R	3	3–3	3–3	3–	3–	3–	3–	3	3	3	3
Y	3	3–	3–	3–	3–	3–	3–	3–	3–	3–	3–
G	3	3–	3–	3–	3–	3–	3–	3	3	3	3
H	3	3–	3–	3–	3–	3–	2– 1	3–	3	3	3
K	3	3–	3–2	3–	3–	2– 1	3	3	3	3	3
M	3	3–3	3–3	3–	3–	3–	3	3	3	3	3

– represents movement in one direction. All others indicate movement in both directions.

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[27] This work with a stronger and weaker light had an additional value. It might have been objected that the increase in the intensity of the second light (when we were working with either single or repeated exposures) might serve to facilitate the course of the *phi-function*. Here we found that the movement given by the disappearance of the strong light with the instantaneous appearance of the weak figure was just as clear and as insistent as when the strong light wiped out the weak.

moved over and out of the field. The two lines, as end-positions, were less clearly perceived. The whole experience appeared to be highly integrated, approximating, at times, the movement of a physical body.

With these continuous repetitions we had an opportunity to compare visual movement with relatively long and short intervals. Always there were two intervals, a long (C) and a short (D).

When the radial sectors were close together C was quite long, as much as 885 σ; When the sectors were most widely separated C was about 675 σ. While we did not deviate from our regularly used intervals to test others, this method seems to us the best way of investigating the relative importance of various intervals for visual movement.

In any other method, the time between presentations due to the necessary shift in apparatus is too long for reliable comparisons. Aside from the introduction of the effect due to repetition this is, as we find, the safest and the easiest way of securing a large number of reliable judgments for a comparative study of movement, especially where the primary object is the determination of the interval best suited for movement. This form of stimulation also furnished us with a more satisfactory method of ascertaining the nature of the phenomenal bases of movement. *O*s were able to see the same thing occur 5 times in rapid sequence. In this way they could verify certain phases unclearly perceived under previous expositions.

We have seen that, in the preceding Series, the *O*s apprehended FM (*1*) where only two qualitatively similar lines were present; (*2*) where the two lines were present with the second intensified; (*3*) where the intermediate field was partially or wholly filled with a subdued light; and finally, (*4*) where a varying number of intermediate lines or positions were observed. This phase of the experiment gave us a chance to verify these reports and to note further visual and non-visual foundations for movement. It is understood that we recognize the danger inherent in any attempt to compare two sets of results obtained under dissimilar conditions. Since, however, in this series we kept everything present in the proceeding and added merely the recurrence of the same pattern, the results which we obtained may safely be used toward a further understanding of our earlier experiments. We present for inspection reports taken from this series.

"Very good movement up and down of a single line. The upward movement was quick and light. There was nothing in the visual field. In the downward movement there was the step-wise effect. The movement seems more deliberate and more ponderous, because of the number of intermediate positions. The movement was not wholly smooth" (C, 8).

"Very good downward movement of a line with less insistent upward movement. I do apprehend the line going upward; although I am unable to see anything in the field. Upward movement is much faster than downward. The line see seems literally to 'snap' up and then 'flutter', *i.e.*, there are distinct positions assumed by the line during downward movement but nothing in upward movement" (K, 7).

"A line was seen to move downward 5 times over an angular space of about 45°. This happened 5 times. There was no upward movement. During part of the time that I saw movement there was a yellowish haze over a small part of the area between the upper and lower limits of movement; the rest of the time there was nothing" (Y, 6).

"A line moved fairly slowly up and down over an angle of about 45°. The whole angle was dimly lighted. The downward movement is clearer than upward; it was almost as if I did not see the upward movement; yet I am positive that I did" (S, 9).

"A diagonal line moved rapidly up and down. There was a dark figure between the upper and lower limits. This figure was wide at the top and bottom and narrow in the middle. It was darker than the surrounding area" (S, 5).

"A single line moved from the horizontal to the oblique position and back again. A very clear 'clapping' down and up. All clear and insistent. Nothing in the field between the upper and lower positions so far as I could see" (Z, 10).

"Very rapid and obvious movement up and down of a single line over an angular distance of about 45°. I did not see anything in the area intermediate to the upper and lower limits" (C, 0).

Here we find a difference in the character of movement emphasized where there is some slight observable difference in the phenomenal basis. Light is thrown upon the movement where first one line and then another is apprehended. In all the cases in Series VIII, except those with 0 σ interval, the time between the exposition of the second member of the preceding and the first of the following pattern was relatively great, yet upward movement was seen at some intervals by all *O*s except Y. In the majority of cases the movement upward (interval C) was unlike that downward (D). We pointed out earlier that where the interval was longer and nothing was seen in the intermediate field the movement assumed a 'quicker', a 'lighter', or a 'floating' character. We find this to be true of interval C in a great number of cases in the present Series. The line moves without pause or jerk from one region to another. In interval D frequently appeared intermediate positions, and the movement was more irregular, more jerky and usually slower. Pronounced movement of a smoother and more uniform nature was found where the interval was filled with a dim light which gave *O* the impression that something had passed over the field leaving a haze. It did not mean necessarily that a line was seen to move through this haze. The filling served merely to reënforce the impression of movement and to give a greater feeling of assurance. This seems to be the common effect upon the movement experiences of such visually filled space.

When there was little or nothing in the field intermediate to the lines, the movement could be just as clean-cut and as impressive as under the above described conditions. Only the movement was quicker, smoother and lighter. Other resources than visual were—as it appears—involved. *Os* spoke of diffuse pressures around the eyes, tongue and throat, and also of subvocal speech. In speech there was the same tendency as before to accent one member: *ta-ta, there-there, up-down, down-up.* Moreover, we must not underrate the significance of rhythmic tendencies. *Os* reported tensions in the head, hands and feet, meaning incipient movements of these members. Further proof of this rhythm is given in those reports where movement was better in the last 2 or 3 of each group of 5 distinct exposures. Before the end of each group was reached, the *O* appeared to be aided in apprehending movement by falling into the rhythm of presentation. Another fact brought out sharply by Table VI furnished additional proof of the significance of rhythm. It is to be noted that the smaller D-intervals (aside from 0 interval, where the presentation was different) there was full-movement in one direction only. Every *O* took two expositions as a unitary whole separated from the next two by an interval of blackness. But when the difference between the C and D intervals was lessened, the reports were modified, indicating a linking-up of the 5 double expositions into a fairly unitary sequence with the meaning of movement up and down; and the verbal and somatic rhythms appeared to be more easily conditioned by sequence in which the intervals were more nearly equal in length, so that when the D-intervals were lengthened and the C-intervals proportionately shortened, the rhythm was more insistent. This factor of rhythm is indicated in connection with 0 interval. Here there was succession with equal intervals as well as equal exposures. One object was given for 1 sec. and then the other for a similar time. This was repeated 5 times. *Os* were seldom able to report anything in the intermediate field; but FM of an insistent character was reported in all cases. Z stated that this was the best of all observed movement. We find that the two sets of facts obtained from widely separated intervals (0 σ to 225σ and upward) point to the same thing, the utilization by the organism of resources, in addition to the purely visual, to condition clear movement of a visual character.

It is not to be assumed that the relevance of these 'supplementary' factors in perception was always clear to *O*. It is fully recognized that many clearly observable kinaesthetic factors often occur, not as essential constituents in the perceptual incorporation, but as chance concomitants of perceptual performance. Since the *Os* so often experienced a decided lack of all clear-cut visual factors for the movement-perception, and since they repeatedly mention kinaesthetic factors, we seem justified, however, in accepting the possibility of coöperation of these somatic items in the apprehension of movement.

Series VIII; with Object B. There is no outstanding difference between our present results (Table VII) and those with A (Table VI). There were, to be sure, more cases of dual-movement, but most of the cases belong to 2 *O*s (G and M) and the number is not large enough to be significant. In all cases save one, 0 interval gave full-movement in both directions.

TABLE VII

Series VIII; with Object B

O	0 0 F D	1 15 F D	2 30 F D	3 45 F D	4 60 F D	5 75 F D	6 90 F D	7 120 F D	8 150 F D	9 180 F D	10 225 σ F D
Y	2 1–	3–	3–	3–	3–	3–	3–	3–	3–	3–	3–
C	3	3–3	3–3	3–3	2 1–	2– 1	3–	3	3	3	3
S	3	3–	3–	3–	3–	3–	3–	1 2–	2 1–	2 1–	3
H	3	3–	3–	3–	3–	2 1–	2 1–	3–	3	3	3
K	3	3–	3	3	3	3	3	3	3	3	3
Hn	3	3–	3–	3–	3–	3–	3–	3	3	3	3
R	3	3–3	3–3	3–3	3–	3–	3	3	3	3	3
G	3	3–2	3–	3–	3–1	3–	1 2–	2–1 1	3–	3–	3
Z	3	3–	3–1	3–	3–	3–	3–	3	2 1–	3	2 1–
M	3	3–3	3–3	3–3	3	3	3	3	3	3	3

– represents movement in one direction. All others indicate movement in both directions.

"Rapid movement of a line (dark on white expanse) down and back again" (Y). "The black line moves up and down" (Z). "Very obvious movement up and down of a dark grey line" (C). "A dark horizontal line moves up and down" (S). "A greyish horizontal moves up and down rather rapidly" (K).

The smaller D-intervals, with 1 or 2 exceptions gave FM in one direction. Along with these reports there were a few on dual-movement. *O* stated that one line moved away as the other approached, or a line moved down and then part way back.

"Upper line moved down to the lower in two steps (two intermediate positions). Then lower seemed to move slightly upward, but am unable to state anything about the visual field when this took place" (C, 3). "The upper horizontal swings down over intervening space to lower and then lower moves down slightly" (C, 1).

"Two almost simultaneous streaks. Downward movement of the first to the second. Then the lower moved slightly down, too" (G, 4).

"Two parallel lines moving. The upper when it reached the bottom, again appears at the top and moves down, as it does, the second moves down a little" (M, 3).

The longer D-intervals gave FM either one way or both. On the whole there was nothing new by way of qualitative variety in these longer intervals. The *Os* reported visual resources supplemented by rhythm, verbal kinaesthesis, ocular pressures and subvocal speech, all of which combine to give FM.

"A series of apparent units. Same thing over and over. A path of yellowish light filled a small part of the field between the upper and lower positions. I saw a line move downward several times. There was a definite rhythm in the movement referred to weak pressures around the eyes and head. Movement was smooth and recurrent" (Y, 10); "Movement was jerky and in units. I saw three definite intermediate positions of the line as it moved downward" (Y, 9); "Movement downward was irregular. A slight pause at each of three fairly bright intermediate lines" (Y, 8).

"A decided dipping down and going up of a single line. Movement down was not smooth. I saw several distinct phases of the same line. The movement back was just a quick jump without anything between" (Z, 9).

"A horizontal line which moved down and disappeared. This was repeated several times. There was nothing in the field between the two positions so far as I am able to state" (S, 7).

"A faint horizontal line moved up and down. I saw it move down jerkily and then go up very smoothly and quickly. This happened several times. I saw the line in several positions going down but nothing as it went up" (K, 9).

"Distinct impression of a slow movement downward and a quick movement up. There were diffuse pressures around the eyes" (G, 7).

"Very easy movement, slightly irregular, in a downward direction; then smooth movement up. I could follow the line with my eyes as it went down. I saw it at several places. I am not sure of anything in the upward movement except that the line moved up" (R, 9).

IV. DISCUSSION OF RESULTS

Several studies of visually apprehended movement have been conducted within the past few years. Of these the two which stand out as most significant, both for the general theory of stroboscopic perception and for our own particular study, are the investigations of Wertheimer and Dimmick. From Wertheimer is derived in hypothetical form an interesting explanation of visual movement; an explanation which Dimmick rejects, substituting in its stead one more in harmony with the traditions of descriptive psychology.

With Exner and others, Wertheimer contends that the experience of visual movement is just as simple and as elementary as the sensations of the analyst; that movement cannot be resolved into 'simpler' constituents and still remain movement; that it is an experience which can and often does exist in and of itself. It is just movement, simple and irreducible; movement without qualitative or spatial characterizations. It may appear, it is true, along with some object; but under proper objective conditions the movement may also be completely divorced from the object and so become 'pure-phi' or 'pure movement'. For him the best movement is the 'fullest' movement. It appears near 60 σ and grows progressively poorer as one passes temporally in either direction from this optimal interval. At 30 and 200 σ FM is not to be apprehended. His explanation, as we have seen, is neural. The simple and unitary experience of 'full' movement depends upon a simple and indivisible cortical process effected between two adjacently excited cortical regions. This is best effected at 60 σ, but not at relatively short or at relatively long temporal intervals. When this cerebral process occurs, movement appears without 'qualitative' aspects.[28]

Dimmick interpreted his results as standing against a movement of non-qualitative character; *i.e.*, movement depending solely upon central conditions and wanting a phenomenal basis in vision. He sought to show, instead, that visual movement may be closely correlated with a definite experiential factor, namely, a grey of a certain, fairly brief, durational course. A significant connection exists, then, as Dimmick thinks, between a particular interval and a particular visual configuration whereby the apprehension of a particular type of movement is conditioned. With a shift in the temporal interval there goes a change in the character of the primary integration of quality and duration. As Dimmick puts it "the integration . . . . is directly determined by the temporal interval between the exposures of the two stimulus-members" (p. 332).[29] Upon this basis was formulated an explanation of the change in the nature of visual movement under slight shifts from the 'optimal' interval. This explanation is hard to follow and to verify because Dimmick does not give the intervals at which his bimembral and his unimembral movements occurred. Since Dimmick found the shift in the 'process' configuration to be directly dependent upon the temporal interval (see his conclusions) and since any shift in the process pattern or the temporal interval meant, as he thought, a corresponding change in the type of perceived movement, it is strange that he does not cite figures or other testimony to the alleged change of optimal over into unimembral and bimembral movement as he passes temporally in either direction from the optimal interval. At no place does he show the number of cases of unimembral, bimembral and intramembral movements at various temporal intervals, *i.e.*, the relation between these movements and the time variable. It would have been illuminating to know the number of such movements actually apprehended in connection with every one of his intervals. One is led necessarily to assume from his descriptive account that these incomplete movements did not occur except around the shorter or the longer times. So much, at least, is intimated in the statement that "our intervals lie between those which give succession and simultaneity" (p. 322).

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[28] From this simple beginning, the doctrine has been widely extended through the works of Koffka and Köhler to include also many inanimate and inorganic events. (*Cf.*, H. Helson, The Psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62.

[29] *Cf.* also F. L. Dimmick and G. H. Scahill, Visual Perception of Movement, this JOURNAL, 36, 1925, 412-417.

We have now to consider our results in the light of these two investigations. We cannot hope to compare in all details the three experiments, since our stimulus conditions and our instructions were somewhere dissimilar. It appears, nevertheless, that the results are not incomparable, inasmuch as we all obtained, under the same setting, full-movement, dual-movement, partial-movement and movement within a member; in short, all the types observed. We are justified, therefore, in a general discussion and comparison of all three investigations with respect to their common elements.

Since the correlation of a particular type of movement with a particular temporal interval appears as one of the most significant facts for both Wertheimer and Dimmick, we shall deal first with that matter. We have pointed out in our own experiments that, at no time, could we say that one, rather than another, temporal interval conditioned FM. We have seen that FM was as clearly apprehended at the extreme intervals as at 60 or 90 σ. This was an outstanding fact in all of our experiments. *O*s commented on the fact that the observations were often so essentially alike that they might, at times, have characterized a whole series (*i.e.*, 20 observations at all 10 intervals) by a simple brief description. As over against this wide extension, however, we have the generalizations of Wertheimer and Dimmick concerning the optimal interval. As widely as the latter departs from the former with respect to the experiential character of visual movement, there is, nevertheless, an agreement between them that FM does generally require a particular and definite interval.

To what shall we attribute this striking disparity between our results and those of Wertheimer and Dimmick? It is difficult to say. Perhaps it is due to a difference in experimental setting and control. We have the statement from one of our *O*s who took part also in the Cornell study, that movement was decidedly clearer and more insistent under our conditions. It might well be, were it not for the facts of Series VII, that the elimination of everything in the visual field but two bright lines on a lightless ground is more conducive to the experience of visual movement than black lines on a moderately large white expanse. In Series VII, however, where our cards of light grey with black lines were apprehended as a bright rectangle with dark figures, we were approximating the conditions of the Cornell study in all respects other than the sudden and necessary increase in the intensity of the illumination of the second figure to effect the removal of the first. To whatever the difference is due we must assert, in general, that no particular interval may be said to give FM as against another which does not. Moreover, it does not seem probable, in the light of what we know concerning the functional operation of the organism, that visual movement should wholly depend upon any particular interval. Rather it seems to us that the human organism should be able to arrive at the same functional outcome under fairly dissimilar extra-organic conditions. Everyone knows the wide variety of conditions (visual, auditory, dermal, kinaesthetic, the displacement of objects, of the eyes, or of the body, etc.) under which the perception of movement arises. With a scarcity of one kind of resource, under one set of objective conditions, other means at hand will be utilized for this perceptual performance.

Not only are we unable to set up a significant general parallelism between temporal interval and full-movement; we find that FM may also occur under phenomenal simultaneity. This fact seems highly significant for Wertheimer's original theory. It means that, under our conditions, the shorter intervals do not give a 'resting' simultaneity of the two members but, instead, a very clear and insistent movement. This was observed, as we have said, by all *O*s including Professor Koffka. It cannot be attributed to, or explained by, an increase in the length of either exposition-time; since the same exposures which gave full-movement at 225 and 60 σ gave FM also at 15 σ. This explanation, as we know, has been previously offered; but since our exposition-times were kept constant in Series I-IV (with both A and B) it is not applicable to our facts. One wonders whether Wertheimer would have postulated his doctrine had he observed FM under simultaneity. We are unable to see how he could consistently have developed his central theory as he did, had his facts been a little different. Our results here, as we find, throw a shadow of doubt upon one of the most fundamental and basic assumptions involved in the *Gestalt-Theorie*, in so far as that doctrine assumes a spatially and temporally extended cortical process occurring under certain extra-organic conditions, the most important being the temporal interval necessary for the *phi-function*. Our facts seem sufficient to warrant generalization on this point. We have a total of 3660 reports. Three phases (V, VI, and VII) of this experiment deal with 0 intervals. In addition there are the times of 15, 30, and 45 σ of Series I-IV. In the light of these observations we believe that the assumption of the *phi-function* as running its course under a certain necessary temporal interval is invalid and does not stand experimental test. A modification of the physiological explanation of visual movement as postulated in the doctrine of the *Gestalt*, appears, then, to be called for.

Wertheimer's descriptive facts are not, however, without value. While the essential features of his work had beensented by earlier investigators, his experiments contributed significant material. We find that our own results in Series I-IV (Objects A and B) tend largely to substantiate his account of the various forms of movement. His *O*s reported rapid and slow, smooth and irregular movement from one position to another. His statements of dual-movement are in no way essentially different from ours; and the same is true of the partial-movement of a single member. Since we had few cases of movement within a member, we have less foundation for comparison there; but no striking difference appears in those which we do have. In one very important respect our results stand in close and significant agreement with those of Wertheimer. Very often our *O*s reported clear movement in connection with some part of the total configuration—around or between the members,—although they were quite unable to characterize this movement in any more precise manner. For Wertheimer it was just movement without quality and without a definite spatial character. We find the same in our own observations. Prior to our experiment we were unable to realize how visual movement could exist without a phenomenal visual basis being clearly evident. It was not long before we realized that the *O*s were apparently experiencing this very thing and finding themselves unable to give a clear account of it. Often the two members were present, with a clear impression of movement between. At other times, the lines appeared in succession but with the impression that something had moved about or over the total figure. We must agree, then, with Wertheimer that, at times, the experience of 'pure' movement taken in this sense may occur. We further agree with Wertheimer that this sort of movement occurs in conjunction with the shorter rather than with the longer intervals. In substantiating Wertheimer's description we find ourselves in general disagreement with the explanation and description of visual movement as offered by Dimmick. As we saw, Dimmick rejected Wertheimer's explanation. He found, as he thought, that visual movement had an 'existential' correlate. Where there was optimal movement, for example, the scene of the movement was—for him—filled with a "visual grey" which was itself wholly unmoving. Where there was bimembral movement the "process" accrued to each member, but not to the field as a whole. Finally, where there was unimembral movement the "process" was connected with one member only. In case of intra-membral movement, the "process" was said to lie within the member,—a description which seems a trifle vague.

Now we have to consider whether Dimmick was wholly justified in rejecting Wertheimer's claim that visual movement does not have a specific qualitative visual character; and, furthermore, we have to scrutinize the evidence for Dimmick's existential aspect of visual movement. It must be admitted that, regarded from one point of view, it would be extremely gratifying to find a simple one-to-one correlation between visual movement and some simple invariable mental quality which in itself exhibits no movement.[30] That would settle the matter. But nature does not consult our wishes. In fact, the human organism is so sophisticated that simplicity is hardly to be expected in experiences much less complex than the apprehension of apparent movement under discrete retinal stimulations. Our *O*s, who observed without knowledge, reported insistent movement from 0 interval to 225 σ, time after time, without being able to report any relevant existential visual correlate between the lines. All experienced movement and described its rate, extent, regularity and continuity. When questioned upon the visual character of the interlinear field, they usually reported that there was nothing to distinguish that region from the general visual ground. The description implied then no significant visual relation to the experience of movement. Most of these *O*s were wholly uninformed about the current discussion of visual movement. Moreover, it is to be recalled that, of our *O*s, one served in the investigation at Cornell and was well acquainted with the meaning of the report made in terms of existential qualities. From this *O*, as well as from all the others, we have various reports of full-movement where the field was described as unfilled by any quality different from that of the background. To what shall we attribute this difference between the Cornell study and our own? Let us consider certain internal aspects of Dimmick's account. We must examine certain of his 'movement' reports which seem to us ambiguous. A different interpretation and classification might easily and properly be drawn from them. This ambiguity is, as we think, very important. The first 9 of the following reports are cited in the Cornell study as unimembral movement; the last 2 as bimembral. They are taken from pages 325-326.

(*1*) H. "An oblique came in and moved downward, and below it stood a horizontal line."
(*2*) S. "A line moved down smoothly, getting faster as it went, and then the end-position stood out, sharp and stationary."
(*3*) M. "A horizontal line came into the part of the field moving, then another appeared lower down and stationary."
(*4*) Tu. "First I saw a line which wavered but did not change position, and at the same time there was something moving down into a position below."
(*5*) Ta. "A line moved down, and as it moved another appeared lower down and stationary."
(*6*) M. "An oblique moved down toward the horizontal. The movement was jerky and definitely of the oblique, not of the horizontal."

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[30] "There is, then, no movement in the multiform grey flash; the space is constant; the integration is of time and quality" (p. 332).

(*7*) S. "I saw a line move down very rapidly through an arc. The first part of the movement was not clear, *i.e.*, it seemed to be part way down before I saw it; above was a blank."

(*8*) Ta. "An oblique line appeared, then a horizontal and the oblique moved down to the horizontal."

(*9*) Tu. "An oblique line appeared in the upper part of the field and moved down faster and faster until I could not see the movement. Then the horizontal came in."

(*10*) S. "An oblique line moved down to the horizontal, but there was a pause in the movement part-way down."

(*11*) Tu. "An oblique line moved down to the horizontal but not smoothly; there was a flash of good movement at the beginning and end."

According to (*1*), the movement might just as well be considered full-movement as unimembral. *O* reported two lines, one above the other, with movement between them. In (*2*) report we find even greater uncertainty. Was movement over the whole field? So we are to believe from the term "end-position" in connection with movement. Our uncertainty is further increased by the report of this *O* for *optimal* movement (p. 323). "A rectangle moved down smoothly and stopped for an instant. The speed of the line increased as it moved, and was very rapid just before it stopped." The two reports are very similar. Either the report for optimal movement is in the wrong place or the report given as an example of unimembral is questionable. An unbiased reader might easily consider the second instance above as full-movement. The report of (*3*) is much like that of (*1*). It is difficult to determine from the character of the report whether movement is unimembral or full. The same criticism holds true for the next case (*4*). There might have been full-movement of the second member with the first retaining its position. We had ourselves, as we have indicated, many cases of full-movement where the meaning was that the second line moved away from the first to its own final position. The report of Ta (*5*) is like that of H (*1*). In (*6*), movement might well have been over the entire field, so far as we may judge from the report. Case (*7*) appears as full-movement; since movement was from one position to another; *i.e.*, above the place where the movement started, the field was, according to *O*, a blank. This is what is meant by full-movement. Case (*8*) is the least ambiguous of all so far. What puzzles us is how or why Dimmick considered this to be partial movement of one line; when *O* said the oblique moved all the way across the field. In case (*9*) there is, again, an ambiguity. If the horizontal came at the lower end of the movement, then it is full movement; if it came below with a break between it and the movement, then it is partial; but as the report stands we wonder how Dimmick could determine the exact character of the experience. There is just as much ground for calling it full-movement as for claiming it to be unimembral. In (*10*), once more, we are wholly unable to understand why the report should have been called bimembral. Surely, a pause, as at a line, is not a break; and without a break in continuity of movement it could not be considered as bimembral. The same criticism holds for case (*11*), which might quite properly be considered as full-movement. *O* does not state the movement was broken;—it was simply not "smooth." But smoothness and continuity are surely not identical.

There are still more instances, in Dimmick's protocols, of the same general character; but these are sufficient to exemplify our contention that the reports are subject to an interpretation different from that placed upon them in the Cornell study. We took 9 cases of 16 given as illustrative of unimembral movement. One is forced to ask why, when there were, presumably, so many cases of true unimembral movement, 16 perfectly straightforward and unambiguous cases were not given. Or why movements which might very properly be considered as optimal should be cited as bimembral.

This suggests a further uncertainty. As we know, Dimmick stands for a close correlation between "process" configuration and type of movement. But when we turn to his reports we do not find the evidence clear-cut. I cite several "meaning" reports together with corresponding "process" reports taken from pages 325-328.

Meaning	*Process*
(*1*) M. "A horizontal line came into the part of the field moving, then another appeared lower down and stationary."	"First came a line in the upper part, then a grey, and another line below. The grey joined the lower line but was separated from the upper by a bright streak."
(*2*) S. "A line moved down smoothly, getting faster as it went, and then the end-position stood out, sharp and stationary."	"I saw a horizontal line, then lower down another with a grey patch above it."
(*3*) Ta. "An oblique line appeared, then a horizontal, and the oblique moved down to the horizontal."	"The oblique was grey on its lower side and blurred downward. There was a white space between it and the horizontal line."
(*4*) W. "An oblique line moved rapidly to the horizontal. The movement was best at the beginning, and just at the bottom there was a space of no movement at all."	"A distinct oblique line appeared at the top, then a horizontal, and between them a series of grey triangles, light at the top and dark at the bottom and joined on to the lower line."
(*5*) S. "An oblique line moved down to the horizontal, but there was a pause in the movement part-way down."	"First there appeared an oblique line, which was not clear at the lower edge but shaded off into grey. Below there was an interval of white, and at the bottom a horizontal which also lacked definition."
(*6*) C. "An oblique line moved down to the horizontal. The movement was not smooth; it disappeared in the middle so that there was a blank space."	"I saw an oblique line. It disappeared and the field was only white, then a horizontal appeared. Both lines were indistinct and blurred toward the interior of the angle."
(*7*) D. "I saw two lines, one oblique and one horizontal. The latter remained stationary, and the oblique moved down toward it, but was gone before reaching it."	"A sharply defined oblique line appeared and then a horizontal. When the horizontal came, the field just above it was greyish."
(*8*) H. "An oblique line came in and moved downward, and below it stood a horizontal line."	"First there was an oblique line with a patch of bright field just below it; then at the bottom a narrow sector of light grey, which was later replaced by a horizontal."

Consider these cases. We are led to assume that where there is movement there will likewise be the "psychological correlate" (D's visual grey); that some visual process "will be found closely paralleling the . . . perceptions" (p. 321). Where the field is filled, there will be full-movement; where the process accrues to one member, movement will be unimembral; and where the process involves both members, movement will be bimembral. It must be understood, of course, that, according to the method employed in the Cornell study, the two reports were not taken from the same identical exposition. Under one exposition *O* reported upon movement and under another exposition and another instruction, upon 'quality.' These reports are cited by Dimmick as representative of the alleged correlation.

In (*1*) we have unimembral movement. The process accrues to the *second* member; but the *first* moves. In (*2*), the grey patch is above the *lower* line; but the *upper* line (at least) moved. (*3*) appears to be *full-movement*, but with the grey accruing only to the *upper* line. The field is *filled* with "grey triangles" in (*4*); but only *partial* movement of one line was experienced. (*5*) might be *full-movement*, but with a *part* of the field *unfilled* with grey. In (*6*) there was *nothing* in the field of a phenomenal character save the two lines, yet there was *movement* of both. The grey accrued to the *horizontal* in (*7*); but instead the *oblique* moved. (*8*) is much the same; although the *horizontal* (not the oblique) had the accompanying grey, it was the *oblique* which moved.

Now our criticism is this. If Dimmick meant these reports of 'meaning' and 'process' to be taken together, we can understand his intent; but so taken they seem well-nigh meaningless. There is no significant parallelism between them. If he did not mean the two sorts to be considered together, why should he place *process* reports under such captions as *optimal, unimembral* and *bimembral* movement, in this way indicating them as illustrative of the process side of these specified perceptions? For he tells us that "the nature of the visual experience which filled the interval can be pointed out most clearly by direct reference to the introspections" (p. 323). How did he know, we must ask, that a particular process configuration which he placed under unimembral movement really belonged there rather than under some other head? Or why did he put down as the process-configuration for bimembral movement two lines each slightly blurred at one end? A more obvious illustration of this point is found in his account of pure *phi*. Both lines might be clearly perceived with movement between them; or neither line was perceived—only movement. In the first case, under the process attitude, *O*s reported the total configuration; in the second, merely the flash without the limiting lines. How was Dimmick to know one total configuration from another? Or what (unreported) difference existed on the side of process to give, on the one hand, optimal movement of a single line, and, on the other, bare but still optimal movement? To have designated certain process-reports as 'optimal', 'bimembral', or 'unimembral', *either* O noted and reported the movement and its precise character under process-instruction, *or* the character of the movement which might have been observed under the perceptive attitude is inferred in the light of Dimmick's hypothesis of parallelism from the process reports. The reader is not told which procedure was followed.

Dimmick offers, as additional proof of the parallelism, the fact that "in a number of reports the *O*s have stated that a certain movement had a particular process-aspect and conversely that . . .," (p. 328). If, under the process-instruction, *O*s generally observed only "process", there would seem to be neither logical nor experiential justification for claiming that the process configuration could possibly mean anything more than just what it was. It could scarcely be set up as the basis for a particular movement-experience. What the organism apprehends at one moment is not to be identified with its earlier and later experiences. To contend, *e.g.*, that, because S (Table I, page 321) observed optimal movement 72 times, unimembral 23 times, and bimembral 7 times out of 100 observations,[31] under movement-instruction; while, under process-instruction, the same *O* reported 1 sort of configuration 90 times, and 2 other kinds 1 time each, there is a significant and thoroughgoing connection between the two sets of observations is hardly logical. If we assume, however, that movement was apprehended just as clearly and as insistently under the process as under the perceptual instruction, the observations approach those of Wertheimer and of our own *O*s.[32]

The numerical discrepancies in the parallelism of the two sorts of reports should not be overlooked. The figures below were obtained from Dimmick's Table III (designed to show the influence of the temporal interval upon *optimal* movement with the horizontal lines) by taking the difference between P and M. The "plus" sign represents the percent. excess of process over meaning reports; and the "minus" sign, the excess of meaning over process. At 30 σ there were 103 more cases of full-move-

30 σ	60 σ	90 σ	120 σ	150 σ
+12	− 3	+ 2	+ 3	− 7
−25	+25	− 5	+35	− 5
+ 5	+15	+ 5	+ 5	+20
−25	−17	+15	0	+62
−23	− 2	+ 3	+53	+12
−20	+28	+27	+ 7	+15
+ 5	+ 5	+15	− 5	+18
−10	− 5	+ 7	+33	+ 8

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[31] The addition is Dimmick's, not mine!

[32] In fact, in a more recent study on Visual Movement, Dimmick uses only the *process*-instruction, this JOURNAL, 36, 1925, 412-417.

ment than of corresponding process-configuration (5 *O*s); at 90 σ 74 more cases of total configuration than of movement (7 *O*s); and at 150 σ the field was filled with grey 135 more times than movement was reported (6 *O*s).

Again, we may ask how Dimmick proposes to account for differences in rate of movement within the visual field under a given exposition? For example, his *O*s reported movement as increasing or decreasing in rate toward the end. If the field were filled *uniformly* with a grey (to condition full-movement) or *partially* (to give partial-movement), we should expect movement to be the same in character throughout the particular exposition unless the 'process' report indicated a precise phenomenal basis for the observed difference in rate. But an inspection of the actual (process) reports does not show, in a single case, any ground for this difference. The reader gains the impression that a plain but brief grey was being described; a grey such as is sometimes secured in tachistoscopic expositions. Only near the end of the study is the reader informed that the visual process was not, at any time, a "text-book grey." Beyond the figurative terms "flashing," "flickering," and so on, we have little or no description of the temporal aspect, which is presumably, for the author, as important as the qualitative.

What shall we say at large of the gray intermediate to the lines? In our own observations it is neither a constant phenomenon nor one necessary to the perception of movement. The intermediate filling is, when it occurs, one among several visual and non-visual resources which may or may not be utilized by the organism at a given moment in the apprehension of movement.

In the addition and the subtraction of figures, we described movement under conditions which seem to us to be new. Here the *O*s reported 'easy' and unimpeded movement over the entire field from one line to another. What light do these Series (V and VI) throw upon the problem of movement under the method of successive presentations with interval? There an object is apprehended, followed by a second coming out of the first; or else the first moves over to form the second. But here movement depends in part upon the apprehended relation between the members. If the lines are seen as unrelated or are perceptually unconnected, there is no movement. The members must be ligated to condition movement. In these Series, which we consider as an extension of the method of successive presentations, we sought to determine the principle of ligation.

How is the movement in these series to be explained? We indicated a possibility. Eye-movements certainly occur, with addition or subtraction of members, at the precise moment of visual movement. There is, moreover, the fact of the negatively colored lines moving across the field, as well as the momentary disappearance of the fully exposed line when movement is seen. There is, finally, the matter of the visual intermediates. In terms of eye-movements we can most adequately explain the apprehension of the lines as successive positions. In addition to these items there was, as we have indicated, a visual basis, at times, for the movement. When one line was added, the field might be partially or wholly filled by a dim light which spread out from the lines. In subtraction, this filling, when it was present, had to go. *O* occasionally mentioned that the disappearance of the line was accompanied by a clear 'swooping up' of part of the light. Obviously, it is difficult to account in purely visual terms for FM under this last condition. *Gamma*-movement might be conditioned by the phenomenal shrinkage of the field; but FM in wholly visual terms in quite different. FM where retraction is in a direction opposed to movement perceived is a visual paradox.

What we have said about the qualitative aspect of visual movement furnishes abundant proof of a fact which struck us as being particularly impressive. We have here clearly demonstrated the remarkable facility of the organism whereby it may attain a functional end under extreme poverty of experiential resources. At times, one is hard-pressed to indicate the experiential items involved. The phenomenal basis is often so vaporous and tenuous that one is driven either to central conditions or to obscure somatic factors. The *O*s noticed this inadequacy of 'tangible' experiential factors. They were certain of movement but not of the means utilized to apprehend it. Their reports indicate that the perception progressively grew thinner, especially toward the close of a sitting. It was as if the organism became definitely inclined toward an immediate apprehension. Under such conditions use was made of any kind of experiential material at hand. But the material was often so obscure that *O* could only hint at its character.

Apprehension of movement stroboscopically appears, on the whole, to resolve itself into functional operation under various central, peripheral and extra-organic conditions. The relative contribution of each varies from occasion to occasion. We saw that certain objective conditions (*e.g.*, rate and interval) might be widely altered without producing significant differences in perception. We find no reason for believing that any single quality (such as a 'flashing grey') may be set up as the experiential basis of visual movement. We saw that so long as one member is held over or related to another, or so long as one member is apprehended in two or more successive positions, movement ensues. This ligation may be accomplished by a variety of experiential and somatic factors. But movement may also depend, as we believe, upon the operation of a firmly entrenched set, capable of producing, even when released under extremely diverse objective conditions, the same functional outcome. Many of the cases where the experimental basis was meager could be attributed, so we surmise, to a highly habituated perceptive attitude. The progressive 'bareness' of the performance may be partly ascribed to this central disposition. Under the extreme limits of functioning it means, as Linke found, that strict object-identification is not necessary—the organism using merely a *'that-that'* or a *'there-there.'* In this way, triangles, for example, may move into circles, or movement may be seen before the object is perceived.

Besides the influence of central factors, there were, we said, other factors for connecting movement-wise the discrete expositions. In the absence of all visual items other than the perception of successive positions, clear movement was apprehended through kinaesthesis. These experiential qualities served to integrate the whole perceptual incorporation; that is to say, to join the otherwise unrelated visual constituents. Verbally stressed exposures, ocular shifts and lingual pursuit movements, as well as slight or incipient rhythmic or nonrhythmic movement of the general musculature furnished an experiential background for the purely visual items to functionate in perception.

The contribution of central tendencies and kinaesthesis to the apprehension of movement includes many factors of a nonvisual character. In them we find justification for the assertion that the visual perception of movement may be considered as an abstractive perception in the sense that one need not see object of particular dimensions as moving. It may mean only that within a given area of apprehended space something has moved. There is vague spatial characterization but little shape or filling. The relative degree of abstraction depends primarily upon the relative contribution to the performance of central factors and kinaesthesis as against the contribution of purely visual moments.

V. CONCLUSIONS

Many experiments have been conducted upon the problem of visually apprehended movement under discrete retinal excitations. Various explanations have been offered; but no single explanation that is wholly adequate to the facts has yet been advanced. Two have been presented within recent years;—the one given by Wertheimer is physiological, the other offered by Dimmick is psychological in character. In this investigation we have sought to test the bearing of the facts upon these rival explanations. Our results justify, as we believe, the following conclusions.

(*1*) It is impossible to set up a significant correlation between any one temporal interval and full-movement.

(*2*) Clear and insistent visual movement may occur under phenomenal simultaneity of both figures.

(*3*) Pronounced visual movement may be objectively determined by the immediate addition (without interval) of one member to another to form the acute angle or the two parallel lines.

(*4*) Clear movement may be likewise obtained by the sudden removal, instead of the addition, of one member.

(*5*) Insistent visual movement may be apprehended under successive presentations with no temporal interval between.

(*6*) Wertheimer was correct in his description of certain visual movements which do not lend themselves to a characterization in qualitative terms of vision.

(*7*) The fundamental assumption of Wertheimer's physiological explanation: namely, that the *phi-function* requires a determined and specific temporal course, does not stand experimental test. It must therefore be abandoned.

(*8*) The significance of a visual grey of a certain durational course, alleged to be the psychological correlate of visual movement, appears, in the light of our experiments to have been exaggerated.

(*9*) Non-visual resources are utilized to a greater extent in the apprehension of movement than Wertheimer and Dimmick recognized.

(*10*) The visual apprehension of movement under discrete retinal stimulation is a form of abstractive perception determined by stimulus, receptor and central organs, the degree of abstraction depending upon the availability of experiential items and upon the relative contributions of environmental and of organic factors.

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THE EFFECT OF REPETITION ON ENDING PREFERENCES IN MELODIES[1]

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HISTORY

Besides the innate factor in musical aesthetics, there is the habit effect. This has been shown in the work of Moore[2] and others.

Moore gives, in his monograph, a resumé of interval preferences, showing the varying ratings in the several historical periods. Thus, in ancient times, the octave, fifth and fourth were considered the most pleasing intervals, the thirds and sixths displeasing, and the seconds and sevenths extremely discordant. In our day, however, the first three named are classified as pleasing but uninteresting, the next two as very pleasing, and the last mentioned as still somewhat displeasing but of increasing interest.

Experiments performed by Valentine[3] on 146 adults, all university students, give similar results. The order of preference from best to worst was; major third, minor third, octave, major sixth, minor sixth, fourth, tritone, fifth, major second, minor seventh, major seventh, and last the minor second.

This historical evidence is strengthened by the experiments which Moore also undertook. *S*s were trained by having certain intervals presented to them many times. The change in the preference ratings which followed was then tabulated. The results showed that the interval preferences of individuals can be enormously modified through training.

INTRODUCTION

Meyer[4] has shown that, other factors constant, a melody which ends on the ratio symbol 2 is preferred to all other melodies.[5] This fact constitutes the tonic effect in music. In a previous paper,[6] the writer demonstrated that atonic endings —those not ending on ratio symbol 2—differ among themselves in preference ratings. That is, where the tone c is ratio symbol

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[1] From the Department of Psychology of Ohio State University.

[2] H. T. Moore, The Genetic Aspect of Consonance and Dissonance, *Psych. Mon.* 17, 1914, No. 2.

[3] C. W. Valentine, The Aesthetic Appreciation of Musical Intervals Among School Children and Adults, *Brit. J. Psych.*, 6, 1913, 190-216.

[4] M. F. Meyer, this JOURNAL, 14, 1903, 459.

[5] A ratio symbol is the frequency ratio with the pure powers of 2 extracted. For example, c is to f as 3 is to 4. The ratio symbols would be c, 3; f, 2.

[6] P. R. Farnsworth, Atonic Endings In Melodies, this JOURNAL, 36, 1925, 394-400.

2, a melody which ends on g will be preferred over one ending on e, and e over b♭. As the ratio symbols of c, g, e, and b♭ are 2, 3, 5, and 7 respectively, this means that, as an ending, 3 is preferred over 5 and 5 is preferred over 7.

Part 4 of the summary of that paper states, "When compared with ratio symbols 5 and 7, 3 apparently gives more finality than 5, and 5 more than 7, in the proportion of: symbol 3, 41%; symbol 5, 34%; and symbol 7, 25%."

PROBLEM

The problem then arose to determine the stability of these preferences. For example, might the percent. be changed in the direction of an increasing preference for the ratio symbol 7 as an ending over ratio symbol 3? Experiments were undertaken to ascertain the answer to the above and similar questions.

The 30 Judgments Test. The manner of approach was in part similar to that of the previous problem. 3 tones,—g, e, and b♭,—were chosen as representing the ratio symbols 3, 5, and 7, where c is ratio symbol 2. The corresponding musical notation, vibration-ratios, and actual vibration-rates, are here listed.

Musical Notation	*Ratio Symbol*	*Vib. Ratio*	*Vib. Rate*
g,	3	6	96.89 d.v.
g′	3	12	193.77 d.v.
e′	5	10	162.94 d.v.
b♭,	7	7	115.22 d.v.
b♭′	7	14	230.43 d.v.

These tones were played successively on a reed-organ. The 3 tones were so arranged that in one group g (3) was the last tone heard, in a second group e (5) was the last, and in a third group b♭ (7) was the final tone given.[7] The groups were sounded in a random order, and judged by the method of paired comparisons, as shown in the following experimental programme.

Thirty judgments were made by every *S*, as follows:

- Ten judgments with 3 and 5 paired
  - Five judgments with 3 first and 5 last
  - Five judgments with 5 first and 3 last
- Ten judgments with 3 and 7 paired
  - Five judgments with 3 first and 7 last
  - Five judgments with 7 first and 3 last
- Ten judgments with 5 and 7 paired
  - Five judgments with 5 first and 7 last
  - Five judgments with 7 first and 5 last

Instructions. The instructions were as follows: "You will hear two series of three tones each, which you are to compare. Which series sounds more finished, more complete, more at rest? If it is the first, mark '1' on your paper; if it is the second, mark '2'." The instructions were repeated so that every *S* understood what was expected of him.

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[7] The numbers in parentheses in the text refer to ratio symbols.

Subjects. 3 groups of *S*s were selected from the summer school student body. They will be designated groups A, B, and C. Group A averaged 12 members, group B 9, and group C 8. Each group met for approximately a half hour every Monday, Wednesday and Friday, until groups A and B had each completed 7 work periods, and group C 11. The end of the summer quarter was then at hand. Group C's greater number of work periods is due to the fact that it started some days earlier, and held to its work on days when vacations interfered with groups A and B.

Procedure. At the beginning and end of every work period, the 30 judgments test, described above, was given. Between these, a drill period was inserted which consisted of listening to a prepared series of tone combinations. These drill periods differed in some respects with the several groups. The effect of this drill period was measured by the differences between the beginning and end tests.[8]

EXPERIMENTS AND RESULTS

Group A. The drill for this group was in hearing the melody end on ratio symbol 3. During the drill period, the *S*s heard tones in the following order,—a key note (ratio symbol 2), pause, ratio symbol 5, 7, and last 3. Examples would be: c, pause, e, b♭, g; g, pause, b, f, d; etc. These were all played with a falling inflection. Such sequences were played, using the different scale notes as keys, and proceeding up and down the scale. It should be noted that the drill melody invariably ended on ratio symbol 3. This drill was continued slowly for some 10 min. During this time about 70 sequences were presented.

Example of Falling Inflection

Order Presented	*Mus. Notation*	*Ratio Symbol*	*Vib. Ratio*
1st.	e′	5	10
2nd.	b♭	7	7
3rd.	g	3	6

After this, the 30 judgments test was again given. With group A, the tones in this test were heard in a rising, then falling inflection. An example would be: e (5), the next higher b♭ (7), then the next lower g (3). The percent. of preferences for ending the melody on g (3) was then calculated, and compared with that found at the beginning of the hour.

Example of Rising then Falling Inflection

Order Presented	*Mus. Notation*	*Ratio Symbol*	*Vib. Ratio*
1st.	e′	5	10
2nd.	b♭′	7	14
3rd.	g′	3	12

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[8] For more detailed explanation of the technique, see the writer's article referred to above.

Percent. of Preferences for g (3)

Period	*Beg. of Hour*			*End of Hour*			*Inc. during Hour*			*Inc. over 1st Day*		
	g	e	b♭	g	e	b♭	g	e	b♭	g	e	b♭
1	38	33	29	42	36	22	+ 4	+ 3	− 7	+ 4	+3	− 7
2	34	36	30	44	36	20	+10	0	−10	+ 6	+3	− 9
3	43	42	15	48	39	13	+ 5	− 3	− 2	+10	+6	−16
4	48	42	10	53	38	9	+ 5	− 4	− 1	+15	+5	−20
5	45	43	12	55	39	6	+10	− 4	− 6	+17	+6	−23
6	48	40	12	48	40	12	0	0	0	+10	+7	−17
7	42	45	13	55	35	10	+13	−10	− 3	+17	+2	−19
Average daily increase							+ 7	− 3	− 4			
Inc. from beginning to end							38 to 55			+45		
							33 to 35				+6	
							29 to 10					−65

Conclusion: a drill of 10 min. on ratio symbol 3 gave an average increase of 7% in the preferences, or an increase of 45% from the beginning to the end of the experiment.[9]

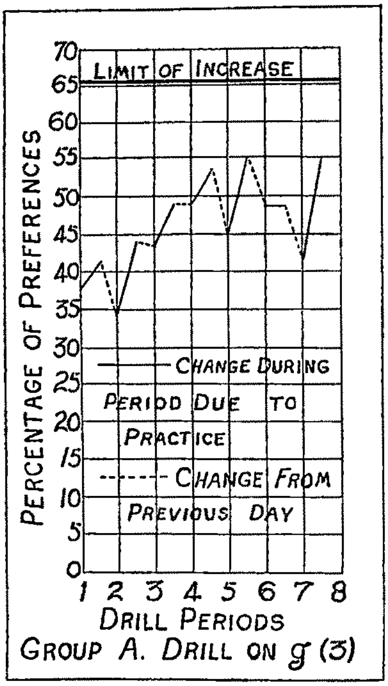

FIG. I

Explanation of Fig. I. When 3 factors are being compared by the method of paired comparisons, 1 factor appears in only ⅔ of the ballots, so that it can maximally receive only ⅔ of the votes. With preferences expressed for the 3 ratio symbols, the highest score possible for any one of these would be 66%. This percent. is therefore labelled 'Limit of Increase.'

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[9] Nine controls on this and subsequent groups showed slight variations.

From the solid lines, it can be seen that the preferences increased after drill in almost every instance. The downward slope of the majority of the dotted lines shows loss of training from one practice period to the next. That the curve is not smooth is to be expected in view of the small number of subjects and the daily variations—metabolic and otherwise.

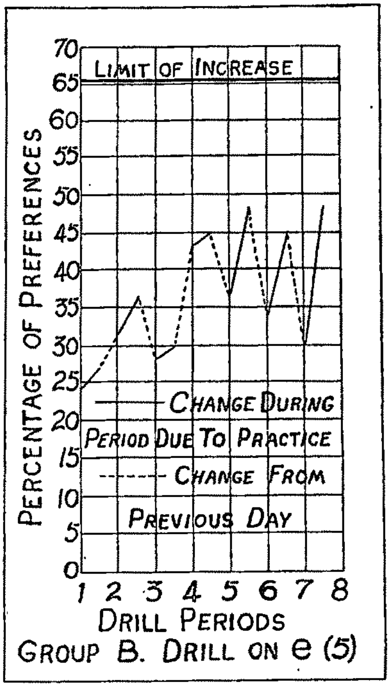

FIG. II

Group B. The drill here was for ending on the ratio symbol 5. In the 30 judgment test a falling inflection was employed—as g (3), next lower e (5), next lower b♭ (7).

Example of Falling Inflection

Order Presented	*Mus. Notation*	*Ratio Symbol*	*Vib. Ratio*
1st.	g′	3	12
2nd.	e′	5	10
3rd.	b♭	7	7

During the drill period, the *S*s heard tones in the following order—a key note, pause, ratio symbol 7, 3, and last 5. Examples would be: c, pause, b♭, g, e; g, pause, f, d, b; etc. The inflection here was rising, then falling; as b♭, next higher g, next lower e.

Example of Rising then Falling Inflection

Order Presented	*Mus. Notation*	*Ratio Symbol*	*Vib. Ratio*
1st.	b♭	7	7
2nd.	g′	3	12
3rd.	e′	5	10

Percent. of Preferences for e (5)

Period	*Beg. of Hour*			*End of Hour*			*Inc. during Hour*			*Inc. over 1st Day*		
	e	g	b♭	e	g	b♭	e	g	b♭	e	g	b♭
1	24	47	29	27	50	23	+ 3	+ 3	− 6	+ 3	+ 3	− 6
2	32	35	33	37	32	31	+ 5	− 3	− 2	+13	−15	+ 2
3	28	29	43	30	38	32	+ 2	+ 9	−11	+ 6	− 9	+ 3
4	43	30	27	45	40	15	+ 2	+10	−12	+21	− 7	−14
5	37	35	28	48	30	22	+11	− 5	− 6	+24	−17	− 7
6	34	36	30	45	28	27	+11	− 8	− 3	+21	−19	− 2
7	30	40	30	47	29	24	+17	−11	− 6	+23	−18	− 5
Average daily increase							+7½	− 1	−6½			
Inc. from beginning to end							24 to 47			+96		
							47 to 29				−38	
							29 to 24					−17

Conclusion: a drill of 10 min. on ratio symbol 5 gave an average increase of 7½% in the preferences. At the end of 7 drills the percent. preference had almost doubled,—96%.

Group C. This group heard as endings the ratio symbol 7 As with group B, a falling inflection was used in the 30 judgment· test. During the drill period, the simple melody g (3), e (5), b♭ (7) was played with a rising, then falling inflection.

Rising then Falling Inflection

Order Presented	*Mus. Notation*	*Rat. Symbol*	*Vib. Ratio*
1st.	g	3	6
2nd.	e′	5	10
3rd.	b♭	7	7

Percent. of Preferences for b♭ (7)

Period	*Beg. of Hour*			*End of Hour*			*Inc. during Hour*			*Inc. over 1st Day*		
	b♭	g	e	b♭	g	e	b♭	g	e	b♭	g	e
1	18	49	33	28	46	26	+10	− 3	− 7	+10	− 3	− 7
2	24	55	21	26	55	19	+ 2	0	− 2	+ 8	+ 6	−14
3	24	52	24	33	52	15	+ 9	0	− 9	+15	+ 3	−18
4	29	59	12	34	59	7	+ 5	0	− 5	+16	+10	−26
5	37	28	35	37	29	34	0	+ 1	− 1	+19	−20	+ 1
6	40	36	24	43	32	25	+ 3	− 4	+ 1	+25	−17	− 8
7	33	37	30	44	28	28	+11	− 9	− 2	+26	−21	− 5
8	39	32	29	41	35	24	+ 2	+ 3	− 5	+23	−14	− 9
9	33	32	35	47	22	31	+14	−10	− 4	+29	−27	− 2
10	48	27	25	48	24	28	0	− 3	+ 3	+30	−25	− 5
11	51	23	26	53	23	24	+ 2	0	− 2	+35	−26	− 9
Average daily increase							+ 5	− 2	− 3			
Inc. from beginning to end							18 to 53			+195		
							49 to 23				−53	
							33 to 24					−27

Conclusion: a drill of 10 min. on ratio symbol 7 gave an average increase of 5%, but an increase of 195% from the beginning to the end of the experiment.

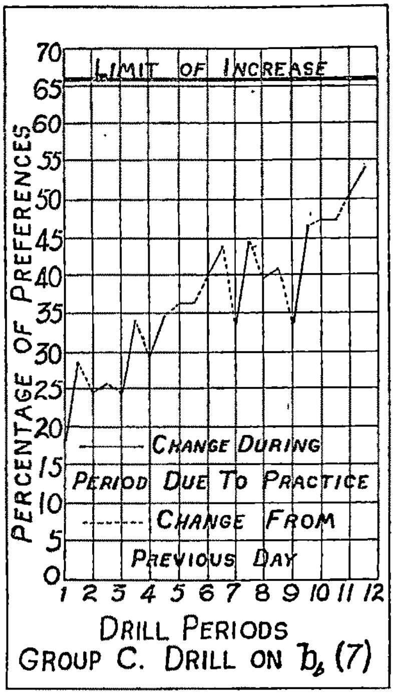

FIG. III

It is interesting to note that at their origins the curves had preferences in the order, ratio symbol 3, 38%; 5, 24 %; 7, 18%. At the end the preferences were in the order, ratio symbol 3, 55%; 5, 47%; 7, 53%.

SUMMARY

(1) Ending preferences may be permanently altered by training. An increasing familiarity with a ratio symbol ending leads to increasing preferences for that specific ending.

(2) The type of training,—order, specific notes played, etc.,—does not seem to have a large effect. The important factor in the drill appears to be the hearing of the same continuous ratio symbol as the melody ending.

BLACK AND GREY IN VISUAL THEORY

By Gilbert J. Rich

Everyone who has given attention to theories of vision is aware that, with the exception of Hering's theory, little attempt is made to explain the sensation of black. Ladd-Franklin[1] claims that black may be accounted for on her theory, without going into the details of the explanation. And now Neifeld[2] comes forward with the promised account. He holds that the sensation of black is a sensation of constant intensity attached to a cortical situation of inactivity or rest. Sensations of grey are made up of this constant sensation of blackness and a variable amount of the sensation of whiteness. Moreover, he attempts to explain why such a constant sensation corresponding to cortical inactivity should exist. We are constantly aware of the visual field in which we live; indeed, since every point has its 'place-coefficient,' we are aware of every point in the visual field. As a result, any break in the field due to absence of stimulation would be decidedly unpleasant; and what would otherwise be such gaps are filled with a definite non-light sensation, blackness. Two questions may be asked concerning this theory of black. (1) Does it actually explain black in the sense in which colors are explained by theories of color vision? (2) Does it explain the various visual phenomena into which black enters?

In considering the first question we must remember that any sensory theory attempts to give an hypothetical account of the nervous activity involved in the process of sensing which will explain as many as possible of the facts of sensation. Thus, in the case of color theories, Hering postulates his three reversible photo-chemical reactions and explains contrast, adaptation, after-image, etc., in terms of them. Ladd-Franklin similarly explains these phenomena in terms of her genetically conceived color molecule. One may look in vain for any such account in Neifeld's paper. No nervous mechanism is postulated. We are told that black occurs when there is a situation of inactivity in the cortex, but we are not told how it occurs. That the sensation of blackness is experienced when no light waves reach the retina is well-established fact, not theory. Neifeld merely says that under these conditions we see black. The only positive aspect of this explanation is that it makes black the point of retinal inactivity, instead of the grey of the Hering-Müller hypothesis. This, as we shall see, is the crucial point of the whole argument. Does a constant black or a constant grey better explain the facts of vision? This problem leads to a consideration of the second question stated above.

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[1] C. Ladd-Franklin, Tetrachromatic Vision and the Development Theory of Vision, *Science*, 55, 1922, 560.

[2] M. R. Neifeld, The Ladd-Franklin Theory of the Black Sensation, *Psych. Rev.*, 31, 1924, 498-502.

In discussing the extent to which the facts of vision may be explained by the one or the other of these postulates, we may well confine ourselves, for the sake of brevity, to the outstanding facts of contrast, adaptation, negative after-image and the black-grey-white series. In the case of contrast, a positive explanation may be given in several ways. So long as grey is the center of the system, the equilibrium point of a reversible reaction or of a split color molecule, black-white contrast stands upon the same plane as does color contrast and may be explained by the same mechanisms. For the Hering theory it presents few problems once the Müllerian constant grey is postulated. The Ladd-Franklin theory might, in much the same way, postulate an original grey molecule which splits into white and black just as the white molecule splits to give the colors. This type of postulate would give a positive black sensation and a common basis for color contrast and brightness contrast. But Neifeld's theory neither brings the two forms of contrast under the same category nor affords a basis for explaining black-white contrast. If black is a constant sensation, how can neighboring stimulation by light increase it? Suppression of the so-called endogenous retinal processes cannot be invoked, since they are of the same nature as a central grey, only in the retina instead of in the cortex, against which Neifeld takes a definite stand. When the contrast is in the opposite direction, the situation is even more difficult to explain. A dark grey will produce a contrasting whiteness. Yet the dark grey is a mixture of the constant black with a low intensity of white. Now, if the constant black and a slight white excitation make surrounding objects white, why should not a mixture of the constant black and a low intensity of yellow make the surroundings yellow? But we know that this does not happen. Thus, constant black and low-intensity white have a white contrast effect while constant black and low-intensity yellow have a blue contrast effect. The difficulty is that one pole of the colorless series instead of its middle is made the neutral point. Black and white, although they contrast, are not admitted as a disappearing color pair.

White should, if the Neifeld hypothesis be valid, in time adapt to black. If the principles of color adaptation of the Ladd-Franklin theory are applied to the complete color molecule we should not expect less. If the blue or yellow part of the molecule ceases to act under continued stimulation, why should not the entire molecule behave similarly? Again, the faint grey due to the endogenous retinal processes is no help. If it is sufficiently intense to be the grey resulting from adaptation to white, it is sufficiently intense to be the constant visual sensation, and we are back to Müller's hypothesis, only with localisation in the retina. A further difficulty in that case arises from the fact that it would become necessary to explain vision under conditions of a constant mixture of central black and retinal grey to which is added any reaction of the color molecule. Adaptation to black is likewise difficult to explain. Lack of light gives a black sensation only at first; the black changes in time to a neutral grey. Where does this grey come from? If from a constant retinal process, why add a constant cortical black? In the case of negative after-images, the arguments already set forth may be applied, since the conditions are analogous to those of contrast. Constant black added to weak white excitations results in a white after-image, while a blue after-image follows if it is added to weak yellow excitations. All of these difficulties occur because black and white are not considered either antagonists or molecular complements.

Neifeld makes no provision in his system for grey as a separate quality. It has, of course, been long accepted among psychologists that the black-white series is a single linear series and that all greys are merely admixtures of black and white. This view has been based in large part upon the supposition that, as one proceeded along the series from dark to light, the first sensations beyond the blackest black are noticeably whitish, becoming more and more so as one passes up the scale; and that, although they become less and less blackish, all colorless sensations save the most intense white have some black in their composition. More recently, however, the notion of the black-white series as forming a single straight line has been challenged, together with the observations upon which it is founded. Dimmick[3] holds that the black-grey-white series actually consists of two series which join each other at an angle of 180°. One of these series runs from black through the darker greys to middle grey; the other runs from white to middle grey through the light greys. Dimmick contends that a light grey, although resembling both white and grey, contains not the least hint or trace of black. Similarly a dark grey resembles both grey and black, but not white. Dimmick sought to illustrate the relation of grey to black and white by determining (by the method of limits) the thresholds of whiteness, blackness, redness, greenness, blueness and yellowness as a white, black or colored paper is added to grey upon a color mixer. He found that the average limens were all of the same order of magnitude and suggests that all may be regarded as rough chromatic limens, those for black and white as well as those for the four principal colors.

As a proof of his view as to the nature of the black-grey-white series, Dimmick's experiment fails to carry conviction.

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[3] F. L. Dimmick, A Note on the series of Blacks, Greys, and Whites, this JOURNAL, 31, 1920, 301-302.

Similar results might be obtained even if the older notion of grey as intermediate between black and white were true. In that case the thresholds obtained for black and white would be merely qualitative DL's within a single series. We have, therefore, attempted to attack the problem experimentally by a more direct method. The data given below are results of a preliminary series of experiments performed in 1921, unfortunately not carried to their logical completion. Our only excuse for presenting psychophysical data based on but 10 series is the lack of any other results bearing upon the problem, the importance of the status of grey for visual theory, and the hope that some investigator may be encouraged to study the problem more intensively.

In general, our procedure consisted in obtaining (by the constant method) the stimulus thresholds for black, grey and white. The stimulus was in every case a mixture of Milton-Bradley black and white papers displayed upon a rotating color mixer. In order to have the stimuli differ by approximately equal psychological intervals, the amount of white in the mixture was varied in a geometrical series. Ten stimuli were used, namely: 6.0°, 9.4°, 14.8°, 23.2°, 36.5°, 57.3°, 90.0°, 141.2°, 220.0°, and 348.0° of white. These were presented to the *O* in haphazard order. The instructions were: "After 'ready' and 'now' signals, you will be shown a colorless disk. You are to report whether or not it shows any likeness or resemblance to, or hint of, white, reporting either 'yes' or 'no.' You are to make this report without reference to any relation of the disk to black or to grey." There were, in separate series, exactly similar instructions to judge grey without reference to black or white, and to judge black without reference to white or grey. Ten series were run under every instruction and all instructions were used (in varying orders) in every observational period. The *O*s were three graduate students (women), each of whom had had two years' experience in routine laboratory observation.

As the results are based upon but 10 observations for every stimulus, it did not appear justifiable to apply the usual mathematical procedures for the computation of thresholds. Instead we used the simple method of interpolation. But our stimuli were arranged in geometrical progression, since brightness follows Weber's Law. For this reason the values given here were obtained by geometrical (logarithmic) interpolation between the two values on each side of 50% judgments. Four thresholds were worked out; an upper limen for blackness, a lower limen for whiteness, and both upper and lower limens for greyness (all thresholds given in degrees of white).

Threshold	*Obs. A*	*Obs. B*	*Obs. G*
Grey-Lower	15	12	2[4]
Black	23	46	13
White	152	74	12
Grey-Upper	177	141	103

A consideration of these figures makes it evident that all three *O*s were not using the same criteria of judgment. Reference to the introspective reports (too lengthy to quote here) shows that A was judging whether or not the quality in question predominated rather than whether or not it existed at all in the mixture. As a result, her thresholds for black and for white are far apart, and grey overlaps the other two qualities but little. B, and more especially G, appear to have adhered to the instructions so far as possible. In any case, the important point is that there is no overlap of whiteness and blackness (the overlap of 1° in the case of G is not significant in only 10 series). So far as this type of procedure is concerned, grey does not seem to be intermediate between black and white any more than it is intermediate between blue and yellow. A dark grey is not whitish. It is a part of a black-grey series, not of a black-white series. In the same way, a light grey is not blackish and should be placed in a white-grey rather than in a white-black series, just as a pale blue is not yellowish and is part of a blue-grey series but not of a blue-yellow series. Preliminary results of this sort cannot, of course, be considered conclusive evidence. Further experimentation is necessary. But they may be considered as a strong indication in favor of Dimmick's hypothesis that the black-grey-white series consists of two linear series meeting at the common point of grey.

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[4] Limen obtained by extrapolation.

The importance of this view is obvious. If grey is not the psychological intermediate between black and white, any theory that attempts to explain it as a mixture of a constant black and a variable white excitation fails to meet the facts. Grey, it is true, may be produced by mixing physically the stimuli for black and for white (lack of light and heterogeneous light). But we may not safely argue from stimulus to sensory process. Grey may also be produced by mixing the stimuli for blue and for yellow. Yet it is not intermediate between them on a single linear series, but is the meeting point of the blue-grey and yellow-grey series which join each other at an angle of 90°. Thus we again see, as already stated in another connection, that black and white must be paired in the same way as are other complementary colors. Grey, not black, is the center from which visual experience radiates, so to speak. Titchener recognizes this fact in his recent discussion of visual intensity. He points out that grey stands as the minimum of visual intensity, just where we should expect the constant visual excitation to stand.

A further question may well be raised. Why is grey so similar to black and white that it has usually been considered a part of linear series with them? A developmental answer, however valid such an answer may be, is possible. Neifeld remarks that a more primitive eye than that of man might distinguish the presence or absence of light without the sensation of black. While it is difficult to speculate upon the quality of such primordial sen-

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[5] E. B. Titchener, Visual Intensity, this JOURNAL, 34, 1923, 310-311.

sory stuff, it is nevertheless logical to suppose that of all our visual sensations grey has developed the least distance from it. Black and white, more primitive than color, are most like grey; while the colors, being most developed and differentiated, are least like grey. The whole development may, perhaps, be illustrated in human vision if we remember that our eye is a dual organ. The night eye (rod vision) is more primitive and less differentiated than is the day eye (cone vision). But what experiences do we have in night vision? Merely greys of varying brightnesses. We do, of course, also see black at night. Is it due to lack of stimulation of the rods or of the cones? In view of the lack of evidence to the contrary, it is reasonable to ascribe it to the same organs as its paired experience white, that is, to the cones. The cones, then, being at a higher stage of development than the rods, mediate more differentiated types of sensation, black and white instead of grey in the first instance, followed by the later differentiation of white into colors as postulated by Ladd-Franklin. The black-white paradox, that black and white are both complementaries (mix to give residual grey) and non-complementaries (mix to give an intermediate) now disappears, since grey is not intermediate to black and white but merely the quality from which they are but partially differentiated. Black and white are to be explained in the same way as are colors.

We return, then, to our original argument. The facts of vision seem unexplainable save on some hypothesis of a constant visual process. Müller assumes that this constant process is grey, Neifeld that it is black. On *a priori* grounds there seems to be little choice unless one tries to argue from stimulus to sensation. A cortical black is just as hypothetical as is a cortical grey. But there is a great difference in explanatory value between the two hypothesis. A constant grey enables one to explain the facts of contrast, adaptation, after-image, etc., as applied to black and white by placing them upon the same basis as similar phenomena in color. It also is in accord with the evidence (as yet not conclusive) as to the nature of the black-grey-white (double) series. In all these respects the hypothesis of a constant black process meets difficulties. Whether the grey or black be considered retinal or cortical is of little importance. Morphologically, the retina is a part of the brain, and its complexity as compared with other sense organs is sufficient to explain a constant excitation not found elsewhere. But so long as the constant visual process remains in the stage of hypothesis its probable location matters but little. The outstanding question is: What type of constant visual process will best explain the facts of vision? As we have shown, a hypothetical constant grey is more adequate to the explanation of observed visual phenomena than is a postulated constant black.

AN APPROACH TO THE PSYCHOLOGY OF MOTIVES[1]

By A. J. SNOW, Northwestern University

The object of this paper is iconoclastic, for it will attempt a criticism of some of the favorite psychological preconceptions of economists[2]. Among these darlings of the economic mind, two conceptions stand out. One is that there is a certain kind of action called "native" and another called "acquired." The second is that a man is a pattern of definite "instincts" or "instinctive drives," whose interactions determine his conduct. These two conceptions are closly related, for belief in the existence of definite instincts must rest on a distinction between native and acquired disposition.

Historically, these concepts were the result of the attempt to get away from the point of view that human actions are the result of abstract reason to the point of view that actions are impulsive, habitual—the result of conflicting desires, wants, and needs. As a matter of fact, however, the doctrine of instincts as an instrument explaining activity is of little value, since the value of an explanation to a scientific man lies in its predictive usefulness, and the above theory makes prediction just as futile as the rationalistic theories of the seventeenth and eighteenth centuries. To say that human activity at any given moment is the result of some native driving force does not throw any light on the situation, but on the contrary muddles it, because that ends our search and drives us to the unknown. A classification of the causes of human behavior in terms of ends or desires may give the reader with a teleological bias some moral support; but to use such classification as a scientific one must be abhorrent to many biologists.

The only way in which the doctrine of instinct would be of use would be to give it an interpretation not given it usually. We must abandon the notion of plurality of instincts, and replace that with the notion of *instinctive*[3] behavior, implying by *instinctive* action merely that all activity is a natural consequence of the interaction of the functions of the many organs that constitute the human organism and its immediate environment. Under this more sensible interpretation, instinctive activity would naturally include not only the activities of the simple reflexes and tropisms that are generally recognized as native, but also the other modifications of the inherited structures, such as habits of action as well as of thought.

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[1] Read before the American Economic Association, Chicago, Dec. 30, 1924.

[2] For a more complete discussion see A. J. Snow, Psychology in Economic Theory, *Journ. Polit. Econ.*, August, 1924. See also *Psychology in Business Relation*, chaps. I, II, 1925.

[3] K. Dunlap, Are there any Instincts, *Journ. Abnor. Psychol.*, Dec., 1919. The Identity of Instinct and Habit, *Journ. Philos.*, Feb., 1922.

This leads us to our second point, the question of the distinction between "native" and "acquired" disposition. I should say that this distinction is arbitrary, that habits are just as much the result of *native* responses of the nervous system to the environment as the original reflexes that the baby inherits. How does one tell where "native" ends and "acquired" begins? For example: at a given time the factory whistle blows, whereupon all the employees in the factory gather up their tools, put them away, and leave the workshop. In this case we have a demonstration of the tendency to react in a very definite way. No matter how complex or simple the action in this may be, it is a reaction to a particular stimulus of a particular stimulus-response pattern. The reaction pattern of course does not exist immediately preceding the blowing of the whistle. However, the factory workers did have the specific tendency. It may not have existed during the whole working day; but no one will question its existential reality when the whistle blew. It was there ready to react in a given way when the time came. Now then, what is that tendency—that stimulus-reaction pattern? Is it any more than a certain arrangement in the nervous system? Is it not an arrangement that may be described in physical, chemical, and organic terms? Is it not true that because of the existence of such an arrangement the response was made, and prediction is possible? Is this disposition to react in this definite way to a factory whistle any less native, instinctive, or is it organically different from the activities of digestion? Why, then, should the one be called "acquired" and the other "native?" Is it not more appropriate to say that all of the responses of an organism to the total stimulation are "native?" and that these are what they are at any given time because of the specific conditions of the organism and the situation to which it happens to react at that time?

We must recognize, if we study the problem of human motives without bias, that the reactions of any animal are due to the accumulated individual organic history, and that that history consequently differs for different individuals. But that is not all. The environment is an important factor: the same situation may have different meanings to different individuals and to the same organism at different times, and consequently is productive of different reactions. In fact, the idea in mind, the purpose, desire, or need that may control the individual's interpretation of any specific experience to which he reacts, or within which he acts, is in itself the result of the particular biological history of that individual. Wherefore, the specific motives or causes involved in every case of like activity of different individuals, or of the same activity at different times for the same person, may differ, and the motives or causes involved can only be discovered accurately anew in each situation for each individual organism. While for general purposes we may classify different forms of stimulus-response patterns, we must remember that such a classification must always remain artificial, as we deal, not with any single stimulus, but always with a complexity of stimuli, and never with any single individual or independent nerve pattern, but almost always with a multiplicity of interacting and counteracting patterns, with the whole complex organism.

Therefore, the only approach to the study of human motives is not in the abstract or general but in the concrete situations. Abstract and essentially arbitrary conceptions of set instincts and equally arbitrary distinctions between "native" and "acquired" dispositions do not help us. We must make concrete studies. For example, for labor turnover we may enumerate the following causes or motives: abuse of power, wages, hours, unsanitary conditions, arbitrary advancement policy, faulty machinery or tools, heterogeneous "consciousness of kind" among team workers, personal incompatibility with immediate superiors, lack of standardized methods of process, lack of specialized training on the part of the worker, economic insecurity, physical incapacity, mental incapacity, and a mentality too great for its task. Statistics are unfortunately lacking as regards the relative importance of most of these various factors.

While the above enumeration of causes of labor turnover may be of some value, nevertheless, its usefulness is mainly for classification rather than for prediction. This sort of investigation would be better than the abstractions about instincts, but even better would be a series of concrete case-studies. In such studies we should consider concrete individuals in specific situations. For example, a study has been made of the relationship of mental alertness to labor turnover among a group of office boys in a certain factory. "In each case there has been found to exist a definite relationship between the mental alertness of employees, as determined by test scores, and labor turnover.... While admitting the danger of any study which aims to disassociate any one factor from its immediate surroundings, nevertheless, such a study may be justified on the grounds of its illuminating results and their actual use, when other factors composing the complete situation are given due weight."[4]

In this series of case-studies, as in any other, the causes valuable for predictive purposes for any specific human behavior are revealed, and only in some similar way shall we ever be able to determine the relationship that exists between human reactions and their causes.

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[4] A. J. Snow, Labor Turnover and Mental Alertness Test Scores, *Journ. Applied Psychol.*, September, 1923.

FURTHER RESULTS IN "THE RANGE FOR VISUAL ATTENTION, COGNITION AND APPREHENSION" EXPERIMENT

By H. Sherman Oberly, University of Pennsylvania

Problem

In a recent number of this Journal,[1] Professor Dallenbach takes exception to the choice of the judgments selected for the calculation of the limens reported in my Study on "The Range for Visual Attention, Cognition and Apprehension."[2] He based his exception on the ground that the limens were not computed in accordance with the phi-gamma hypothesis, and states that the limens presented "give us merely the ranges of confident, correct cognitions that are immediate and ungrouped, immediate and grouped, and mediated and counted." To these 3 types of judgments we have attached the categories of 'attention,' 'cognition,' and 'apprehension.' The purpose of this paper, which was in the course of preparation before the appearance of Professor Dallenbach's note, is to present the results of the experiment in greater detail so as to include all judgments, and also to determine the extent of the correlation between the correct responses of the *O*s and the degree of assurance.[3]

Results

Tables I to VI inclusive give the results of the observed relative frequencies for each *O* in greater detail than presented in the original paper.[4] The percent. of correct responses for every stimulus value (*s*) are given under the categories of attention, cognition and apprehension. The following columns to the right contain the percent. of correct judgments for every degree of assurance reported by the *O* under every category. It will be noted that, in the present tables, the observed relative frequencies used in finding the limens for these categories are spread or separated under the degree of certainty which each *O* reported. In the last 2 columns under every category will be found the percent. of judgments to which the *O* reported an assurance of either 2 or 1 (which latter corresponds to a "do not know" judgment).[5] It will be remembered that the limens of our former Study were calculated from the combined data of the first 3 columns (assurances 5, 4 and 3) and that the results in the last 2 columns were omitted from the calculations by the setting of the problem. As 250 judgments were made for every value of the stimulus, fractionated into 5 series, the values found in these tables are given at the nearest average.

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[1] *Op. cit.*, 36, 1925, 154-156. [2] *Ibid.*, 35, 1924, 332-352.

[3] The *O* was asked to "report verbally the degree of assurance or certainty of your judgment, using this scale for your report: 5—if it is a 100 to 1 bet; 4—if it is a 5 to 1 bet; 3—if it is a 1 to 1 bet; 2—if it is a 1 to 5 bet; 1—guess or do not know."

[4] *Op. cit.*, 336-338.

[5] Results for F are not included inasmuch as he was familiar with the plan to exclude these judgments when calculating.

TABLE I Observer F

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	0.84	1.00	1.00	0.84					1.00					1.00				
3	0.64	1.00	1.00	0.64					1.00					1.00				
4	0.42	0.96	0.98	0.42					0.96					0.98		0.01		
5	0.18	0.80	0.95	0.18					0.78		0.02			0.89	0.03	0.03		
6	0.04	0.39	0.86	0.04					0.36	0.02	0.01			0.63	0.10	0.13		
7		0.08	0.69						0.05	0.02	0.01			0.26	0.18	0.25		
8		0.03	0.57						0.01	0.01	0.01			0.08	0.11	0.38		
9			0.32											0.02	0.03	0.27		
10			0.20											0.03	0.03	0.14		

TABLE II Observer M

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	1.00	1.00	1.00	1.00					1.00					1.00				
3	0.80	1.00	1.00	0.80					1.00					1.00				
4	0.49	0.96	1.00	0.49					0.96					1.00				
5	0.04	0.84	0.99	0.04					0.84					0.95	0.02	0.02		
6		0.63	0.94						0.51	0.06	0.06			0.59	0.14	0.21		0.01
7		0.16	0.77						0.04	0.07	0.05			0.05	0.18	0.54		0.06
8		0.14	0.62							0.02	0.12				0.02	0.56	0.04	0.28
9		0.02	0.16								0.02					0.10	0.06	0.68
10			0.01														0.01	0.83

TABLE III Observer S

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	1.00	1.00	1.00	1.00					1.00					1.00				
3	0.95	1.00	1.00	0.95					1.00					1.00				
4	0.59	0.96	1.00	0.57	0.01	0.01			0.92	0.03	0.01			0.95	0.03	0.02		
5	0.09	0.75	0.98	0.08	0.01				0.44	0.25	0.06			0.56	0.34	0.08		
6	0.02	0.76	0.96	0.02					0.26	0.32	0.15	0.03		0.32	0.40	0.22		
7	0.01	0.57	0.83				0.01		0.02	0.18	0.25	0.11		0.03	0.28	0.37	0.03	0.04
8		0.65	0.83							0.11	0.34	0.20			0.14	0.45	0.04	0.05
9		0.33	0.42							0.01	0.11	0.21			0.01	0.15	0.05	0.40
10		0.14	0.21									0.14				0.02	0.05	0.65

TABLE IV Observer Mc

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	0.73	1.00	1.00	0.73					1.00					1.00				
3	0.78	0.96	1.00	0.78					0.96					0.99				
4	0.32	0.99	1.00	0.32					0.98		0.01			0.99		0.01		
5	0.13	0.87	0.97	0.13					0.85	0.02	0.00			0.94	0.03	0.00		
6		0.74	0.83						0.45	0.23	0.05	0.01		0.52	0.24	0.06		
7		0.57	0.67						0.11	0.30	0.14	0.02		0.14	0.35	0.16		0.04
8		0.58	0.67						0.08	0.31	0.16	0.03		0.08	0.35	0.20		0.06
9		0.27	0.31							0.05	0.15	0.07			0.06	0.17	0.01	0.34
10		0.23	0.25							0.03	0.09	0.11			0.03	0.10	0.01	0.51
11		0.10	0.11							0.01	0.01	0.08			0.01	0.01	0.01	0.71
12		0.01	0.02									0.01					0.01	0.88

TABLE V Observer SC

	% Correct Response			*Attention*					*Cognition*					*Apprehension*				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	1.00	1.00	1.00	1.00					1.00					1.00				
3	0.99	1.00	1.00	0.99					1.00					1.00				
4	0.96	1.00	1.00	0.96					1.00					1.00				
5	0.54	0.98	1.00	0.54					0.98					0.98	0.01	0.01		0.01
6	0.12	0.95	0.98	0.12					0.83	0.06	0.05	0.01		0.85	0.07	0.05		0.00
7		0.81	0.84						0.48	0.17	0.15	0.01		0.51	0.18	0.15		0.01
8		0.76	0.81						0.18	0.30	0.27	0.01		0.19	0.32	0.28	0.01	0.01
9		0.54	0.56						0.04	0.18	0.29	0.03		0.04	0.18	0.29	0.02	0.06
10		0.38	0.38							0.04	0.30	0.04			0.04	0.30	0.00	0.10
11		0.41	0.42							0.01	0.26	0.14			0.01	0.26	0.01	0.10
12		0.30	0.31								0.17	0.13				0.17	0.00	0.24

TABLE VI Observer JC

	% Correct Response			*Attention*					*Cognition*					*Apprehension*				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	0.73	1.00	1.00	0.73					1.00					1.00				
3	0.68	0.99	1.00	0.68					0.99					1.00				
4	0.44	0.64	0.98	0.44					0.65					0.98				
5	0.05	0.38	0.98	0.05					0.38					0.96	0.01	0.01		0.01
6		0.33	0.92						0.31	0.01		0.01		0.82	0.05	0.03	0.01	0.01
7		0.11	0.91						0.05	0.04	0.01	0.01		0.47	0.29	0.12	0.02	0.03
8		0.05	0.73						0.01	0.02	0.01			0.05	0.09	0.35	0.23	0.09
9			0.35											0.01	0.01	0.06	0.27	0.40
10			0.23													0.03	0.20	0.60
11			0.06														0.06	0.77
12			0.04														0.04	0.91

In Table VII will be found a record of the observed relative frequencies of incorrect judgments for all of the *O*s. A column is given to every *O* and the results for every value of stimulus dots is to be found in a single row. This table is given for the sake of completeness in presenting our results. It will be observed that the percent. of wrong judgments varies considerably from *O* to *O*. For Mc and JC wrong judgments were given for even the smaller stimulus values. For M, S and SC no wrong judgments are given on smaller values of the stimulus, although for SC higher values are considerably greater than for the other *O*s.

From Tables I to VI it would be possible to calculate the limens for the categories of judgment set by our problem in the manner which Professor Dallenbach desires. We believe, however, that we were not only justified in excluding the lower degrees of assurance from our calculations, but still believe that it was the desirable thing to do. In former psychophysical work little attention has been taken of the degree of assurance of the *O*, which means no attention has been taken of the variation of conservatism from *O* to *O*. Urban[6] made such an attempt for lifted weights but abandoned it as leading to a too complicated protocol. It will be remembered however that Urban was dealing with a difference threshold which has as its basis a much more complicated procedure than that which we were requiring for our *O*s. The protocols of the *O*s in the present experiment indicated in every case that the requirements of our instructions were not too complicated to be fulfilled with reasonable facility. It is perfectly obvious that the degree of conservatism between different *O*s varies and that different judgments are made on varying degrees of assurance. Your conservative *O* will report only those judgments which have a relatively high degree of assurance. The unconservative will report on the slightest cue. Little or nothing has been done in psychophysics of the past to control or standardize this factor. The introduction of "guess" judgments, although it is an attempt to control this factor, can of course, be of little use until the "guess" judgment itself is more exactly defined. In other words, that which shall be included in a guess judgment is still determined by the degree of conservatism of the individual subject.

TABLE VII

S	F	M	S	Mc	SC	JC
2	0.00	0.00	0.00	0.00	0.00	0.00
3	0.00	0.00	0.00	0.00	0.00	0.01
4	0.01	0.00	0.00	0.00	0.00	0.01
5	0.01	0.01	0.02	0.03	0.00	0.01
6	0.01	0.05	0.04	0.17	0.02	0.07
7	0.02	0.17	0.13	0.29	0.14	0.07
8	0.05	0.11	0.12	0.27	0.18	0.18
9	0.06	0.16	0.18	0.35	0.38	0.25
10	0.05	0.06	0.14	0.24	0.52	0.17
11				0.18	0.48	0.16
12				0.10	0.45	0.05

Hence our attempt, crude though it may be, to define 5 degrees of assurance on the basis of a scale of betting odds. After a good deal of consideration we failed to think of any scheme by means of which we could express this assurance in psychological terms, and we therefore employed this widely familiar but extra-psychological definition of our degree of assurance.

It would of course have been possible and would have obviously been the complete procedure, to calculate the constants of the psychometric functions for each of our degrees of assurance. The labor involved in this would have been very great indeed and we still fail to see what advantage can be gained from such a series of calculations. The data are herewith presented for anyone who cares to make them.

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[6] F. M. Urban, *The Application of Statistical Methods to the Problems of Psychophysics*. 1908.

It will be remembered that our problem was one in which we wished to *compare* thresholds for three categories of judgment from the same subject and under the same conditions. Therefore, so long as the conditions of calculation were the same, these comparative results have validity. We perfectly arbitrarily excluded all judgments made on the basis of assurance less than of an even money bet inasmuch as it seemed to us that any judgment with a lower degree of assurance was in the nature of a "guess."[7] Certainly these conditions were clearly stated in our original paper. We still believe that this procedure of standardizing our degree of assurance is one which should be taken into account in future psychophysical work inasmuch as our results seem to clearly indicate the necessity for controlling such a factor.

A study of Tables I to VI indicates that for all *O*s and for everyone of the 3 categories of judgment, assurance 5 is given with a relatively high degree of frequency for the lower stimulus values, while as the number of stimulus dots increased the relative frequency of 5 judgments gradually becomes smaller and smaller and the frequencies of first the 4 and then the 3 judgments gradually increase. A consideration of the results for our category of apprehension shows this most clearly. It was thought therefore worthwhile to calculate the coefficient of correlation between the different values of the stimulus and the degrees of assurance for everyone of the 3 categories of judgments as originally published. The Pearson rank order method was used in these calculations.

TABLE VIII

		5			4	
O	Att.	Cog.	App.	Att.	Cog.	App.
F	+1.00	+1.00	+0.98		+0.50	+0.26
M	+1.00	+1.00	+1.00		+0.50	−0.05
S	+1.00	+1.00	+0.96	+0.50	+0.36	+0.37
Mc	+1.00	+0.99	+0.96		+0.35	+0.46
SC	+1.00	+1.00	+0.96		+0.49	+0.09
JC	+1.00	+1.00	+0.99		−0.50	−0.07

		3			2	
O	Att.	Cog.	App.	Att.	Cog.	App.
F		+0.80	−0.75			
M		+0.40	−0.40			+0.40
S		−0.48	−0.10		−0.50	−0.70
Mc		−0.31	−0.36		−0.28	+0.50
SC		−0.53	−0.62		−0.79	+0.65
JC		+0.50	−0.35		+0.50	−0.32

The results will be found in Table VIII. In the first column will be found the initials of the different *O*s. The table is divided into 4 groups of 3 columns each. In each group will be found the results for the 4 degrees of assurance and in each column the coefficients for each of our 3 categories of judgment. It will be observed that the coefficients of correlation for assurance 5 are all so high as to be of greatest mathematical significance,—12 of the 18 being a perfect positive correlation. 5 of the 6 results that are not of perfect positive correlation are found in the column of assurance 5 for apprehension, which it will be remembered will be found as our most complex form of judgment. Very few of the other coefficients found in the table are of great mathematical significance. Indeed, they were of such little mathematical significance it seemed hardly worthwhile to work out their probable errors. It will also be observed that in the case of only one *O*, (S), does any coefficient appear except for assurance 5 (a coefficient of +0.50 appears for S for assurance 4). This means that for all *O*s except S, all attention judgments were given with assurance 5. A study of Table III will show what a small percent. of judgments were given by S with assurance other than 5.

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[7] All of the *O*s except F worked without knowledge of the procedure and it will be observed that the results of F for assurances 1 and 2 are not included.

This may mean two things. (1) It may mean that we should have included only judgments with assurance of 5 in our calculations. In other words, we have been perhaps too lenient in admitting judgments given as low as an even money bet assurance into our calculations. (2) The exceedingly high positive correlations for attention, cognition and apprehension with assurance 5 (which indicates almost positive certainty) show that all 3 of these systematic categories, as we have defined them, are proper bases for experimentation under the problem as we have set it.

Conclusions

(1) Further treatment of results indicate that attention, cognition and apprehension, as defined, are proper systematic categories for psychological experimentation.

(2) Our attempt to standardize the degree of assurance of the judgments of the different *O*s seems to be valid, although the present results show that in our former study it might have been better to include only those judgments which were given with the highest degree of assurance and to have still further eliminated from the calculations some of the judgments given with a lower degree of assurance which were included.

CHANGES IN AUDITORY ACUITY DURING THE PERFORMANCE OF CERTAIN MENTAL TASKS

By Lee Edward Travis, State University of Iowa

Three years ago the writer began a series of experiments[1] upon changes of sensitivity during certain artificially induced mental states. It was found that some normal individuals and dementia praecox patients experienced reduced cutaneous and auditory acuity during crystal-gazing, reverie and day-dreaming, while other normal persons and psychoneurotics experienced heightened cutaneous and auditory acuity during these states. The mental processes induced seem to be those of unrestrained association. The *O*s attend only to images and ideas that arise involuntarily in consciousness. Their attention shifts from one image or idea to another without any internal effort being made to direct the trend. If a variation in the liminal value can be brought about by unrestrained association it was thought worth while to ascertain if a variation could be produced by some other mental process, such as reasoning, memorizing, or imagining.

Hamlin,[2] in 1896, performed some experiments for the purpose of measuring the effect of attention upon sensible intensity. She found that addition, as the distracting task, could not be used effectively together with judging small differences of intensity, brightness or pressure, because "the subjects usually found that it acted as a spur rather than as a check to the attention. Their attention was wholly absorbed in the addition for a part of the interval, but as a rule the distraction was not continuous, and there came free instants in which the stimulus flashed into consciousness with great clearness and distinctness."[3] This seemed to indicate that sensitiveness for judging small differences was increased rather than decreased by the distracting task.

Peters,[4] in 1906, approached the problem from the measurement of attention. He presented his *O*s with stimuli under three conditions: (1) attention concentrated upon the expected impression; (2) attention concentrated upon a different activity,—as reading, arithmetic or dynamometer tensions; and (3) attention directed to whatever stimulus (light, noise or pressure) that might be given, without expecting any particular one. Conditions (1) and (2) are comparable to those of this study. Peters found a uniform rise of the limen above the normal value under the second condition (attention concentrated upon a different activity). This furnishes experimental evidence for the assumption that concentration with *Einstellung* will give a lower liminal value than concentration without *Einstellung*. Thus we see that these two experimenters arrived at quite opposite conclusions. Since the work of Peters nothing is to be found bearing directly upon the effect of various mental processes on liminal values.

The results of the present paper corroborate those obtained by Hamlin.

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[1] L. E. Travis, Studies in dissociation, *Jour. of Exp. Psych.*, 5, 1922, 338-346; Suggestibility and negativism as measured by auditory threshold during reverie, *Jour. of Abnorm. Psych. and Soc. Psych.*, 19, 1924, 350-368; Test for distinguishing between schizophrenoses and psychoneuroses, *ibid.*, 283-298.

[2] A. J. Hamlin, this Journal, 8, 1896, 1-66. [3] *Op. cit.*, 49.

[4] W. Peters, *Arch. f. d. ges. Psych.*, 8, 1906, 403.

METHOD

The apparatus for determining the auditory *RL* has been described in an earlier Study.[5] Instead of the crystal-gazing situation of the other experiment, various written materials were presented to the *O*s. This material consisted of 17 mathematical problems of sufficient difficulty to be interesting and absorbing yet easy enough to be calculated without the use of paper and pencil; Pearson's discussion of 'Matter' in his "Grammar of Science;" a poem, "The Moral Warfare," by Whittier; and a love-story, "The Victory of Connie Lee," by Aldrich. The materials were presented to the *O*s in the order named.

The *O*'s so-called 'normal threshold' was first of all ascertained. This was found with attention upon the incoming tones. Then the mathematical problems were presented and *O* was directed to solve them, leaving his finger, however, on the response key in order to respond whenever he heard any tones during the solution of the problems. After the *RL* had thus been obtained the *RL* under normal conditions was again found. Then the *RL* during the reading of the passage was ascertained, and then again under normal conditions; and so on for the other tasks, giving 9 measurements of the liminal value for every *O*. The comparisons pertinent in this Study are those made between the *RL*s obtained during the exercise of any mental function and the *RL*s obtained before and after that exercise.

TABLE I
Readings on Individuals

	*Thresholds**											
	Solving Problem			Reading Pearson			Memorizing Poetry			Reading Love-story		
O	Before	During	After	Before	During	After	Before	During	After	Before	During	After
A	12	10	12	24	20	26	20	18	20	26	14	16
B	25	16	30	10	6	10	25	9	10	19	10	19
C	6	1	5	4	1	3	5	2	4	3	3	7
D	14	4	6	6	5	9	6	5	8	6	4	6
E	5	3	5	5	3	4	5	4	4	4	2	5
F	28	15	28	9	10	15	15	11	20	20	7	13
G	12	1	12	5	15	5	12	11	18	5	4	10
H	11	10	13	8	23	6	14	20	14	14	26	8
I	27	23	27	—	—	—	4	4	4	14	10	14

*These thresholds are expressed in terms of ohms resistance.

RESULTS

Nine *O*s served in this Study; 8 were graduate students in psychology, and 1 was a patient of the psychopathic hospital. The latter, a case of conversion hysteria, a school teacher of ability and success, was, however, essentially adjusted in all spheres.

Table I shows that the majority of the *O*s experienced a decrease of the *RL* during the performance of all four of the mental tasks. Only 1 *O* showed an increase in the *RL* in three of the mental operations. No *O* failed to show a decrease of the *RL* during the performance of at least one of the mental tasks. Let us take the four mental functions separately.

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[5] *Jour. of Abnorm. Psych. and Soc. Psych.*, 19, 1924, 350-368. The stimulus was a clear tone of constant pitch. The intensity of the tone was varied by means of a series of non-inductive resistances. The threshold of the "normal period" was ascertained with the attention concentrated upon the incoming tones. During the solution of the problems, the memorization of the poem and the reading of the two forms of printed matter, the attention was directed upon the different activities. *O* was told, however, that he would be given tones, and, if he should hear any, to respond as he had done previously.

During the solution of the problems every *O* reacted with a lowered *RL*; during the reading of the passage from Pearson, 5 had lowered *RL*s, 1 had a *RL* between that of the pre- and that of the post-reading periods, and 2 had raised *RL*s, 1 *O* was not tested in regard to this operation; during the memorization of the poetry, 6 had lowered *RL*s, 1 a raised, and 2 experienced no change; and finally, during the reading of the love-story, 7 experienced a lowering, 1 a rise, and 1 no alteration of the *RL*. These facts are more clearly shown in Table II.

TABLE II
Summary of Table I

	During Solving Problems	*During Reading Pearson*	*During Memorizing Poetry*	*During Reading Love-story*
No. of *O*s with lowered *RL*	9	5	6	7
No. of *O*s with raised *RL*	0	2	1	1
No. of *O*s with no change in *RL*	0	0	2	1
No. of *O*s with *RL* between that of the previous and subsequent periods	0	1	0	0

DISCUSSION

One of the main conclusions of a previous Study by the author was the identification of suggestibility with a lowered *RL* and of negativism with a raised *RL*. As has been stated above, the characteristic mental state of crystal-gazing is that of unrestrained association, and from the results of the Study it seemed that some individuals were capable of entering into this state while others were not. The hysteric and the suggestible normal individual seem to be able to lose themselves in reverie and approach a state of auto-hypnosis while the dementia praecox and the negativistic normal individual resist such change. Thus the ability to become mildly dissociated under crystal-gazing conditions is prerequisite for a lowered threshold or heightened sensory acuity. The lack of this ability would give no change in the threshold while active or passive resistance to any such procedure as crystal-gazing or unrestrained association would give a rise in the liminal value. The crystal-gazing situation then becomes the hinge on which swings the differentiating power of the test. It is not to be thought that the present Study can be used for any differential indicator, for some of the individuals who experienced a rise in the crystal-gazing threshold had a lowering of the limen during some or all of the mental operations used in the present experiment. This can be explained on the basis that there is a very marked difference, if not an opposition, between free association and the attitude of facing a task such as solving problems or memorizing poetry. Individuals who would be opposed to the former and hence fail to lose themselves in the crystal-gazing situation may be able to become absorbed in impersonal tasks or mental operations that would not involve catching them "off-guard" in regard to some of their personal fancies.

Our interpretation of the data of this Study would be then that a lowered *RL* or heightened sensory acuity in regard to an impersonal mental operation is indicative of complete absorption in that mental task or operation and that a heightened *RL* during the performance of a certain mental function is indicative of inability to become completely lost in the operation, or of an inhibition to the execution of the task. This is borne out by introspective data gathered from the *Os*. Every *O* found the problems interesting and enjoyed doing them, and it is to be noted that they experienced a lowering of the *RL* during their solution. *F*, *G* and *H* thoroughly disliked the dry and involved discussion in the passage from Pearson, and the table indicates that their *RLs* rose during the performance of this task. *H* also found the memorization of the poem too much like work and the love-story not at all interesting, and consequently he failed to get into the tasks at all.

This conclusion raises certain questions in regard to attention. It seems to indicate that a fainter tone can be heard if attention is not focused directly upon the tone but upon something else. The teachings of current psychology[6] hold that attention upon a stimulus increases the intensity of the stimulus, while distraction decreases the intensity. For example it is taught that one can hear with attention, a faint sound that one cannot hear if he does not attend. Now the difficulty seems to be in the idea that one has to attend to the faint sound in order to hear it. The results of this Study indicate that the function of attention, rather than the direct object of attention, is requisite for heightened acuity and to attend to something else would be more conducive toward hearing the faint sound than to attend directly to the sound itself. This apparent contradiction might be explained by the fact that if heightened sensory acuity during the performance of a mental task indicates complete absorption in that task, then just attending to tones is not sufficient to bring about a state of complete absorption and hence not capable of producing maximal sensitiveness of the organism. This would seem to be the case as there is not sufficient challenge in a simple tone to command complete absorption of attention. Thus it appears in the last analysis that the *degree* of attention determines the liminal value of a stimulus regardless of whether the stimulus is the object of primary attention or some other object that may come under the direction of attention.

Experimentalists[7] interested in attention have published much in regard to its effect upon the apparent intensity of the stimulus, but their works have added nothing new to the relevant theories of attentional intensification in the ordinary sense. Most of these theories offer a physiological explanation or nervous correlation which generally means a facilitating or reinforcing process.

The present Study does not offer a new theory of attentional intensification or necessarily a contradiction of old ones but it seems to have opened up a new way of looking at the problem. Instead of seeing a facilitating or reinforcing process in connection with only the sensory field attended to, may not one view it as rather a reciprocal innervation process spreading to all sensory fields and thereby causing generalized heightened acuity.

More extensive research along many lines of approach is necessary to determine further the value of a new point of view of attention.

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[6] E. B. Titchener, *A Text Book of Psychology*, 1909, 280. W. B. Pillsbury, *Attention*, 1908, Chap. I.

[7] In a recent work by S. N. Newhall. Effects of attention on the intensity of cutaneous pressure and on visual brightness, *Arch. Psych.* 9, 1923, 5-75, we read: "we may apparently conclude in general, that attention increases the intensity of moderate and liminal areal pressures, and the degree of moderate visual brightness when near the difference-limen. The results in both sense fields seem to indicate that on the one hand attention increases the number of stimuli which are superthreshold, and on the other hand, that attention increases the intensity of pressure and the degrees of brightness of the respective stimuli when they *are* superthreshold."

BOOK REVIEWS

Der Strukturbegriff in der Psychologie. By Felix Krueger. Jena, Gustav Fischer, 1924, pp. 26.

The concept of structure is reviewed in its historical, philosophical and psychological settings in this little *Sonderabdruck* from the reports of the eighth congress for experimental psychology held at Leipzig (1923). Interwoven with all of this material is a criticism of the concept of structure as it has been used by the members of the school of *Gestalt.* While asserting that problems of individuality and personality demand the concept of psychic structures and that totalities, wholes, must be studied for their own sake descriptively, Krueger attacks such attempts as have been made to bring order out of a welter of opinions. We thus find both praise and blame which makes no distinctions: Wundt is given credit for having stressed *Ganzheitsmomente* along with Wertheimer, yet no distinction is made between the absolutely different points of view from which these two men approach the problem of configuration. The author insists that *Gestalten* may be single atoms while the whole still remains to be treated. But precisely what the totality is and how one would go to work in the laboratory to investigate it, remain *dahingestellt!* Krueger's scythe cuts both ways; the path he tries to follow is too broad and leads him in circles.

A structured *Seelenwesen* is the real necessary presupposition for all psychic phenomena. A more enduring reality must be postulated for psychic events. Dispositions are enduring. By means of analysis and synthesis we can study part structures. Then the old psychology comes in for its share of criticism in following the false idols of physics and speaking in terms of fusion of elements and the like. Psychology must have a system of laws but talking about the total universe certainly will not give it to us. No definite constructive picture emerges from Krueger's discussion: it is a bundle of notions in the jargon of philosophy, psychology, logic, and theory of value and out of this no scientific methodology emerges.

Some of Krueger's criticisms are to the point if one takes the trouble to disentangle them from an almost hopeless context. Several of them are as follows. Not every totality is a configuration. Temporal configurations offer many difficulties (See the reviewer's article on "The Psychology of *Gestalt*" pp. 25-62 supra). Wertheimer and his followers have not paid enough attention to the feelings as integral and important elements in the total psychic life of the individual. The word *Gestalt* should be limited, as Müller has pointed out in his book *Komplextheorie und Gestalttheorie.* Formed character and total character must be distinguished.

Just as his criticisms of *Gestalt* and other concepts are meagre, so are his comments on phenomenology which is playing such a prominent part in German psychology. Krueger points out that the assumptions and assertions of phenomenology, no matter what they may mean for logic, require thorough psychological investigation. The presence of an act in all experience does not seem to him a proved fact, indeed, it is contradicted by the unreflective behavior of animals and children. The further criticism of normative and metaphysical abstraction in phenomenology do not seem to the reviewer to concern the phenomenology which one finds in present-day psychology. However, Krueger has hinted at several things which make us await with interest the promised book to appear on the concept of structure in psychology.

University of Illinois — Harry Helson

Old and New Viewpoints in Psychology. By KNIGHT DUNLAP. St. Louis, C. V. Mosby Co., 1925, pp. 163.

The lectures and papers contained in this little book were in part designed for popular audiences, but because of certain systematic and scientific views they invite attention from the psychologist. The chapter on mental measurement, the first lecture, presents a sound view of the aims and methods of individual psychology. The author expresses the fact too seldom recognised that mental tests measure intelligence, defined as the ability to acquire knowledge, indirectly by measuring acquired knowledge. Very apt also is the warning that the tests are merely tools and are valuable only in proportion to the insight and psychological training of the tester, and the reminder that mental measurement as an applied science can proceed no faster than purely scientific research will permit.

The second lecture attempts to show that certain schools of psychology have been nothing more than eddies in the main stream of 'plain psychology.' Although the author is quite able to define these by-movements, such as behaviourism, Freudism, and instinct-psychology, aside from a doubtful characterisation of the 'reactionary structuralists' —whose method he confuses with the method formerly known as introspection—he does not state specifically what the methods or problems of 'plain psychology' are. The point that specialists in other fields regard the domain of psychology as a proper field for speculation will be appreciated by all psychologists.

The remaining chapters, especially those on spiritism and on character analysis, invite the layman to take a scientific attitude toward the problems in these fields.

On the whole, one comes away from the book with a feeling that the title is a misnomer. Only in the second chapter does the author touch upon fundamental points of view, old or new; the remainder of the book is given over to the less fundamental by-products of the science.

Cornell University J. P. GUILFORD

Pleasure and Behavior. By F. L. WELLS. New York, D. Appleton and Co., 1924, pp. xvi, 267.

The purpose of this volume, we are told in the introduction written by Joseph Jastrow, editor of the *Science of Mind Series* in which this book appears, "is primarily to set forth the foundations of the affective life, so that a comprehension of the natural setting of pleasure in its relation to behavior may lead to a truer insight into the control and the ordering of desire in the artificial setting of today" (xv). Since the aim of the volume is practical "the author assumes responsibility for its scientific character" (xi). To the layman, therefore, the book may appeal as an interesting set of observations upon behavior, morals, marriage, and the like, from the standpoint of a psychologist; to the trained psychologist it seems to be a concatenation of common-sense observation presented in the form of hasty generalization supported by doubtful scientific evidence and a hopeless confusion of fundamental psychological concepts which should be handled clearly and cautiously in popular as well as in scientific discussions.

The terms brain, mind, behavior, memory, attention, and even psychology itself, are used indiscriminately and with no precise or traceable meanings. If we take the definition of psychology offered by the author seriously then it is hard to understand what is meant by a great deal of what follows. Thus we are told that the subject-matter of psychology is the "behavior of the organism as a whole," which is certainly not objectionable if one cares to define it in that way, but when the author proceeds to speak of "the will to activate a striped muscle group" (11), of bodily and mental pleasures, of memory as an after-image, and the control of attention, it is hard to know precisely what he means. After the foregoing definition of psychology the following statement seems to violate the canon of consistency: "The pleasures of imagination and hope can be described in mental terms alone" (30). In fact it is asserted that there may be a "blending" of bodily and mental factors in pleasures. Some statements taken merely on the popular level are incomprehensible to the reviewer: "The brain keeps memories intact and serviceable." "The psychological difference between smooth and striped muscle lies in that the latter contracts and relaxes promptly on receipt and cessation of its impulse; while smooth muscle is very much slower to act" (10). A reflex "is aroused by instinct and stimulated by trial and error" (14). So much for the psychological medley.

Fields other than psychology come in for consideration in this book since the quest of pleasure may lead one anywhere. The three great sources of pleasures are friends, wealth, and love, hence social, economic, moral, and legal matters must be discussed as they bear on the satisfactions of the individual. Thus a consideration of sexual pleasures leads to the problems of the double standard, divorce and marriage. We find that "inward traits of personality are rather unfavorable to unions as close as marriage" (137). Psychology and sociology join hands in a consideration of intelligence and social organization, language and communication. The relative importance of instinct and intelligence for social organization seems never to be clearly settled for, while the author seems to adhere to McDougall's view that the instincts are the prime movers of society, he asserts that "intelligence" is more important than instinct after all. Again we are told that intelligence alone is impotent, instinctive drives are necessary. When we come to the complex types of behavior with which law, ethics, and religion must deal the pleasure principle is supplemented by "loyalty to an in-group" and the statement is made that "goodness and happiness may be brought into amicable relations" (162) with the implication that something more than the hedonistic principle is necessary to bring them together. The two fundamental postulates seem to be that the satisfaction of fundamental drives gives pleasure, thwarting them gives pain. Religion, comradeship, wealth and love may serve important compensatory functions in life for fundamental drives which cannot be directly satisfied (sublimation?). But the pleasure principle still needs a prop for "the acclaim of history is for those who gave to life; the pleasure value to the giver is a secondary consideration." The obvious implication is that while pleasure may influence behavior there are other causal factors at work in human motivation.

One wonders what scientific evidence there is for several generalizations of the following sort: "Women, in general, find more pleasure in the courtship of men who have passed through a baptism of love." "Save as love-objects men and women do not understand one another so readily as they do those of their own kind." "History indicates that the dominant peoples of the world have been those that responded to Christianity." "It takes a much more developed mind to worship something seen with the eye of faith alone."

The book will appeal to many people interested in a superficial way in a variety of problems gathered together under attractive headings. The style is simple and attractive. The comments and observations betoken a wide range of reading. The book is suggestive of what an applied psychology might be. It cannot be taken seriously by the reader seeking psychological principles or their application to definite problems. It is hard to say for whom the book is suited in view of its slight scientific value and the wrong impression it may give the lay reader of what psychology really is.

University of Illinois HARRY HELSON

The Problem Child in School. By MARY B. SAYLES. Joint Committee on Methods of Preventing Delinquency, 1925, pp. 287.

Three Problem Children. Joint Committee on Methods of Preventing Delinquency, 1925, pp. 146.

The Visiting Teacher in Rochester, By MABEL BROWN ELLIS. Joint Committee on Methods of Preventing Delinquency, 1925, pp. 205.

Data collected during the last fifteen years have made the educational profession acutely conscious of the child who fails to make normal progress in school. Whether retardation be due to low mental ability, or whether it be caused by poor teaching, a formal curriculum, deficient eyesight, lack of proper encouragement in the home or any one or more of many other factors, it is obvious that society is vitally concerned. Not only do the repeaters increase the cost of the schools but they tend to drop out of school early and often become problems in delinquency for the civil authorities to deal with. In the three volumes named above the Joint Committee on Methods of Preventing Delinquency has presented some aspects of its work. The first volume analyzes some 25 cases of children classified as problems because of parental attitudes, feelings of inferiority, dishonesty, or sexual abnormality. In each case the situation in which the child was found is fully described and the methods employed in attempting to meet the situation are given. In the second volume three cases are described in considerable detail while the results of the physical and psychological examinations are given in the appendix. In the third volume the work of the visiting teacher as it is carried on in the city of Rochester, N. Y., is described. Information concerning the development of the visiting teacher in that city is presented and the organization explained in some detail. About half the book is devoted to "What she does and how" in which the visiting teacher is followed from hour to hour and her methods of work described. In the year 1923-24 this department of the Rochester schools dealt with 3,894 cases. All three volumes are written in untechnical language, and, since the case method is generally employed, the stories given are gripping. Parents, teachers, and administrators will find a perusal of these volumes much worth while. Further activities of this Committee should be followed with interest by all socially-minded persons.

Cornell University JULIAN E. BUTTERWORTH

Instinct; A Study in Social Psychology. By L. L. BERNARD. New York, Henry Holt and Company, 1924, pp. viii, 534.

This book is an argument against certain conceptions of instinct; it is an avowed attempt to show that the psycho-social environment is of far greater importance in the making of the human individual than is heredity. The argument begins with the assumption that, since instinct is generally defined as inherited behavior, it must be inherited according to Mendelian units. Such a unit cannot be a sophisticated abstraction or even a "tendency" to act in a certain direction, but must rather be some definite nervous structure of which the behavior is a result. The author defines instinct as "a specific response to a specific stimulus or set of stimuli."

After discussing the nature of instinct and its organic basis, the author presents a survey of the literature, scientific and non-scientific, which shows all possible uses of the term. The list, secured from 323 authors, includes 847 classes of general instincts and 5,684 specific responses grouped into 1,594 classes. Testing these instincts in the light of his own definition, he finds that not one is a specific response to a specific stimulus. By analysis there may be found in every one, *e.g.*, the so-called instinct of criminality, a large number of more or less definite responses which may also be found in many other so-called instinctive acts. The question now arises, what is the organising agent which brings these interchangeable units into an integrated act which has been characterised as an instinct? The author rejects the assumption of McDougall that emotion is the integrating agent, the core, or characteristic part of the instinct,—as for example, the tender emotion is the core of the maternal instinct,—chiefly because there is no original structure for this emotion which can be inherited as a Mendelian unit. He maintains that there is only one other possible organising agent, and that is the environment. The inborn basis of behavior is merely a few discrete, simple responses to stimuli upon which environment builds an immense superstructure by supplying the proper stimuli or combinations of stimuli. What McDougall and others have called instincts are really habit-complexes which continue to grow under the influence of environment until "the child who has reached a rational age is reacting in nine-tenths or ninety-nine one-hundredths of his character directly to environment and only in the slight residual fraction of his nature directly to instinct." Society has only to manipulate the child's environment in order to control his education.

This book is undoubtedly the most noteworthy systematic contribution to the subject since McDougall's *Introduction to Social Psychology* and one which will be heartily welcomed by sociologists, and perhaps by educators. The survey of the literature is very complete and the author's conclusions form the basis of his attack. The book also opens up new avenues of investigation. Are there Mendelian units of behavior? If so, what are they, and how complex may they be? What is the organising effect of environment or what is the relation of environment to integrated behavior?

The argument, however, may be partially motivated by the desire to find controllable factors in the social environment of the child. The *reductio ad absurdum* of the instinct of criminality is made easy by choosing an instinct regarded as absurd without reduction by many scientists. The problem of integration of the unit responses into functional complexes may be solved in more than the two ways discussed, as the *Gestalt* psychologists have suggested. To substitute the environment for emotion as the organising principle merely shifts the problem of integration from mind to the stimuli which act upon it.

Cornell University J. P. Guilford

Hypnotism and Suggestion. By Louis Satow. Translated by Bernard Miall. Dodd, Mead and Co., 1924, pp. 283.

This book is written for the lay reader, and is more an historical treatise on the origins of our present forms of suggestion—hypnosis, religion, war, panic, "booms," rabid nationalisms, etc.—than an effort at a psychological analysis of the subject. Though the book is provided with a glossary adapted from Paul's translation of Baudouin's "Studies in Psychonalysis," no reports of original researches on the nature of the phenomena are given.

The book deals quite adequately and succinctly with the history of hypnotism; sketches briefly the origin of religion; discusses the theories of the relation of mind and body; and, above all, is frankly a philippic against religion, nationalism and militarism. The chapters on the phenomena of hypnotism and therapeutic suggestion follow the usual lines (*e.g.*, of Moll, Wetterstrand, Grasset), except that the details here are not so numerous, nor are both sides of doubtful questions given. For example; it is taken for granted that the perceptual powers can be greatly augmented in hypnotism (p. 120), and that posthypnotic amnesia is the differentiating characteristic of the deepest, the somnambulistic, stage of hypnotism (p. 76). The Reviewer's researches in hypnosis (*cf.* this Journal, 36, 1925, 214-232) would lead him to deny both of these assumptions. The chapter on mass psychology is sanely written.

Satow traces hypnotism and all other forms of suggestibility to native credulity resting on an instinct of imitation. The remedy for the present evil state of mass-mindedness which is seen in all religions (no distinctions are made), in our narrow nationalisms, in race hatreds, and in militarism, is knowledge. The education of the future must raise the individual above the level of mass suggestibility. Yet, strangely enough, by continued propagation of the right ideas these ideas will filter down to the subconscious minds of the masses; so that, after all, we shall be taking advantage of the blind force—suggestibility—that now enslaves the world. The optimism shown in the last few pages, after the black picture painted in the rest of the book, gives it almost a Shellyian aspect.

Louisiana State University PAUL CAMPBELL YOUNG

Lazarus. By HENRI BÉRAUD, translated by Eric Sutton. New York The Macmillan Co., 1925, pp. 187.

This book will be of special interest to students of the abnormal phases of psychology, for the story is about a case of double personality. The author draws a vivid and dramatic picture of a man who has forgotten his past and who is now haunted by his former self. The story opens with the awakening of Jean Mourin, a musician, a man with all the refinements and the temperament of that profession. He is in a hospital which he recognizes though he knows perfectly well that he has never before seen it. Because he feels no discomfort, no illness nor injury, he calls out; and when the nurse comes he demands to know, to her utter amazement, where he is and why. He is overwhelmed to learn that sixteen years previously he had been seriously injured in a motor accident in which his wife and all the other members of the party had been instantly killed, that on recovering from his injury he had retained none of his memories of the past, and that he had during the intervening years been employed in the hospital as a tailor. In this personality he is docile and tractable and responds only to the name Gervais. Shortly after his awakening, Jean Mourin is given his discharge from the hospital and, like Lazarus of old, he returns to the world as from the dead. He finds that all his friends and acquaintances have died during the years of his absence, and that except for the haunting presence of Gervais, of whom he is fearful and from whom he continually flees, he is utterly alone. His struggles and fears, his thoughts and actions during his hours of freedom are vividly and realistically described. Béraud has woven into an interesting novel the psychological facts of double personality.

Cornell University C. V. BROOK

An Introduction to Objective Psychopathology. By G. V. HAMILTON. St. Louis, C. V. Mosby Co., 1925, pp. 344. With a foreword by ROBERT M. YERKES.

Not many of the numerous books on psychopathology in recent years make a distinct contribution to the subject. Dr. Hamilton's volume is an exception. Although its point of view and its contents cannot startle the world as Freudism has done, it promises to be of more permanent scientific value.

The author's aim, as the title suggests, is to study abnormal behavior from the objective point of view, making use of the methods of comparative and genetic psychology. The results of observation do not form a departure for speculations about an unconscious, but are stated in a form to suggest certain behavior types. One is, however, a little irritated to run across such expressions as "pathologically persistent nonadjustive affective reactions to baffling somatic discomforts and disabilities."

Approximately one-half of the book is devoted to detailed descriptions of the reaction tendencies of 200 nervous patients. A statistical treatment shows that the most frequent abnormal behavior tendencies are "persistent nonadjustive affective reactions" to baffling situations, somatic or social, or "indirect reactions" and "conditioned reactions."

The remaining pages are given to a constructive, systematic treatment of the facts of psychopathology, which the author properly defines as a branch of medicine. Many Freudian mechanisms will be recognised under new names, but names which now refer to observable behaviour, which includes a report of the psychological experiences of the patient, rather than to mystical entities. A report of comparative studies of twelve species of mammals in similar baffling situations is very enlightening, and leads the author to conclude that the best cure for nervousness is to bring the patient to a rational understanding of his symptoms. A final chapter on sex behavior agrees in part with Freud's theories, but in places it conflicts with them. Wherever there is disagreement one is inclined to accept the undisputed facts of comparative behavior. A glossary of medical terms makes the book accessible to the non-medical reader. A bibliography of 126 references is given.

Cornell University J. P. GUILFORD

NOTES

DR. JOHNSON ON THE MEASUREMENT OF ATTENTION

The problem of the measurement of attention is of importance from both a practical and a theoretical point of view; consequently many attempts with various methods have been made to solve it. Geissler,[1] in an historical survey, arranged the methods in 6 groups: "The first 5 may be classed together as methods of expression analogous to the physiological methods employed in the investigation of the affective processes. According to these 5 methods the degrees of attention may express themselves in changes of (1) peripheral vision; (2) muscular strength; (3) liminal and differential sensitivity; (4) reaction-time; (5) accuracy of work. The sixth method may be likened to the method of impression. By a series of graded distractors, different degrees of attention are to be induced in the *O*, and he is afterwards to report which degree he experienced." Not one of these methods has given univocal results, and, for one reason or another, not one of them seems to be adequate to the problem.[2] I am here concerned only with (4),—the method of measuring degree of attention by speed of reaction.

Obersteiner, in 1879,[3] was the first to determine degrees of attention by means of reaction-time. He had two distractors,—an induction current and a music-box,—and he found that under distraction his *O*s' reactions were slower than under normal conditions; "this retardation," he concluded, "stands in inverse proportion to the intensity of attention;"[4] but "the differences in the reaction period, which may serve directly as the measure of attention, vary in different individuals, and in the same individual under different conditions."[5] The results of this first attempt had thus to be qualified. Since Obersteiner, many investigators[6] have tried to obtain an objective measure of attention by way of the reaction method; the results have been flatly contradictory.[7]

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[1] L. R. Geissler, The Measurement of Attention, this JOURNAL, 20, 1909, 492.

[2] *Cf.* L. R. Geissler, *op. cit.*, 493-502.

[3] H. Obersteiner, Experimental Researches on Attention, *Brain*, 1, 1879, 439-453.

[4] *Op. cit.*, 444.

[5] *Ibid.*

[6] G. Buccola, La durata del discernimento e della determinazione volitiva, *Rivista di filos. scientif.*, 1, 1881, 19 ff.
G. S. Hall, Reaction-time and Attention in the Hypnotic State, *Mind*, 8, 1883, 170-182.
E. J. Swift, Disturbance of Attention, this JOURNAL, 5, 1892, 1-19.
J. McK. Cattell, The Time taken up by Cerebral Operations, *Mind*, 11, 1886, 220-242; 377-392; 524-538.

The results of the other methods are no less equivocal. Consequently, when Geissler turned to the problem of measuring attention, he decided to strike out upon a new tack. He proposed to measure attention in terms of clearness,—the single characteristic of attention upon which all writers, theoretical as well as experimental, seemed to be agreed. This attempt led to positive results. Geissler, however, worked only with material that was visually presented; hence his results were subject to the criticism that they were obtained in a sense-department that had itself a focus and a margin, and was itself differentiated into degrees of optical clearness or distinctness. Before his method could be accepted, and before the conclusion that attention was measurable in terms of attributive clearness or attensity[8] could be drawn, his investigations had to be carried with positive results into other domains of sense.

In 1913, and again in 1916, I extended Geissler's method to the fields of auditory and cutaneous sensation. In the earlier[9] of these two Studies, the *Os* were directed to report all changes, in pitch and intensity, of the tonal stimulus; and in the later,[10] all extensive and intensive changes of the cutaneous stimulus. Description of the changes and subjective estimation of the clearness of content at the times of change were given by the *Os* in their verbal reports. Because a close parallelism was found to exist between the introspectively distinguishable variations of clearness and the accuracy and precision of the work performed at the various levels of clearness, I felt justified in concluding, with Geissler, that "attention can be measured introspectively in terms of attributive clearness."

W. James, *Principles of Psychology*, 1, 1890, 95, 425, 427-434.

S. E. Sharp, Individual Psychology: A Study in Psychological Method, this Journal, 10, 1899, 344, 356 f., 379 ff.

A. Kästner and W. Wirth, Die Bestimmung der Aufmerksamkeitsverteilung innerhalb des Sehfeldes mit Hilfe von Reaktionsversuchen, *Psych. Stud.*, 3, 1907, 361-392; 4, 1908, 139-200.

L. R. Geissler, A Critique of Professor Wirth's Methods of Measurement of Attention, this Journal, 20, 1909, 126 ff.; The Measurement of Attention, *ibid.*, 508 f., 514.

I. Grassi, Einfache Reaktionszeit und Einstellung der Aufmerksamkeit, *Zeits. f. Psych.*, 60, 1911, 46-72.

J. V. Breitweiser, Attention and Movement in Reaction Time, *Arch. of Psych.*, No. 18, 1911, 1-48.

K. M. Dallenbach, The Measurement of Attention, this Journal, 24, 1913, 483 ff., 501 ff.

H. Woodrow, The Measurement of Attention, *Psych. Mon.*, 17, 1914, No. 76, 1-158; Reactions to the Cessation of Stimuli and their Nervous Mechanism, *Psych. Rev.*, 22, 1915, 444 f.; Outline as a Condition of Attention, *Jour. Exp. Psych.*, 1, 1916, 23-39.

K. M. Dallenbach, The Measurement of Attention in the Field of Cutaneous Sensation, this Journal, 27, 1916, 453 ff.

E. E. Cassel and K. M. Dallenbach, An Objective Measure of Attributive Clearness, this Journal, 29, 1918, 205 ff.

F. L. Wells, C. M. Keller and G. Murphy, On Attention and Simple Reaction, *Jour. Exp. Psych.*, 4, 1921, 391-398.

J. C. McComas, A Measure of Attention, *Jour. Exp. Psych.*, 5, 1922, 1-18.

H. M. Johnson, Reaction-Time Measurements, *Psych. Bull.*, 20, 1923, 578-580.

K. M. Dallenbach, Dr. Johnson on Reaction-Time Experiments, this Journal, 35, 1924, 305-307.

H. M. Johnson, The Definition and Measurement of Attention, this Journal, 36, 1925, 604-613.

[7] Cattell, in 1886 (*op. cit.*, 242), found that there was no corresponding lengthening of the reaction-time with reduction of attention. Geissler, in 1909 (*op. cit.*, 498), wrote, "the final outcome of the reaction experiments, as used for the measurement of attention, has been on the whole negative; it has been impossible to establish a positive correlation between high degrees of attention and short reactions, and between low degrees and correspondingly lengthened reactions." Woodrow, in 1914 (*op. cit.*, 139), said, "that reaction time may vary without corresponding variation in attention." Johnson, in 1925 (*op. cit.*, 612), asserted, "that knowledge of a particular reaction-time . . . . does not enable prediction . . . . of the unknown clearnesses associated with them."

[8] E. B. Titchener, The Term "Attensity", this Journal, 35, 1924, 156.

[9] *Op. cit.*, 474, 492.

[10] *Op. cit.*, 443-445.

In both Studies graphic records were made of the objective results. One line on the kymograph showed the time of change; a second line, *O*'s judgment of that change. One 'tick' in the line denoted a change of intensity; two 'ticks', a change of pitch (in the first Study) or of extent (in the second Study). A Jacquet chronoscope, writing fifths of seconds, was placed between these two lines, and reaction-times were taken directly from the records.

What to do with these 'reaction-times' after they had been calculated was a question! To ignore them would, it seemed to me, be unscientific; to compute, from the scatter-graph, the correlation ratios between the gross measures would, after the failure of the earlier attempts, be futile. I had another choice,—to work the data over from a new angle. Rather than discard the data, or repeat a method that had proved useless, I decided to draw a correlation between degree of clearness and average reaction-time. I based the procedure of averaging on the theory that clearness was not the only determinant of the length of the simple sensory reaction, that many other unknown and adventitious conditions were also effective, and that the averaging of the reaction times that were paired with the various degrees of clearness would cancel out the effect of all the uncontrolled determinants and would thus reveal the true relation between clearness (the only condition whose variations were known) and reaction. "The *average reaction-time* was computed for each degree of attention as estimated by the clearness of mental processes,"[11] and the correlation was computed between these two arrays. Though the correlations were high, I qualified my conclusion "that the average reaction-time would index attention," because my *O*s were not working under reaction instructions;[12] they did not know that their reactions were being measured, and so they were not under the compulsion to react as soon as possible after perceiving a change. Indeed, as I pointed out in both Studies, the *O*s at times delayed their reactions;—particularly "if the change occurred at a critical point they might voluntarily delay their reaction until a more favorable opportunity to react presented itself."

Since my times were not "reaction-times" in the true sense of the term, I was glad, while working with Miss Cassell in 1918,[13] to avail myself of the opportunity of testing the average reaction-time method and the proposition that clearness and length of sensory reaction are related.[14]

Our experiment fell into two parts. In the first part, *O* estimated, after every series of 10 reactions, the clearness of the mental processes during that period. Under our conditions, the average time of the simple sensory reaction was inversely correlated with the introspectively reported clearness-values. The results, however, were open to two obvious objections: "(1) that *O*'s reports, inasmuch as they were delayed until after the completion of 10 reactions, partake more of the nature of vague impressions, of guesses, than of true introspective descriptions; and (2) that the figures, inasmuch as they are averages of averages, are artificial and not representative."[15] To meet these objections and further to check the method and results, *O*, in the second part of the experiment, reported immediately after every reaction (700 in all) the clearness of the mental processes during the reaction. Correlation was computed as before between the different clearness values reported and the averages of the reaction-times for the different clearnesses. Since the results of the two parts of the experiment agreed, and the correlations thus obtained were high and persisted through marked shifts of attitude, we felt justified in concluding that "unde rour conditions of training, attributive clearness may be measured by the average duration and mean variation of the simple sensory reaction."[16] We explicitly forebore generalization, as the following sentence from our conclusion shows, "Since they [the results] were obtained from a single *O*, who was highly practiced both in reaction and in the introspection of clearness, we have no right to generalize."

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[11] *Op. cit.*, 1913, 501. Italics in the original. [12] *Opp. cit.*, 1913, 485, 502; 1916, 455 f.

[13] E. E. Cassel and K. M. Dallenbach, The Effect of Auditory Distraction upon the Sensory Reaction, this JOURNAL, 29, 1918, 129-143.

[14] E. E. Cassell and K. M. Dallenbach, An Objective Measure of Clearness, this JOURNAL, 29, 1918, 205.

[15] *Ibid.*, 206.

In his review of reaction-time experiments, Dr. Johnson[17] considered our experiment, and criticised us for not "treating each paired measurement as a single datum."[18] Such treatment would be futile, as Dr. Johnson himself shows by obtaining from our data a correlation that is only "3 percent more reliable than guesswork,"[19] and as any one conversant with the literature on the problem knows. Regarding our correlation between clearness and average reaction-time, Dr. Johnson says, "such treatment disregards the variations of time corresponding to each degree of clearness."[20] Yes; as I pointed out in my Note written in reply to his criticisms,[21] it was just such variations that we wished to disregard or to eliminate by averaging. "We thought, by averaging the reaction-times that were paired with the various degrees of clearness, to eliminate the effect of the other conditions (unknown and uncontrolled) and thus to discover the relations that existed between clearness and reaction."[22] Against this procedure he raised no objection; his criticisms were therefore irrelevant,—he missed the point of the experiment entirely.

In his rejoinder,[23] Dr. Johnson (*1*) accuses me "of abandoning the proposition" that "reaction-time is closely correlated with clearness;" and (*2*) complains that he does not know "the meaning of the expression 'average duration' or 'average reaction-time,' "[24] *i.e.*, that he does not understand our method. Regarding (*1*) he is in error; we never maintained that proposition. From our knowledge of the earlier work we knew that the proposition was untenable. Every reference, every conclusion, explicit or implied, referred to the *average* reaction-time as the measure of clearness. Averaging was the very 'essence' of our method. How this escaped him I am at a loss to know, for 'average' appears as a qualifying word in our table captions and in our conclusions. The only place in which it does not appear is in a footnote; but this should not have confused him as the sentence to which the footnote was appended contained the specific qualification.[25] Regarding (*2*): if the meaning of 'average duration' or 'average reaction-time' is not clear from Miss Cassell's and my report, it should have been clear from the exposition of the method given in my Note, where I said, "We give, in Table II, (1) the different clearness values reported, (2) the frequency of their report, (3) the average reaction times for the different clearnesses, and (4) the mean variations of these averages. We computed the correlations, . . . between clearness and average reaction-time, *i.e.*, between (1) and (3)."[26] Yet he complains that he is unable to understand what we did! He asks: Does average reaction-time "refer specifically to the means of times which were associated with specific class-values of clearness [restricted sense]; or does it refer to the average of times taken without regard to the numerical values of clearness associated with them [unrestricted sense]?"[27] Because he is unable to decide which is meant he examines both; and shows that the 'unrestricted sense' is untenable. With this argument, as with his previous mathematical argument, I am in substantial agreement,—which is to say that I regard it as irrelevant.

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[16] *Ibid.*, 207.

[17] H. M. Johnson, Reaction-time Measurements, *Psych. Bull.*, 20, 1913, 562-589.

[18] *Op. cit.*, 570. [19] *Op. cit.*, 580. [20] *Op. cit.*, 570.

[21] K. M. Dallenbach, Dr. Johnson on Reaction-time Experiments, this JOURNAL, 35, 1924, 307.

[22] *Ibid.*

[23] H. M. Johnson, Definition and Measurement of Attention, this JOURNAL, 36, 1925, 601-615.

[24] *Op. cit.*, 608. [25] *Op. cit.*, 207 [26] *Op. cit.*, 306. [27] *Op. cit.*, 608 f.

Though he admits the truth of our conclusion, that "attributive clearness may be measured by the *average* duration of the simple sensory reaction," if 'average duration' be taken in the 'restricted sense,' he nevertheless raises the following objections against it. "(1) It is without practical significance,"—with its practicality we were not at all concerned. "(2) The assertion [our conclusion] is misleading since the restricted meaning is not denoted,"—but we did denote the restricted meaning. "(3) The assertion [our conclusion], taken literally, is irrelevant to a number of passages in the article with which it was connected."[28] Of the 3 passages that Dr. Johnson quotes as illustrative of this objection the first is an excerpt from a footnote and is out of its contextual setting; the second is relevant for it contains the very words of our conclusion; and the third is qualified by the phrase "under our conditions," which refers to method as well as to procedure; none of his illustrations sustains his criticism.

In my Note, I said that Dr. Johnson was not always accurate in quotation and report, and I gave instances.[29] In his rejoinder he repeats the offense. In the first place, he confuses cognitive and attributive clearness, and makes "degree of consciousness" synonymous with "clearness of content;"[30] and in the second place, his statements regarding our method do not accord with the facts. "Experienced introspectors," he writes, "assert that the task of comparing the clearnesses of various portions of content, independently of their other characteristics, is not easy. The assignment of satisfactory numerical values to these clearnesses is still more difficult. After long practice, a few adepts in the art of introspection achieve . . . consistency."[31] In none of the experiments with this method have the *O*s complained of any difficulty. On the contrary, they found it as a technical method unusually easy to master. Geissler's *O*s, "both experienced and inexperienced, found it relatively easy;" "after 8 or 10 hrs. of practice the *O*s had worked out for themselves a scale of 9 degrees of clearness."[32] Rogers found after "2 series of preliminary experiments" that her *O*s were able to estimate "the various processes according to 9 degrees of clearness."[33] In my experiments with the same method I have never met with any difficulty.[34] In all, 36 *O*s which is more than a few, have become adept in this "art" of observation. Not one of those who took part in these experiments was, to my knowledge, excused from the work because of difficulties with the method.

To the charge of inaccuracy, I must now add the charge of inconsistency. In his Review, Dr. Johnson claimed that "the introspective findings might, of course, be challenged."[35] In his rejoinder, he maintains that he has 'never been seriously inclined to challenge the concreteness or the appropriateness of the introspective clearness values or the reaction-times.'[36]

Dr. Johnson gives our results an interpretation of his own: "*The reaction-time is determined chiefly, if not entirely, by the degree of adjustment*

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[28] *Ibid.*

[29] *Op. cit.*, 307. As an example of his inaccuracies I gave the following: "Two flashes of a Geissler tube serving as preparatory signals were given in succession $3''$ and $1.8''$ respectively, before the stimulus." I pointed out that we did not use a Geissler tube, but a "2.9 volt electric globe," and the preparatory signals occurred "3 sec. and 1½ sec. before the occurrence of the stimulus." In the rejoinder Dr. Johnson says that the first error was a copyist's substitution, and that the second was not an error of report on his part as the preparatory signals actually occurred "as timed by the Hipp chronoscope $3265 \pm 32.6\,\sigma$ and $1853 \pm 11.8\,\sigma$ respectively before the stimulus." If he makes the correction in the time of the second signal which differs from the true setting by 0.35 sec., should he not have, by the same token, made a correction in the time of the first signal, which, as reported, differed from the true setting by almost the same amount, 0.26 sec.? Or does his "liminal threshold for accuracy" fall between these two values?

[30] *Op. cit.*, 601. *Cf.* E. B. Titchener, The Psychological Concept of Clearness, *Psych. Rev.*, 24, 1917, 48 ff.

[31] *Op. cit.*, 601. [32] *Op. cit.*, 511.

[33] A. S. Rogers, An Analytic Study of Visual Perceptions, this JOURNAL, 28, 1917, 558 f.

[34] *Op. cit.*, 467. [35] *Op. cit.*, 578. [36] *Op. cit.*, 606.

for reception and response; and hence indicates the degree of 'attention' in the objective meaning of that term."[37] The value of this interpretation becomes evident when we recall that in muscular, sensory, choice, or associative reactions, the degree of attention may be the same while the reaction-times differ greatly.

Cornell University K. M. DALLENBACH

EXPERIMENTAL PSYCHOLOGY IN THE ORIENT

Notable progress in the development of laboratories for psychology in the Far East is indicated by the following bit of information, most of it supplied to the JOURNAL by Mr. W. Wideman, Secretary to the C. H. Stoelting Company, Chicago. It appears that many of the Eastern universities have been coming to America since the War for psychological equipment as well as for 'testing' supplies. Among them are the University of Calcutta (Dr. N. N. Sengupta); Maharaja's College, Mysore (Dr. M. V. Gopalaswami); Dacca University (H. D. Bhattachharyya); Brahmamavidyashrama ("School of Universal Knowledge") Adyar, Madras; National University of Kwangtung, Canton, China (Dr. Hishing Wong); Teachers College, Wuchang (Professor F. B. Cheng); Fuh Tan University, Shanghai (which has recently built a psychological laboratory for Dr. Zing Yang Kuo, formerly of the University of California); Peking Government Teachers College (Professor Y. C. Chang); Amoy University (Dr. C. H. Chou); National Southeastern University, Nanking (Dr. C. W. Luh); National University of Peking (Dr. T. T. Lew); Amoy University; Kyoto Imperial University, Japan; College of Commerce, Nagoya (Professor Y. Kaga); and Tokyo School for Mental Deficiency (Dr. R. Ishii).

Industrial applications are well on the way, at least in Japan. It is reported that a Department of Psychology is maintained in one of the spinning centers, and the "Institute for Science of Labor" at Kurasiki, has just now published a report, which includes a bibliography of about twenty Japanese references and recites four years' work of the six departments of the Institute.—The *Japanese Journal of Psychology* (published in Japanese) is now in its third year. It has published a number of experimental articles.

University of Illinois MADISON BENTLEY

IMPROVEMENTS ON THE SANFORD VERNIER CHRONOSCOPE

Two improvements on the Sanford Vernier Chronoscope have been worked out by Mr. John R. Roberts, mechanician of the Psychological Laboratory at the University of Pennsylvania. (1) The method of fastening and adjusting the threads that hold the bobs was improved. In the older type of Sanford apparatus the threads were wound about a washer on the horizontal bar. Exact adjustment of the length of the bobs was difficult because the threads had to be unwound at every change. The improvement was made as follows. A hole was drilled in the horizontal bar between the two thread-holes and reamed to a taper. Into this a taper pin was fitted. A slot was cut in the base of the pin and a hole drilled through the side near the base. The thread was then passed through the bob, through the holes in the horizontal bar, and finally through the hole in the taper pin and there tied. When the taper pin is turned, as one turns the key of a violin, the length of the threads is changed. Adjustment is easy Twisting the taper pin brings up both sides evenly so that the bob always hangs in its proper position; it is not canted. (2) The second improvement has to do with the holder for visual stimuli. On the older type of chronoscope the visual stimulus had to be inserted in a clip fastened to the horizontal bar. In the case of choice reactions, in the laboratory drill section, the changing of the stimulus took a great deal of time. On the bar that carries the clip, we have now mounted a small slotted frame, both side of which are open. This is on a swivel so it can be rotated through 180°. The true and false stimuli are placed back to back in the frame and the exposure of either stimulus is effected by a mere turn.

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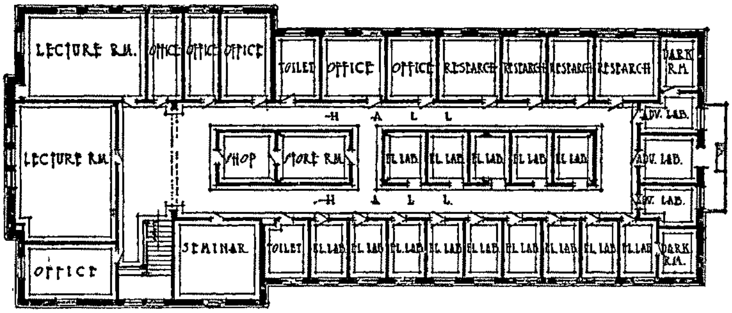
[37] *Op. cit.*, 605. Italics in the original.

University of Pennsylvania Samuel W. Fernberger

The New Psychological Laboratory of the University of Oregon

The department of psychology of the University of Oregon, now occupies its new quarters on the top floor of the new science building. Fifteen small rooms are devoted to elementary laboratory. Every room is furnished with a cabinet in which there is a year's supply of materials for one pair of students. Four rooms, including a dark room, are reserved for the advanced laboratory, four rooms for graduate students, six offices for members of the staff, two lecture rooms for classes not over 70, a combined seminary room and departmental library, a supply room and a shop.



PSYCHOLOGICAL LABORATORY, UNIVERSITY OF OREGON

With the exception of the rooms in the center of the hall,—which are temporary structures to be torn down if desired when the next unit of the science building is constructed and the department expands on the same floor,—all rooms are supplied with direct and alternating currents and an intercommunicating system of wires. Every room has compressed air and gas. The department has its own generator, batteries and air-pump. The laboratory is relatively sound-proof in construction, with staggered sills, padded joints, lined walls and floors, and battleship linoleum in all except the lecture room.

University of Oregon E. S. Conklin

Eighth International Congress of Psychology: Second Announcement

The second announcement of the eighth international congress of psychology, which will be held at Groningen, Holland, Sept. 6-11, 1926, gives the following provisional list of symposia and speakers: *Understanding and Explanation* by Dr. Binswanger of Kreuzlingen, Dr. Jaspers of

Heidelberg, and Dr. Spranger of Berlin; *Intensity of Sensations* by Dr. Boring of Harvard, Dr. Myers of London, and Dr. Werner of Hamburg; *Form-Qualities* by Dr. Benussi of Turin, Dr. Koffka of Giessen, and Dr. Michotte of Louvain; *Behaviorism* by Dr. McDougall of Harvard and Dr. Piéron of Le Vésinet; *Psychology of the Primitive Races* by Dr. Bartlett of Cambridge, Dr. Levy-Bruhl of Paris, Dr. Mayer-Gross of Heidelberg, and Dr. Storch of Tübingen; and *Religious Psychology* by Dr. Janet of Paris and Dr. Leuba of Bryn Mawr.

Other papers have been promised by Dr. Baumgarten of Solothurn, Dr. and Mrs. Bühler of Vienna, Dr. Buyse of Louvain, Dr. Foucault of Montpellier, Dr. Jaensch of Marburg, Dr. Julász of Budapest, Dr. Ley of Brussels, Dr. Marbe of Würzburg, Dr. Mayer-Gross of Heidelberg, Dr. Paulhan of Paris, Dr. Peters of Jena, Dr. Rignano of Milan, Dr. Rupp of Berlin, Dr. Selz of Mannheim, Dr. Spearman of London, Dr. Stern of Hamburg, Dr. Thurstone of New York and Dr. Werner of Hamburg.

Psychologists who wish to attend and have not received an invitation should communicate with the First Secretary, Professor Dr. F. Roels, Maliebaan 86, Utrecht, Holland. Membership subscription is 15 gulden.

Psychologie und Medizin

We have just received the first number (October, 1925) of *Psychologie und Medizin, Vierteljahrsschrift für Forschung und Anwendung auf ihren Grenzgebieten*, the official organ of the *Psychologische Gesellschaft* and the *Arbeitsgemeinschaft für praktische Psychologie* of Berlin. The Journal is edited by Dr. R. W. Schulte of Berlin with the cooperation of Professor R. Sommer of Giessen, Professor Erich R. Jaensch of Marburg, Dr. Albert Moll of Berlin, Professor W. Wirth of Leipzig, and Professor R. H. Goldschmidt of Münster.

A prefatory note by the editors and the following original articles appear in this number: R. W. Schulte, *Psychologie und Medizin;* R. H. Goldschmidt, *Psychologische Grundbegriffe für die ärztliche Begutachtung;* Robert Sommer, *Psychologie und Organisation des Erfinderwesens;* F R. Jaensch, *Ueber eine Lücke im gegenwärtigen Wissenschaftsbetrieb und das Verhältnis von Psychologie und Medizin;* Albert Moll, *Okkultismus und Psychologie;* Fritz Giese, *Zum Begriff der Kulturpathologie;* Hans Friedenthal, *Die Rolle der Physiognomik in der menschlichen Praxis;* J. H. Schultz, *Zur Psychologie der Homosexualität;* R. W. Schulte, *Ueber Elektrodiagnose seelischer Eigenschaften;* G. A. Roemer, *Atmung und musikalisches Erleben;* and Georg Villwock, *Werkstattarbeit und Muskelphysiologie.* Besides these articles the initial number contains reports of the meetings of the *Deutscher Verein für Psychiatrie* and of the *Gesellschaft Deutscher Nervenärzte* (Kassel, September, 1925); as well as the proceedings of the Second International Congress for Individual Psychology held at Berlin. Notes.

K. M. D.

ERRATA

In the article on "The Acquisition of Absolute Pitch," by Helen K. Mull, which appeared in the October, 1925, number of the JOURNAL, the following corrections should be made: p. 486, line 16, for "5," read "0.5;" p. 486, line 17, for "75," read "0.75;" p. 488, line 42, for "g" read "g#" in both cases.

E. G. B.

In my article on "Definition and Measurement of Attention," which appeared in the October, 1925, number, the following corrections should be made: on p. 610, Definition (14), for "$f\sigma_m\sigma_m$," read "$f\sigma_{mx}\sigma_{my}$;" on p. 611, Equation (III), for "σ_{mx}/σ_{my}," read "σ_{my}/σ_{mx};" the same correction should be made also in the third line below this; and on p. 613, footnote, line 1, for "longer," read "shorter."

H. M. JOHNSON

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AUDITORY THEORY WITH SPECIAL REFERENCE TO INTENSITY, VOLUME, AND LOCALIZATION[1]

By Edwin G. Boring, Harvard University

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In respect of the theory of hearing, it seems to me that we need fewer theories and more theorizing. Of theories, focused upon some new finding and seeking to align the entire body of auditory fact with the new principle, we have more than a plenty.[2] Of straight-forward attempts to bring the known facts into reasonable relation, to point out clearly the lacunae in our knowledge and the discrepancies that remain in even the best interpretation, and to formulate the crucial experiments that might resolve these discrepancies—of such theorizing we have too little, and it is such a discussion that I seek to formulate in this paper. The facts, so it seems to me, now point toward a frequency-theory of hearing instead of a place-theory; but, if my paper can stimulate more theorizing and more research that may eventually establish the place-theory, I shall be just as happy at seeing Helmholtz' sixty-year-old hypothesis vindicated at last.

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[1] An expansion of a paper read before the Washington meeting of the American Psychological Association; *cf. Psych. Bull.*, 22, 1925, 96 f.

[2] Think of the theories that there are! The resonance-place theories, which assume the specific energy of nerves: Helmholtz (1863), Mach, Shambaugh, Lehmann, Hartridge, Fletcher, and now in an entire book Wilkinson and Gray (1924); the resonance-frequency theories: Hensen, Wundt, Ebbinghaus; the non-resonance-place theories: Waller, Hurst, Ewald, ter Kuile, Watt; the non-resonance-frequency theories: Rutherford, Hardesty, Max Meyer's famous theory, and Wrightson's recent book (1919); and then there is Jaensch's *Duplicitätstheorie*. And there are others, if one presses the term "theory," all stimulated by the original work of Helmholtz and by our fairly precise knowledge of the intricate mechanics of the inner ear.

Intensity vs. Quality. No theory is worth anything at all if it cannot take account of both the intensity and the pitch of auditory experience. A place-theory which assumes the specific energy of nerves takes account of pitch by correlating it with the fibers stimulated and presumably therefore with cortical locus; it takes account of intensity very simply by assuming a correlation with the degree of the nervous impulse. Modern physiology, however, is being forced to accept the all-or-nothing principle of nervous excitation as applying, not only to motor fibers, but also to sensory fibers, with the necessary conclusion that a single fiber can ordinarily give rise to but a single intensity.[3] If for the moment we accept Helmholtz' notion that there are a determinable number of discrete sensible pitches, say 11,000, then there are a few thousand fibers left over. Some pitches could, therefore, have two fibers and two intensities; others could have but one. There are, however, not nearly enough fibers to account for all the quanta of pitch and intensity; there would have to be about as many fibers as the product of the pitch quanta by the intensive quanta, perhaps 300,000 in all.

It seems to me, therefore, that the old theory of the specific energy of nerves is inadequate as against modern physiology, and that the best thing to do is to begin theorizing again and to begin this time with intensity; partly because we are in a position to know a little more about it physiologically, and partly, as I hope to show, because this mode of procedure promises a solution of the primary problem of auditory localization, which has as yet avoided contact with the general theory.

In general the all-or-nothing principle has been neglected by the theorists of hearing. Recently it has had some mention,[5] but more or less by way of casual dismissal.

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[3] *Cf. e. g.*, A. Forbes and A. Gregg, *Am. J. Physiol.*, 39, 1915, 172-235; E. D. Adrian and A. Forbes, *Jour. Physiol.*, 56, 1922, 301-330.

[4] H. Fletcher, *Jour. Franklin Inst.*, 196, 1923, 294-296, 318-322.

[5] *E.g.*, H. Hartridge, *Brit. J. Psych.*, 12, 1922, 368 ff; G. Wilkinson and A. A. Gray, *Mechanism of the Cochlea*, 1924, 175 ff., who cite Forbes and Gregg.

My colleague, Professor Forbes, has, however, given a generalized theory of sensory intensity that is consistent with the all-or-nothing principle. Reserving 'place' as the correlate of quality and thus preserving the specific energy of nerves, he shows that sensory intensity could be correlated with the frequency of the nervous impulse.[6] He assumes that the stimulus gives rise to a sustained local excitatory process in the receptor, and that this excitatory process is graded in degree in accordance with the degree of the stimulus. Now a sustained excitatory process will give rise in the nerve to a repetitive discharge, and the more intense the process the greater will be the frequency of the discharge, because the stronger process becomes effective for the all-or-nothing discharge at an earlier period of the relative refractory phase. In this way the degree of the stimulation gets itself translated into frequency of impulse which can be held to be the nervous correlate of sensory intensity.

This suggestion seems to me very attractive and I want to point out, as I believe Forbes has not done, that it is consistent with the general nature of Weber's law. The well known curve of Adrian and Lucas,[7] showing the excitability of the nerve at successive intervals after stimulation, is of such a form that for successive equal increments of increase in the strength of the excitatory process there would be diminishing returns in the increase of the rate of the induced frequency. It is true that the time of the absolute refractory period would necessarily determine a maximal intensity which could not be transcended in experience, but then we have no psychological knowledge at present which tells against this corollary.[8]

The two most reasonable theories then are counterparts of each other. In a 'frequency-theory' quality may be correlated with frequency, and intensity with the number of specific fibers stimulated. In a 'place-theory' the theory of specific energy of nerves may be supplemented by Forbes' suggestion: intensity would be correlated with frequency and quality with the specific fiber stimulated. I wish to choose tentatively the former theory for no other reason than that it seems to me to lead to the explanation of more facts, including the facts of auditory localization. If by thus relegating quality to frequency we eventually run into difficulty when the refractory period of human acoustic nerve fibers comes to be known, then we must scrutinize the whole theoretical structure again.

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[6] *Opp. citt.*

[7] *Cf.* K. Lucas, *Conduction of the Nervous Impulse*, 1917, 35; or W. M. Bayliss, *Principles of General Physiology*, 1924, 389.

[8] Goldscheider implies such a maximal sensory intensity in discussing the tuning of warm and cold spots in the skin.

Sensory Quanta. Any theory that involves the specific energy of nerves is a theory of sensory quanta. A qualitative series cannot then be a continuum with an infinite number of points: there can be only as many physiologically simple qualities as there are nerve fibers. It used to be said that the limits of hearing include about 11,000 liminal steps, and writers then spoke of "11,000 auditory sensations." In general, however, psychology has accepted the Helmholtz theory without accepting this implication. If we find that the differential limen at 435 d. v. is 0.3 d. v., we mean that 435.3 will be just as often sensed higher than 435 as it is not; but we also mean that the same difference will be found between 435.2 and 435.5. If there were qualitative quanta we might find just as much difference between 435.2 and 435.3 as between 435 and 435.3; a critical point might lie just below 435.3.[9] With all the work that has been done upon pitch discrimination, it seems almost as if we should certainly have stumbled upon such critical points did they exist, and that our failure to find them argues for a rejection of a theory of specific nerve energies.

Nevertheless we must not forget that the rejection of Forbes' theory and the assumption that auditory intensity depends upon the number of fibers stimulated, commits us to a quantum theory of auditory intensity. Here we need research. It should be perfectly feasible experimentally to determine the presence or absence of critical points in both the qualitative and intensive series. Meanwhile I think it is safer to admit intensive quanta in our theorizing than to admit quanta in pitch, because more work has been done on pitch discrimination than on auditory intensive discrimination without finding the quanta. To a slight extent, therefore, this conclusion tells against Forbes' theory.

Integration. At this point it is impossible to avoid raising the problem of central integration with respect to intensity. For motor nerve fibers the muscle is an integrating organ. The greater the innervation the more fibers are stimulated, but these separate nerve impulses are all combined in the muscle into a single tendinous pull, graded according to the innervation. It would seem that, if sensory intensity is conditioned in a manner similar to muscular response, the separate sensory impulses should be integrated cortically. I cannot quite justify this belief. It is founded upon the fact that fusion of intensities is the most intimate fusion about which we know anything. Two positions may constitute (according to Külpe) only a colligation; two qualities may fuse intimately like red and yellow or less intimately like c and g; but two intensities summate without leaving the least trace of multiple origin in the integrated product. You cannot, said James and the others who raised the quantity objection against Fechner's measurement of intensity, hold that a 'feeling of scarlet' is composed of so many 'feelings of pink.'[10] It is also tempting to argue that, since spatial specificity in the brain must be the correlate of the perception of spatial difference, it cannot also persist as the correlate of intensity. I think, however, that we should avoid this temptation, since, as I hope to show, there is a demonstrable relation between auditory volume and auditory intensity. On the whole, however, it is plausible, even if not essential, to suppose some kind of integration in the brain where multiplicity of impulses is translated back into degree of excitation.

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[9] For a text-book discussion of sensation as a continuous function of stimulus, see E. B. Titchener, *Experimental Psychology,* 1905, II, i, xxxvi

For such a purpose one turns naturally to Köhler's theory of the electrical brain field.[11] Köhler thinks of excitation of the cortex as the establishment of electrostatic fields, limited by the origin of the excitation in part, but spreading with greater freedom than a perfect insulation would allow. Such a theory seems to be the modern equivalent of Bernstein's older theory of central projection and central dispersion.[12] Unless something of this sort occurs, I do not see how we are to account for the relation of auditory volume to intensity, for the magnitude of the stars, for the dispersion evident in the perceptual series of forms that appear in the two-point stimulation of the skin,[13] or for other similar phenomena.

In this connection we may note that Forbes' theory would lead to a similar result. To get an intensity from a frequency we should feel the same need for a successive summation in the brain, and then other considerations of fact would lead us to suppose a dispersion of excitation. Here, however, a difficulty would occur. A spread of excitation ought on his theory to lead to a change of quality, since pitch would be correlated with cortical locus. Nevertheless, although a change in pitch ordinarily leads to a change in volume, it is not true that change in volume necessarily leads to an alteration in pitch. Moreover, we should still have a problem of spatial integration on our hands in order to explain fusions of pitch like the octave and the other tonal dyads. Thus I think we come upon a second reason for not abiding by Forbes' theory.

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[10] W. James, *Principles of Psychology*, 1890, I, 546.

[11] W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920.

[12] *Cf.*, W. Nagel, *Handbuch der Physiologie des Menschens*, 1905, III, 720 f.; especially E. G. Boring, *Quart. J. Exper. Physiol.*, 10, 1916, 86 ff.

[13] E. G. Boring, this JOURNAL, 32, 1921, 465-470, and *loc. cit.*

Intensity and Amplitude. If intensity depends upon the number of cochlear fibers stimulated, all functioning under the all-or-nothing principle, then amplitude of vibration, rather than energy, would be the stimulus-correlate of tonal intensity. The energy of a tone is proportional to the square of the product of its amplitude and frequency, but plainly the number of fibers stimulated would be a function of the amount of displacement of the organ of Corti from the base of the cochlea and thus of the amplitude of the stimulus. This view of the operation of the cochlea is like Max Meyer's,[14] although my theorizing follows his but little further.

Volume and Intensity. Besides pitch and intensity all tones have volume; they are "large" or "small" as the case may be. Because volume appears to vary markedly only when some other attribute varies, its independent status was for a long time unrecognized. It varies with pitch (*vide infra*) and it varies with intensity. The logical order of this paper reverses in many cases the historical order of the discovery of the facts with which it deals. It is interesting to note, however, that volume as a covariant with pitch was quite well established before it ever occurred to anyone that it might vary with intensity also. Then Halverson tried it out, because, as a result of his work on auditory localization (*vide infra*), it seemed probable that he would find just this relation.[15] Halverson's results are now available. By varying the resistance in a telephone circuit carrying a tone electrically, he produced changes in both the intensity and the volume of the tone. He used the method of constant stimuli for determining differential limens of both intensity and volume, determined a series of successive limens, and thus found the law of the limen in each case. The volumic limens were always much larger than the intensive limens and the laws were different. The volumic limen is practically a linear function of the current; the law of the intensive limen is curvilinear, showing an increase in the relative increment with increase of current. A logarithmic function, like Weber's law, would be linear when stated in this manner. The point is that the difference in the sizes of the limen and the difference in the laws show unequivocally that the observers were not judging the same thing under the two different instructions, but were giving independent judgments of separate covariants.

Our theorizing led us to suppose that more intense tones would be larger, and Halverson's experiment established this point. The conclusion would seem to be that simultaneous impulses in adjacent fibers set up a field of excitation in which both the dispersion and the ion-concentration are greater for a greater number of fibers. It is not necessary at this point to speculate as to whether the dispersion would occur if it were not already given by the displacement of the fibers from one another. It is sufficient simply to note the correlation as a reasonable theoretical account of the results of the experiment.

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[14] M. Meyer, *Univ. Missouri Studies, Science Ser.*, II, no. 1, 1907, 6-14.

[15] H. M. Halverson, this JOURNAL, 35, 1924, 360-367. *Cf.* also H. J. Watt, *Psychology of Sound*, 1917, 183; *Brit. J. Psych.* 11, 1920, 164 f.

Volume and Pitch. Volume was discovered as a covariant of pitch. Low tones are large and massive; high tones are small, hard and concentrated. For a time the evidence consisted in 'office introspection,' but Rich made of volume an experimental fact, determining, as Halverson did later for intensity and volume, the limens and the law of the limens.[16] The argument that volume is not pitch is the same as the argument that it is not intensity. Volumic limens are larger than pitch limens; in small differences of frequency (less than the musical comma) one can quite casually hear pitch change without volume. The pitch limen is a rather erratic function of frequency. Volumic limens, like musical interval, are proportional to the absolute value of the frequency, a fact which has led Ogden to consider them as the basis for the perception of musical interval.[17] Rich worked at Cornell, but Halverson got similar liminal results for frequency at Clark[18] and at Maine.[19] Moreover, the volume that Halverson contrasted with intensity at Maine must be the same volume that Rich described, for Halverson trained his observers to report on volume by using as a variable, not intensity, but frequency.

At first it is not apparent how volume can possibly be dependent upon both the frequency and the amplitude of the stimulus. It must be remembered, however, that its dependence on frequency was measured before its dependence on amplitude had been thought of, and that the amplitude of the stimulus was not controlled. What would seem to have happened is that, in measuring volume by varying frequency, the energy of the stimulus remained approximately constant. Helmholtz assumed roughly, though not exactly, that the delivery of a constant stream of air to a siren gave sounds of equal energy.[20] In both of Rich's and in both of Halverson's experiments Stern variators were used when frequency was varied.

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[16] G. J. Rich, *Jour. Exper. Psych.*, 1, 1916, 13-22; this JOURNAL, 30 1919, 121-164, esp. 149 ff.

[17] R. M. Ogden, *Hearing*, 1924, 165 f.; *cf.* also the discussion of volume that runs through this book.

[18] Halverson, this JOURNAL, 33, 1922, 526-534.

[19] Halverson, 1924, *op. cit.*

[20] H. L. F. v. Helmholtz, *Sensations of Tone*, Chap. IX, first paragraphs (4th Eng. ed., 1912, p. 174).

It is probably approximately true (though not exactly) that the tones were of equal energy. Now the energy of a tone is proportional to the square of the product of its amplitude and frequency. Hence it would have come about that the amplitude of the low tones would have been greater than the amplitude of the high tones, and that volumic differences would have resulted from the differences of amplitude of the different frequencies used. On this view the dependence of volume upon frequency becomes an artifact of the experiments, and, even though it seems to me to be fatal to Ogden's belief in the relation of volume to musical interval, I do not see how it can be avoided.

We must note in addition that the view is not only plausible but would also lead us to anticipate Rich's result that volume is a logarithmic function of frequency, or that the volumic limen is proportional to the frequency at which it is determined. If in an experiment all the frequencies were of equal energy, then the amplitude would be inversely proportional to the frequency, since the (squared) product of the two is given as constant. Our view is that the volumic limen should be inversely proportional to the amplitude, if limens are equal sense-distances. Rich's conclusion follows immediately from these considerations.

On the other hand I must admit that I take Professor Joseph Peterson's criticism[21] quite seriously. He suggests that for all this indirect proof we should substitute the obvious direct proof. We should see experimentally whether an intense high tone can be made identical in volume with a weak low tone. More precisely stated the problem is: are tones of different frequencies and the same amplitude, and therefore unequal in energy, alike in volume? Here is a problem, a test of the present theory, which ought to be undertaken.

Auditory Localization. It seems that auditory space is primarily uni-dimensional, from one side of the head to the other. Judgments of "rightness" or "leftness" are most immediately and most precisely made. Perception of the distance of the sound and of whether it lies high or low, in front or behind, are presumably secondary—at least as secondary as the third visual dimension is to the primary areal space of the retina.

There are three theories of the mechanism of the perception of this primary auditory linear space: the intensity-theory, the time-theory, and the phase-theory.[22]

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[21] In discussion at the Washington (1924) meeting of the American Psychological Association.

[22] *Cf.* Köhler, *Handbuch der Neurologie des Ohres*, 1923, I, 462 ff.; Halverson, this JOURNAL, 33, 1922, 178 ff.

The intensity-theory holds that localization is toward the side of the ear in which the intensity of the sound is greater.[23] Localization is thus spoken of as a function of the ratio of the intensities at the two ears. This ratio is usually called simply the "binaural ratio." The intensity-theory has long been most popular among psychologists as an opponent of the phase-theory, since psychologists could not conceive how the ear could "perceive phase."

The time-theory is new and is due to the work of von Hornbostel and Wertheimer[24] in the first place and of Klemm[25] subsequently. This work shows that with two discrete sounds, qualitatively alike but separated by a very short interval of time, one sound is heard and its localization is toward the side of the earlier, provided the two are equal in intensity. The theory does not deny the intensity-theory, for a later intense sound may be equivalent to an earlier weak sound, resulting in median localization (Klemm).

The times involved are very short and have to be expressed in $\sigma\sigma$ (secs. × 10^{-6}). If the sounds are practically simultaneous with a displacement of not more than 2 $\sigma\sigma$ (Klemm) or 30 $\sigma\sigma$ (Hornbostel and Wertheimer) a single sound, localized in the median plane, is heard, provided the intensities remain equal. If the temporal discrepancy is increased, localization shifts toward the side of the prior sound. At 630 $\sigma\sigma$ localization is maximally lateral (Hornbostel and Wertheimer), and remains at that side for greater intervals of sound. If, however, the temporal interval be increased to about 2000 $\sigma\sigma$ (Klemm, 2σ), the sounds no longer fuse but break up into two successive sounds, of which the observer can state accurately whether the right or the left is prior. These times are of the utmost importance and must be referred to later.

The phase-theory[26] has, in general, enjoyed more popularity with physicists than with psychologists. When the same harmonic frequency is brought to the two ears with a difference in phase of the two vibration rates, localization is toward the side where the phase is leading. If the two stimuli are in phase, localization is median, except as difference in acuity of the two ears may introduce an intensive difference which shifts the localization slightly to one side or the other. If the stimulus at the right is made to lead in phase (*e.g.*, by lengthening relatively the tube through which it travels from a source common to the other stimulus), localization travels toward that side. When the phase-difference is still further increased until the one tone leads the other by more than half a wave-length, then the one tone must be regarded as lagging behind the other instead of leading, for its wave-form is now, point for point, just behind the wave-form that previously constituted the preceding wave of the lagging stimulus. When the difference has become an entire wave-length the tones are, of course, in phase again and there is no effective difference. There is perhaps as yet no final agreement as to just what happens when the two stimuli are displaced by half a wave-length and either can be said to be leading or lagging. As this critical point is passed, localization shifts from one side to the other. Many have said that the perceived tone passes through the head or that it spreads over and about the head. Halverson, however, found it appearing on the one side just before it disappeared on the other side,[27] and I am inclined to accept this finding and to assume that the other reports were inferences and not observations. (The two are so easy to confuse. 'The coin is in this box. Presto! It is in that. I have made it move from the one to the other.' But the locus of the coin is observed and the movement is inferred—inferred erroneously in most magic. Still it is possible that there is a real movement of the sound; an auditory phi-phenomenon.[28])

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[23] *Cf.* A. H. Pierce, *Studies in Space Perception*, 1901, 23-209; C. E Ferree and R. Collins, this JOURNAL, 22, 1911, 250-297; Halverson, *ibid.*, 33, 1922, 178-212; and references cited in these articles. *Cf.* also C. A. Ruckmick, *Psych. Mon.*, 30, 1923, 77-83.

[24] E. M. von Hornbostel and M. Wertheimer, *Sitzungsber. d. preuss. Akad. d. Wissensch.*, 1920, 388-396; Hornbostel, *Psych. Forsch.*, 2, 1922, 141-143; 4, 1923, 64-114. *Cf.* also H. Hecht, *Naturwissensch.*, 10, 1922, 107-113.

[25] O. Klemm, *Arch. f. d. ges. Psych.*, 38, 1919, 105-110; 40, 1920, 117-146.

[26] Rayleigh, *Phil. Mag.*, 6 ser., 13, 1907, 214-232; H. A. Wilson and C. S. Myers, *Brit. J. Psych.* 2, 1908, 363-386; Halverson, 1922, *op. cit.*; and references there cited.

Although these three theories are supposed to be rivals, it is to be noted that they are already coming experimentally into relation. An intensive difference can be equivalent to a time-difference and the two can cancel each other in effect. Intensive differences may alter effective phase-relations and may presumably be equated to them.[29] Phase-difference, as we shall see presently, is a form of time-difference. I am hopeful that the present paper will show the mutual consistency of all three.

Localization and Intensity. Although we know very little about the physiology of a perception like localization, I think we may say that current psychology assumes, almost as a matter of course, that intrinsically spatial experience is correlated with extensity of cortical excitation. I do not mean that a perceived circle would imply a circular excitation, but merely that there is a gross correlation in kind. At any rate I propose that we accept this assumption, as indeed we have already done in proceeding thus far. If the primary immediate experience of auditory position is unidimensional, we must look then to the brain for an excitation that can vary with localization along a unidimensional locus.

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[27] Halverson, 1922, *op. cit.*, 18 5ff. *Cf.* also T. J. Bowlker, *Phil. Mag.*, 6 ser., 15, 1908, 323-326; G. W. Stewart, *Phys. Rev.*, 1920, 438.

[28] Watt calls this phenomenon a "cinemato-acoustic illusion," 1920, *op. cit.*, 168.

[29] G. W. Stewart, *op. cit.*, 438 f.; Halverson, 1922, *op. cit.*, 207-211.

Beyond this assumption I think it is at present neither necessary nor safe to go. Localization may involve more than the experience of relative position. Auditory localization in most (but not all) observers consists habitually in visual imagery, a visual context to an auditory core. Even when the auditory core does not function for a visual context, it has to function for some mechanism of report when it participates in any experiment. This entire problem is the converse of the problem of integration which we have faced. Somehow an imperfectly insulated excitation excites associative or afferent fibers in such a way that the fibers excited depend upon the locus of the stimulating excitation. In avoiding this problem I do not wish to minimize it. I want simply to escape speculation wherever possible.

Nevertheless we cannot theorize and avoid speculation entirely. It seems to me that we must now assume that the fibers from both ears lead in each hemisphere to two adjacent regions, which are not coincident, but which are very close together and overlap. Watt has made this assumption for similar theoretical purposes.[30] So far as I know there is not one iota of strictly physiological evidence for it except that the cochlear fibers from each ear go to both hemispheres. I dislike such gratuitous assumptions, but I am ready to follow Watt in this one tentatively because it leads somewhere theoretically. At any rate the supposed relationship is not impossible, and is improbable only in so far as one of many possibilities is statistically improbable when we choose in the face of ignorance.

Now the facts of the dependence of localization upon amplitude follow at once.[31] If only one ear is stimulated, then only the region corresponding to that ear is excited, and we have the cortical correlate of lateral localization. If both ears are stimulated equally, both the overlapping regions are stimulated equally and the modal locus of the cortical excitation is intermediate between the two extreme positions of the mode in right and left lateral localization. If both ears are stimulated but amplitude is greater in one, then this mode would tend in the direction of the more intense sound. These are the facts when localization varies with intensity.[32]

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[30] H. J. Watt, *Brit. J. Psych.*, 11, 1920, 163-171; *cf.* also his *Psychology of Sound*, 1917, 182-192. I can not always understand Watt, so I hesitate to ascribe the paternity of my view to him.

[31] Again *cf.* Boring, 1916, *loc. cit.* for the similarity of the present view to the Bernstein theory.

In connection with these facts it must be remembered that primary auditory localization is not very precise.[33] The perceived diotic sound is always large. Klemm speaks of it as a "subjective field of hearing." Halverson's visual observers visualized large spots upon the arc along which the sound moved from one side to the other. In localizing for the purposes of experimental measurement the observer reports that the sound is where its center is. The experience is one of a dispersion said to be localized at its center (what else can an observer say when he must state in numbers the position of a large object on a precise scale?) just as we have pictured the excitation as dispersed with its localization determined by its mode. Undoubtedly the lack of precision in localization is determined by the size and ill-defined boundaries of the sounds. It is for these reasons that I think we must assume that the cortical regions corresponding to the two ears overlap and overlap considerably, thus preventing a very precise spatial differentiation as dependent upon their differential excitation.[34]

Localization and Time. The time-theory becomes a special case of the intensity-theory. The prior sound, within the proper temporal limits, is more effective than the later. The degree of cortical excitation is greater for it, and all the remarks about an intensive difference therefore apply to a temporal difference; which is translated into an intensive difference at the auditory center.

We know more about the psychology than the physiology of this phenomenon. The prior excitation, to a greater or less degree according to the temporal interval, inhibits the later. Physiologically we know that one excitation can inhibit another, and that the vast complexity of the nervous system allows scope for all sorts of complications. McDougall's theory of inhibition by drainage[35] would fit the case, but the theory seems not to have proven acceptable to physiologists.[36] The psychology of such inhibition is, however, pretty clear, and physiology might in this case easily learn from psychology. The range of attention is limited. Of a number of simultaneous adequate stimuli not all are effective for consciousness or, unless they appeal to well mechanized movements, for efferent excitation. Time enters into our consideration when we note that attention is predetermined. When stimuli are too numerous for the range of attention, it is not chance that determines which shall be effective. That stimulus is effective for which the organism is in some way predetermined. Every momentary state of the brain has its selective effect on every immediately subsequent momentary state. And we can go further. Every mental process, and presumably therefore its neural correlate, undergoes a process of cumulation and decay. This temporal course of the process is very rapid. It is not of the order of times which the classical experiments on the fluctuation of attention indicated. These experiments dealt rather with the persistence of a relatively simple meaningful idea and not of a simple bit of experience. Pillsbury's experiment with the ink-dot is, however, in point.[37] If the stimulus is so simple as to permit little fluctuation of attention within the universe of the stimulus, then the fluctuations are very rapid, perhaps ten in a second. At these very brief times it is not possible to measure the duration of the items of a kaleidoscopic consciousness, but the most careful introspection shows that what is happening all the time in mind, many times within the second, is that attention is caught by some conscious datum which comes quickly to full fruition and then gives place to something else.[38] This fact seems to me to be almost the fundamental uniformity of conscious experience. One cannot escape interpreting it as a law of cumulation and decay inherent in nervous excitation, and one finds the same form of process and times of occurrence of the same order in the physiological picture of the current of action induced, under the conditions of the all-or-nothing law, by a break shock in a sensory nerve.[39] At any rate all the evidence points toward the inhibition of a subsequent excitation by a prior when the interval of time between the two is very small indeed.[40]

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[32] The principle of the functioning of the mode is exactly analogous to Gray's "Principle of Maximal Stimulation," and the arguments that he makes in support of it apply here. I avoid taking over his term because the theory of Wilkinson and Gray is a defense of the resonance theory, and I wish to avoid confusion by implication of similarity. See Wilkinson and Gray, *op. cit.*, 152-169.

[33] Halverson, 1922, *op. cit.*, 200 ff.

[34] The regions could be fairly far apart if the fibers from the base of the cochlea ran to the most remote central regions, and the fibers from the tip of the cochlea overlapped centrally, with fibers from intermediate regions of the cochlea leading to intermediate positions in the cortical regions. Since amplitude of stimulus extends excitation up the cochlea, overlapping would automatically be greater for greater intensities, and very weak sounds might lead to no overlapping at all. Thus the question would be open to experimentation. But at present I am not ready to carry speculation so far.

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[35] W. McDougall, *Brain*, 26, 1903, 153 ff, and esp. 167 ff; and again *cf.* Boring, 1916, *loc. cit.*, where I used this theory.

[36] *E.g.*, Bayliss, *op. cit.*, 424 f. Perhaps Head's characterization of my proposal for the skin as "an invocation of purely imaginal anatomical conditions" may have referred in part to my use of the theory of drainage: H. Head, *Studies in Neurology*, 1920, II, 824.

[37] W. B. Pillsbury, *Jour. Philos.*, 10, 1913, 181 ff.

[38] There is almost nothing in the literature to illustrate this most obvious fact of introspection, that attention is limited in its scope to a small part of the field of stimulation and in time to a fraction of a second. Unpublished work of Yokoyama's brought it out clearly, and on this experiment there is a preliminary note: Boring, this JOURNAL, 35, 1924, 301-304.

The experiments which gave rise to the time-theory of localization indicate approximately what these times must be. There is no inhibition between two auditory stimuli of the same quality when they are simultaneous or displaced by a few σσ. They fuse. At the center there is one complex excitation. At 30 σσ or more there is some inhibition, which increases as simultaneity is departed from, until at about 630 σσ an optimum is reached and inhibition is complete. Beyond this point inhibition continues for greater times until at about 2σ the discrepancy has become so great that each excitation is realized independently of the other. If one could measure the current of action at the auditory center, one might expect the maximum current to be reached after, say, 700 σσ and the impulse completely to have died away before 2σ.

One must not attempt, however, to press too closely the relation of these times to the time of rise and fall of the nervous impulse for the reason that stimulation cannot be instantaneous even when it occurs under the all-or-nothing law. Liquids are so highly elastic and so little compressible that the mechanical impulse must travel very rapidly through the cochlea. Nevertheless some time is required and when two stimuli are very slightly displaced in time it may easily be that the distal fibers in the ear first stimulated are excited after the proximal fibers in the ear later stimulated. It seems reasonable to suppose that this temporal dispersion of the stimulus, due to the structure of the cochlea, accounts for partial inhibition and resultant intermediate localization. Otherwise this inhibition might follow an all-or-nothing law, and we should find median and lateral localization, but never localization in intermediate positions.

Localization and Phase. Just as the time-theory becomes a special case of the intensity-theory, so the phase-theory becomes a special case of the time-theory. Frequency-theories of hearing have sometimes been called "telephone-theories" on the assumption that the wave-form of the stimulus is carried by the auditory fibers and that the analysis that occurs in accordance with Ohm's law takes place in the brain. Since theoretical attention has usually centered first upon this analysis, frequency-theories have often been rejected because they simply transfer this problem to the brain without effecting a solution. The all-or-nothing law, however, means that it is not possible for the wave-form to be transmitted as such. A simple harmonic wave would impress its form of motion upon the hair-cells, depressing and bending the hairs,[41] and a complete discharge would occur when the hair had been adequately displaced. There would be a series of discrete impulses of the frequency of the stimulus, except when the time of the single wave became less than the refractory period of the nerve fibers. A lag in phase of the stimulus for one ear behind the stimulus for the other would simply mean that the nervous conditions for difference in time were repeated for every complete wave. We must return later to a consideration of what happens to a compound wave under these conditions.

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[39] *Cf. e.g.*, Garten's curve for the diphasic current of action in the olfactory nerve of the pike, reproduced in Bayliss, *op. cit.*, 379.

[40] The facts of prior entry do not necessarily apply here. The sensations are in different sense departments and there is delay of one but no inhibition. S. Stone (this JOURNAL, 37, 1926, 284-287) has found the delay between touch and hearing to be of the order of 50 σ, a very long time as compared with the times that enter the present discussion.

This reduction of phase-difference to time-difference means that, from a knowledge of the time-differences upon which the localization of discrete diotic sounds depends, we should be able to predict the behavior of localization when the phase-relations of various frequencies are varied. If the inhibition of the excitation from one ear by the excitation from the other begins at about 30 $\sigma\sigma$ and is complete at about 630 $\sigma\sigma$ (Hornbostel and Wertheimer), then it should follow that, for any diotic tone given with dichotic difference of phase, localization should be median when the tones are in phase, should begin to be a lateral when one tone leads the other by about 30 $\sigma\sigma$, and should become completely lateral when the phase-difference has become about 630 $\sigma\sigma$. If the frequency is low enough to permit phase-differences as long as 2 σ (Klemm), then the tone should appear on the one side before it has disappeared on the other. All these relationships have not been worked out, and a thorough research on this matter would be valuable. On the other hand many confirmations of the relation are possible now.

In the first place we may note that a phase-difference of more than 2 σ should lead to double lateral localization on both sides at once. A phase-difference greater than 2 σ can be obtained only with tones of frequency less than 250 d.v. (A tone of 250 d.v. has a wave-period of 4 σ; and a phase-difference cannot be greater than half the wave-period. If one 250-tone, *e.g.*, were to lag 3 σ behind another, it would not be lagging but would be leading by 1σ.) Now this general fact is confirmed: low tones at the critical region where lead changes to lag may be localized bilaterally. High tones, as we shall see presently, not only are not bilaterally localized but may never be laterally localized at all. There is not, however, strict confirmation as to detail. For instance, Halverson got bilateral localization of 512 d.v. throughout a region of phase-difference from 0.10 to 0.16 of the wave-length;[42] that is to say, the "single subjective field of hearing" should, according to Halverson's data, break up at from 816 to 874σσ, and not at 2000σσ, as Klemm states. Klemm, however, was speaking in round numbers,[43] and more exact work needs to be done on both sides of the correlation before the relationship is held invalid. The two values are at any rate of the same order as compared with the other values that we must consider.

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[41] On the kinematics of the hairs, see T. Wrightson and A. Keith, *Enquiry into the Analytical Mechanism of the Internal Ear*, 1919, 113, 127, and esp. 209-211.

An even more important study is the one which will determine the effect of phase-difference upon localization as a function of frequency. We need to know whether the degree of lateral displacement of the sound depends upon the absolute difference in time or the relative difference in phase. If the argument of this section of the present paper is correct, it should depend upon the absolute time-difference: one would find that the low tones reached the lateral position sooner than high tones when phase is measured as a ratio of the wave-length, but that both should reach the lateral position after the same interval when phase-difference is measured directly in time. We have, it is true, Stewart's study,[44] which indicates that the desired relationship holds for tones between 100 and 1200 d.v., though not for high tones. Perhaps we should regard this paper as conclusive and the theory substantiated in the middle range. I confess, however, that I should like to see so crucial a point made the subject of a direct attack.

When phase-difference can not be greater than 630σσ (Hornbostel and Wertheimer), that is to say when the frequency of the tone is greater than 794 d.v., we ought to find that the tone never reaches the lateral position. In general this conclusion is confirmed. High tones are not only poorly localized, but, when localization is made for differences of phase, they are found to move through a more limited region including the median position. Bowlker found that for wave-lengths shorter than 20 in. (*i.e.*, frequencies greater than about 680 d.v.) the maximal displacement of the localization is approximately inversely proportional to the frequency, except that the rule does not hold for very high frequencies (*i.e.* those above about 6770 d.v.).[45] Stewart noted that the movement of high tones, when phase is varied, is restricted.[46] A frequency of 1280 d.v. moved somewhat for him but 1536 did not move at all. Hartley put the critical point at about 600 d.v. and noted the difficulty of localization for greater frequencies: "most observers.....find from about 600 cycles upward the sense of location by phase-difference becomes less trustworthy."[47] In general then there is agreement between the times of von Hornbostel and Wertheimer on the one hand and the work of the physicists on phase on the other.[48] At about 600–800 d.v. the amount of displacement begins to be restricted, and the restriction becomes greater for still higher frequencies.

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[42] *Cf.* Halverson's charts, 1922, *op. cit.*, 181.

[43] It is plain that Klemm's values are approximate only (*op. cit.*, 1920, 126) and that therefore he can note general agreement with Hornbostel and Wertheimer (*ibid.*, 146).

[44] Stewart, *op. cit.*, 435-437.

When phase-difference can not be greater than 30 $\sigma\sigma$ (Hornbostel and Wertheimer), *i.e.*, when frequency is greater than about 16,000 d.v., we ought to reach the limiting case and to find the tone anchored in the median position. The technique for varying phase when wave-length is so short is difficult. Bowlker found 3080 d.v. hard to localize,[49] and, as we have seen, his law of proportionality between displacement and frequency broke down above 6770 d.v. Stewart got no certain movement at 1536 d.v. The data are scanty, but there is every evidence that the localization of high frequencies does not vary with change of phase. The direct researches give 1500 to 7000 d.v. as the critical point; von Hornbostel and Wertheimer's finding implies that the critical point should be at about 16,000 d.v. At any rate all agree that high tones are difficult of localization, a state of affairs that would exist if the localization were fixed and therefore not indicative of the actual position of the object in space.

Incidentally it is to be noted that 16,000 d.v. is not very far from the upper limit of hearing. Most audible tones therefore would admit of some variation in their localization as dependent upon their phase-relations at the two ears.

Volume and Phase. When the phase-relations of a diotic frequency are altered it would seem that the volume of the tone should vary. When the frequencies are in phase and the localization is median, we are supposing that the two adjacent cortical regions are simultaneously excited and that a single field of excitation results in an intermediate mode. It might seem at first thought that the dispersion about the mode would, however, be greater in this case where both the overlapping regions are excited than in the simpler case of lateral localization where only one region is excited.

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[45] Bowlker, *op. cit.*, 326 f.
[46] Stewart, *op. cit.*, 437 f.
[47] R. V. L. Hartley, *Phys. Rev.*, N. S. 13, 1919, 374, 382.
[48] As Hornbostel and Wertheimer themselves point out, *op. cit.*, 395 f.
[49] Bowlker, *loc. cit.*

It was some such view as this that led Halverson to measure the volume of diotic frequencies when the phase-relation was varied.[50] He found, as he had anticipated, that the medianly localized tones where the frequencies were in phase were relatively large, and that, as localization passed toward one side when one frequency was made to lead the other in phase, the volume of the tone was decreased. To his surprise, however, he discovered that volume had reached a minimum when the phase-difference was somewhere between ⅛ and ¼ of a wave-length, and that for greater phase-differences the volume increased again, though more slowly, until it was about as great at lateral localization, with phase-difference half a wave-length, as it had been when the localization was median and the frequencies in phase. Thus Halverson's expectations, which I shared with him at the time, were borne out only in part.[51]

No one would, I think, have expected volume to vary at all with phase, unless they were led to the expectation by some such train of thought as is set forth in the present paper.[52] That it did vary with phase seems to me to strengthen our argument very greatly indeed. Nevertheless the difference between Halverson's finding and his expectation requires explanation.

The explanation is not, I think, hard to conceive. We have assumed that volume is dependent upon central dispersion and that dispersion occurs in two ways: in part it is a function of intensity, since the greater the intensity the greater the dispersion; but in part it is also a function of the dual locus of excitation, since an excitation whose source lies in two adjacent regions tends to be more dispersed than an excitation from a single region. We have had to assume further, in order to explain variation of localization with intensive difference, that, when the two regions are excited successively and the interval of time is brief, there is a partial inhibition of the later by the earlier. We have noted that such a partial inhibition might occur because the excitation of the fibers in the ears would not be instantaneous, and the earliest fibers in the later ear might be stimulated before the latest fibers in the earlier ear. We have now to note that such a state of affairs would lead to some degree of mutual inhibition. Not only would the excitation from the earlier ear fail completely to inhibit the excitation from the later ear, but that portion of the excitation from the later ear which was realized would have a partially inhibitory effect upon the excitation from the earlier ear. It is a very simple picture, which the reader should fix in mind before we proceed.

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[50] Halverson, 1922, *op. cit.*, 526-534.

[51] Halverson's expectation has been stated as fact by Watt: "the binaural sound has a greater volume than the uniaural:" 1920, *op. cit.*, 164 f. He cites S. Baley (*Zeits. f. Psych.*, 1915, 70, 347-365) and Klemm as evidence. Halverson's results do not directly contradict this statement but they give a very different implication: even if the uniaural sound is smaller than the binaural, nevertheless the smallest sounds occur for intermediate conditions. If we accept Watt's verdict on Baley and Klemm, we are saved much trouble for we need proceed no farther in this matter. I am inclined to think, however, that we must take Halverson's function seriously until the matter is settled by more research. Halverson is convincing because he trained his observers especially for volume, he checked the training by duplicating Rich's volumic limens, he made a direct attack, he worked with a careful psychophysical procedure, he measured the differences of volume that he found, and he determined the volumic function along its entire range.

[52] Both Halverson and I had missed Watt's paper at the time Halverson's experiment was begun.

Now what happened in Halverson's experiment? With the frequencies in phase the medianly localized tone was large because both regions contributed to it and the dispersion of the excitation was consequently great. The tone was presumably not as intense as might have been expected, because of the mutual inhibitory effect of each set of excitations upon the other. Presumably also the volume was less than it might have been, because the tone was not maximally intense: volume varies with intensity. When a small phase-difference was established, the one region of excitation predominated over the other and the dispersion due to the dual locus of the excitation was decreased: the volume was less. We do not know what the intensity was and I can not guess what it may have been, because it should have been a function of the two simultaneously varying inhibitions. Finally a large phase-difference was created and volume was found to have increased again. It would seem reasonable to suppose that the tone is now increasing in intensity because of the removal of the reciprocal inhibition of the later frequency upon the earlier. Moreover, since intense tones are large, the tone grows larger again. If this explanation is correct, it means simply that, when phase begins to be altered from equal phase, volume is affected more by the change of dispersion due to the spatial separation of the two regions than by the change of dispersion due to the degree of excitation that depends upon the mutual inhibitions between the two regions; and it means further that the relative values of these two conditions of dispersion are reversed when the difference in phase has become large.

Intensity and Phase. The foregoing section implies that the intensity of a diotic frequency should vary when the phase-relation is varied dichotically. A laterally localized tone for a phase-difference of nearly half a wave-length should be more intense than an intermediately localized tone for a phase-difference of about ¼ wave-length. I can not see how to predict the intensity of the medianly localized tone when the frequencies are in phase, because I can not guess how well dispersed but relatively uninhibited excitations would summate.[53] Of these intensities, however, we know almost nothing at present. The research could be easily undertaken along lines similar to Halverson's work with volume as a function of phase, and would be almost an *experimentum crucis* as regards this portion of our theorizing.

The Nature of Volume. Throughout this paper we have treated tonal volume as if it were comparable to visual or tactual extensity. This view seems to me to be correct. Volume is a true pre-spatial attribute of extent, just as bare visual and tactual extensity are pre-spatial in the sense that they are the perceptual basis of size, form, and shape, but have to be 'worked up' with considerable supplementation into these more complex perceptions.[54] For tones there has not been the same degree of development of spatial perception, and for this reason tonal volume has tended to be overlooked in experience because it is not very important in life. The failure of common sense to have revealed it is, however, no argument against its primary nature. Experimental technique brings it easily under observation and finds for it precise and verifiable functions. Its possibilities as an aid to scientific theory become apparent, I trust, in this paper. It is, nevertheless, at the basis of auditory localization just as visual and tactual extent are basal to visual and tactual localization.

The discovery and establishment of volume as a tonal attribute removes the puzzle as to why other sensations should have extent and tones should not, when presumably the neural conditions of all are similar. Especially was the puzzle troublesome when the experience of extent was correlated in theory with extension of excitation in the cortex. Now, however, not only are we in a position to go further with auditory theory, but we are also better able to apply our knowledge of one sense to the formulation of problems in another.[55]

Pitch and Frequency. In the first two sections of this paper we have discussed the question as to whether the auditory nerve fibers are specific with respect to pitch ("place" theory, usually a resonance theory) or whether they function for intensive quanta. We have concluded there that the latter view is more adequate as accounting for intensity and, abandoning the traditional procedure which explains pitch first, we have begun with intensity. The complete argument for this view of hearing is nothing less than the entire paper up to the present point.

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[53] Watt, 1920, *op. cit.*, 164, cites Klemm, however, as estimating the intensity of a diotic sound as four times the intensity of a monotic sound.

[54] *Cf.* Watt. 1920, *op. cit.*, 167: "bi-systemic functions such as the binaural and binocular are still only ordinal or concerned with form, and not spatial."

[55] Of course the question still remains as to whether smells have volume.

The question now arises as to what becomes of pitch. Tentatively we assigned it, *faute de mieux*, to frequency. It is possible, however, in the present state of knowledge that plausible accounts of intensity and of pitch are mutually exclusive, since each sensory attribute attempts to preempt the same attribute of the nervous mechanism, *viz.*, the specificity of the nerve fibers. Is there, we must now ask, any plausibility to a frequency theory of pitch?

The battle between resonance-theories and frequency-theories still goes on,[56] and I am not ready to predict the outcome. I think, however, that the case for a frequency theory is no weaker than the case for a resonance theory,[57] and that the frequency theory is therefore on the whole much stronger because it can explain the facts of intensity, volume, and localization in the manner of the preceding discussion. Minton thinks that the case for frequency as the correlate of pitch has been strengthened by his discovery that even the congenitally deaf (mutes) can hear tones if the stimulus is loud enough,[58] although it must be noted that we are not ordinarily able to say in a particular deaf-mute just how undeveloped the cochlear mechanism is.

Ohm's Law. There is no escaping the fact that the great stumbling block to a frequency theory of hearing is Ohm's law: the hearing by an observer, presented with a complex periodic stimulus, of the tones that correspond approximately to the simple harmonic components of the stimulus as given by Fourier's analysis. This law is one of the most obvious mysteries of hearing and the *raison d'être* of resonance theories. Since the results of Fourier's analysis can, in the case of a complex wave-form in the air, be given, within certain limits of approximation, by a system of physical resonators, it has been natural for Helmholtz and his successors to seek such a system of resonators in the complicated mechanism of the inner ear. A frequency theory is said to shift the problem of analysis to the brain and then to offer no solution for it there.

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[56] *Cf.* H. Fletcher, *Jour. Franklin Inst.*, 196, 1923, 289.

[57] In spite of H. Hartridge, *Brit. J. Psych.*, 11, 1921, 277-288; 12, 1921, 142-146; 12, 1922, 362-382; 13, 1922, 48-51, 185-194; and in spite of Wilkinson and Gray, *op. cit.*

[58] J. P. Minton, *Proc. Nat. Acad. Sci.*, 7, 1921, 221-225; 8, 1922, 274-280 (esp. 276); *Phys. Rev.*, N. S. 19, 1922, 80-96. Ruckmick, *op. cit.*, 617, accepts Minton's work as "most serious criticism of the Helmholtz theory."

I have no desire to minimize this difficulty. I have only to say to those who have accepted the argument of this paper thus far, that Ohm's law then constitutes the outstanding problem of the psychophysiology of hearing. It is not, however, the only important problem. The thesis of this paper is that we are in the way of solving the problems of intensity, volume, and localization. I do not see that Ohm's law is to be preferred to them as the starting point.[59]

Even though we are not prepared to solve this problem at present, we may at least seek to formulate the problem.

In the first place, it is to be noted that the problem of analysis is not identical with the problem of analysis at the ear. Upon the organ of Corti the complex wave-form of the stimulus, somewhat distorted,[60] is imposed, and Fourier's analysis might be performed there by a system of resonators. At the hair-cells, however, we are supposing that the wave-form is translated into a series of discrete explosions that occur under the all-or-nothing law. At the brain we have then to deal, not with a complex wave, but with a periodic series of impulses, separated, in the case of a complex stimulus, by unequal intervals within the period.

At this point we must avoid the temptation to appeal to Wrightson's theory[61] for explanation. Wrightson held that if the "crests," "troughs," and "crossing points" (zero ordinate; median displacement) of a compound wave were regarded as critical points, then the various frequencies producing the compound wave would be found approximately reproduced by the intervals given by these critical points. Hartridge has shown that this analysis is not valid,[62] but one might wonder whether it might not hold if consideration were given only to the "crossing points" where "crossing" is in one direction; or one might consider only the crossing of some ordinate value other than zero. If the hair cells were stimulated only by bending the hair in one direction a given amount, then such "crossing points" ought to give the actual temporal occurrence of the induced impulses in the nerve fibers. It is not necessary, however, to repeat Hartridge's analysis for these altered conditions. A very little consideration of a few simple cases shows that the displacement is such that the component frequencies of a compound wave are not given in this way. I can not take space to develop these

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[59] For a criticism of Ohm's law, however, *cf.* Köhler, 1923, *op. cit.*, 430-451, and esp. 451-462.

[60] R. L. Wegel and C. E. Lane, *Phys. Rev.*, N. S. 23, 1924, 272-274.

[61] Wrightson (and Keith), *op. cit.*, esp. 24-31, 60-71.

[62] Hartridge, *Brit. J. Psych.*, 12, 1921, 248-252. *Cf.* also Boring and E. B. Titchener, this Journal, 31, 1920, 101-113.

1.31σ (763 d.v.) and a minimum of 0.43σ (2325 d.v.).[71] It is plain that the period varies with temperature and also with other conditions.

One of the other conditions seems to be the character of the particular nerve fiber, which may vary in the same nerve at a fixed temperature. Erlanger and Gasser have discovered that a single stimulation of a nerve may give rise to two, three, or even four impulses that travel at different rates and presumably along different fibers in the nerve, becoming separated in time after travelling a sufficient distance.[72] These impulses they call respectively the α, β, and γ waves, and they find that the different waves have different refractory periods. The faster the rate of propagation of the wave the less its refractory period. Their results for the sciatic nerve of the frog may be tabulated approximately as follows:

	Rate of propagation[73] meters/sec.	Refractory period σ	d. v.
α-wave	42.7	1.42	704
β-wave	24.7	2.06	485
γ-wave[74]	15.5	4.46	224

If the upper limit of hearing is taken as 20,000 d.v.,[75] then the refractory period of the auditory fibers would have to be as small as 0.05σ, a value much less than the least of the figures cited. In fact it bears to the least figure cited about the ratio that the least bears to the greatest. Are we therefore to consider a frequency theory untenable?

The answer will necessarily lie in the experimental determination of the refractory period of the acoustic fibers with a sound stimulus, an experiment which, I am told, is perfectly practicable since the action current can be measured by placing electrodes upon the medulla. The experiment should be performed upon a mammal (*e.g.* cat) and ultimately the upper limit of hearing should be determined for the same kind of animal.

While we are awaiting the research that will yield these results we may, it seems to me, assume that there is a reasonable possibility that the refractory period of the human acoustic fibers is as low as 0.05σ (20,000 d.v.). In considering this possibility there are three things to be kept in mind. There seems to be at times a tendency to refer differences between amphibian and mammalian nerves to the difference of body-temperatures, but Erlanger and Gasser have now shown that fibers of different properties exist in the same nerve and exhibit these different properties at the same temperature. Moreover, the discovery that the rate of propagation of the nervous impulse may vary within limits, in which the greatest is three times the least according to the fiber in which the impulse is excited, is not only a surprising discovery but one which prepares us for other individual differences among fibers. It may turn out that the auditory fibers are fibers with a very rapid rate of propagation[76] and with a very small refractory period.

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[71] C. S. Sherrington and S. C. M. Sowton, *Jour. Physiol.*, 49, 1915, 339, 342, 347.

[72] J. Erlanger and H. S. Gasser, *Am. J. Physiol.*, 70, 1924, 624-666.

[73] The average of the rates for 26° and 22.3° (p. 642), since they do not state the temperature to which the refractory periods (p. 646) pertain. Nor do they note this particular relation between the two variables, although it is a striking correlation.

[74] There was only one instance of a δ-wave, with a rate of propagation of 13.6 m./sec. as against 19.0 for the concurrent γ-wave (p. 642).

[75] *Cf.* C. C. Pratt, this JOURNAL, 31, 1920, 403-406; Ruckmick, *ibid.*, 34, 1923, 278-281.

In the second place, we find some hint from the data for localization. In localization very small differences in time are effective: differences of the order of $\sigma\sigma$ rather than of σ. The smallness of these times was a surprising discovery and means that we have to do with a very precise temporal mechanism: temporal variability is small. The variability of small magnitudes is of necessity also small. The variability of large magnitudes we are accustomed to think of as large, although there is apparently no necessity about the relation. I am inclined to think that there is a presumption in favor of the reversal of the proposition, a presumption in favor of supposing that the very small variability in the temporal events of the acoustic impulses implies a small magnitude of the times of those events, and naturally also, as Erlanger and Gasser's function indicates, a very rapid rate of conduction.

Finally, we must note that the form of the function for the recovery of the excitability during the relative refractory period is in part consistent with our knowledge of the upper ranges of hearing, even though the known times are not. It is a common observation that high tones are weak as well as small.[77] We have already seen that this statement probably means that a high tone of the same energy as a lower tone is weaker than the lower tone. Presumably high tones can be as intense as we please to make them if we increase the amplitude enough.[78] Moreover the actual point of the upper limit of hearing depends upon the intensity of the tone: the limit is raised with intensification but not in such a manner as to make one believe that it is capable of indefinite increase.[79]

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[76] The most accurate determination of the rate of nervous conduction in man is said to be Piper's value of 123 m./sec.: Bayliss, *op. cit.*, 393. It is possible that the rate is more rapid in man than in many other mammals. If the correlation of the table above be extrapolated graphically by eye, it appears that a refractory period of 0.05 σ (20,000 d. v.) should be correlated with a rate of about 150 m./sec. in a frog. On this curve a rate of 123 m./sec. should give a possible frequency of 4000 to 6000 d. v.

[77] Helmholtz, *op. cit.*, chap. IX (*cf.*, note 20 *supra*); C. Stumpf, *Tonpsychologie*, 1883, I, 206 f., 365-371.

Most of these psychological facts would follow from the curve of nervous excitability. As frequency increases the point is reached where successive excitations fall within the relative refractory period. Now, although we have assumed that displacement begins at the base of the cochlea and spreads upward, it is apparent that there would be a pressure gradient along the organ of Corti. The displacement would travel a little way up the cochlea before it was maximal at the base and in this way a gradient would be established. Its slope would depend upon the relative values of friction and of membranous resistance. If such a gradient is established successively within the relative refractory period, it is evident that for a given amplitude fewer fibers than otherwise would be excited, since the threshold of excitation is raised in this period. With fewer fibers excited one gets a tone of decreased intensity, and the intensity would become less as the frequency became greater (*i.e.*, as the excitation occurred earlier and earlier in the relative refractory period).

The absolute refractory period ought to determine the upper limit of hearing, and there is a certain similarity between Fletcher's curves for the limit and Adrian and Lucas' curve for excitation in the refractory periods that bears this presumption out. Nevertheless the fact that the upper limit is raised slightly by increasing the intensity of the tone makes me think that the curve of excitability ought to have a "foot" where it comes to the x-axis and not come abruptly up against the axis.[80] Most biological functions of this sort are smooth and continuous.[81]

Tonal Lacunae. The occurrence of tonal lacunae, with tonal islands beyond them, in the upper ranges of hearing has been looked upon as one of the most positive indications of a place-theory of pitch. If a limited range of frequencies becomes inaudible, what is more natural than to suppose that certain parts of the organ of Corti have degenerated?

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[78] This is the general basis of the modern work in which energy as well as frequency is varied and controlled. *E.g.*, see Fletcher, *op. cit.*, esp. Fig. 1; Minton, *opp. citt.*

[79] E. F. Möller, this JOURNAL, 33, 1922, 570-577.

[80] Like Lucas' similar curves for muscle, *Jour. Physiol.*, 39, 1909, 333-338.

[81] A discussion similar to that of the last three paragraphs can be given for the lower region and limit of hearing, a discussion that deals with the supernormal phase of excitability and the problem of frequency and succession. The lower limit, however, does not bear the same critical relation to the content of this paper as does the upper limit.

One important proof of this nature is held to be Yoshii's experiment.[82] Yoshii exposed guinea pigs to continuous tones for intervals of the order of 50 to 60 days. He used an Edelmann whistle at c^5 (4138 d.v.?), a siren at c^3 (1034 d.v.?), a whistle at b^2 (976 d.v.?), and an organ pipe at g (193 d.v.?). After exposure to one of these stimuli, the animal was killed and the cochlea examined histologically. In general he found destruction of the organ of Corti over a range of one-quarter to one-half a turn of the cochlea. Moreover, he found that the regions of destruction occurred in the order of frequency with the highest frequency toward the base of the cochlea.[83] Since the small end of the basilar membrane lies at the base of the cochlea the results might seem to support Helmholtz' resonance theory, although Yoshii himself concludes that the region affected is too broad for Helmholtz' theory and falls back upon Ewald's.[84]

It does not seem to me, however, that the facts of the tonal lacunae help us very much in theorizing.

In the first place, it appears that Yoshii's results are more easily explained by the mechanism outlined in this paper. Yoshii varied frequency, but the stimuli that he used would also have given wide differences of amplitude. If a stimulus continues to displace the organ of Corti many times a second for two months, it is not at all improbable that degeneration might occur at the point where the moving region joins the quiescent region, for at this point the greatest strains would be produced. It seems to me, therefore, probable that Yoshii got destruction for c^5 much nearer the base of the cochlea than the destruction for g, because the amplitude of c^5 was much less than the amplitude of g, and not at all because the small end of the basilar membrane lies at the base of the cochlea. One test of this view would be to repeat Yoshii's experiment with a c^5 and a g in which the amplitude of the c^5 was made greater than the amplitude of the g, and to see whether the regions of destruction were reversed. Another test would be to repeat the experiment, and then, instead of killing the animal, to test by discrimination methods his ability to hear the frequency to which he had been exposed. If he could still hear it, then it would be in order to kill him and determine histologically whether there had been destruction of a given region. Finally, as against the support of a resonance theory by this experiment, we must note that the broad region of destruction by a single frequency, not only tells against a theory of resonance of basilar fibers, but is also just what one would expect if the cochlear pressures form a gradient (*vide supra*); thus providing a somewhat indefinite termination to a given amplitude.

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[82] U. Yoshii, *Zeits. f. Ohrenheilkunde*, 58, 1909, 201-250. There are ten plates which give an excellent picture of the results. *Cf.* Wilkinson and Gray, *op. cit.*, 199-201.

[83] Yoshii's figure (p. 244; or Wilkinson and Gray, p. 200), showing roughly the cochlear locus of his stimulations, seemed to me to group the range from g to c^5 in too small a region, *i.e.*, in four of the eight half-turn sections of the canal. A rough estimation of the centers of the affected regions showed, however, that the frequencies were spaced approximately in accordance with their logarithms, and that a logarithmic extrapolation would give limits of hearing at about 16 and 16,000.

[84] Yoshii, *op. cit.*, 244-248.

The occurrence of tonal lacunae as a phenomenon of human senescence is not, however, to be put aside in the same manner. Here we have, not an indirect argument from animal histology, but direct introspection. Old people have been said to be unable to hear certain frequencies. Recent research, however, indicates that there really are no tonal lacunae in the strict sense. Sensitivity for certain regions of frequency may become diminished, but even here the tones can be heard if they are intense enough.[85] Thus the problem is somewhat altered. We must merely ask why certain frequencies should be less effective for hearing than frequencies on either side of the region under consideration.

To this question there is no immediate answer. We can observe only that the relation of pitch to intensity is very intimate and that irregularities in the relationship are most apt to occur in the upper ranges of hearing. Since the upper ranges, according to our present view, represent the critical region where the limit of frequency, as dependent upon the refractory period, is approached, we might expect irregularities due to but slight variations in the properties of the nerve fibers.

It is important to note, however, that Möller has shown that there may be diminished sensitivity over a limited range of intensities as well as over a limited range of frequencies.[86] If it is difficult for the proponents of a frequency theory to explain "lacunae" in pitch, it will be equally difficult for the proponents of a place theory to explain "lacunae" in intensity. Moreover, I am inclined to think, we are cultivating an anatomical artifact of thought when we suppose that a phenomenon, like an inversion in a continuum of sensitivity, must be represented by a derangement at some particular "spot" in the organism.

SUMMARY

This paper presents a general view of the physiological mechanism of hearing with special relation to auditory intensity, volume, and localization.

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[85] Fletcher, *op. cit.*, 315; and, in general, Minton, *opp. citt.*

[86] Möller, *op. cit.*, 577; *cf.* Fig. 1 with Fig. 4, p. 574. Similar relationships could be made out from Fletcher's audiograms; *e. g.*, *op. cit.*, 304.

It begins with the problem of intensity, as an aspect of tonal experience that has been more neglected in theorizing than has pitch. It embraces the all-or-nothing law of nervous excitation, and tentatively rejects Forbes' theory of sensory intensity, which it explains as summation of the excitations of all the auditory fibers stimulated (pp. 158 f). It discusses the problem of qualitative and intensive quanta thus raised, and decides tentatively in favor of the latter (p. 160). It suggests further that intensive summation requires some sort of integration in the brain, and indicates that such an integration would probably necessitate an excitatory dispersion similar to Bernstein's theory (pp. 160 f).

In such a view, the paper maintains, the direct correlate of auditory intensity would be the amplitude of the tonal stimulus and not the energy (p. 162).

Moreover it is shown that this view requires a relation between degree of excitation and dispersion, and thus between intensity and volume. This relationship has been discovered experimentally by Halverson (pp. 162 f). The better known relationship between volume and pitch is shown to have been possible because it was determined without control of the amplitudes of the frequencies used (pp. 163 f).

The paper outlines the intensity-theory, the time-theory, and the phase-theory of auditory localization, and shows the experimentally determined relationships between them (pp. 164ff).

By adding to the principle of cortical dispersion, already adopted, Watt's assumption that the fibers from the two ears are distributed to adjacent overlapping regions of the brain, it proceeds to explain the phenomena of the intensity-theory and to show that this theory of localization is fundamental (pp. 166 ff). It then reduces the time-theory to the intensity-theory by the further addition of a mechanism of inhibition (pp. 168 ff), and finally reduces the phase-theory to the time-theory by an appeal to the all-or-nothing law (pp. 170 ff). In this last reduction it seeks to determine the degree of identity between the time-intervals upon which the two theories have been based.

The paper then notes that tonal volume should depend upon the phase-relations of a diotic frequency, cites Halverson's experiment upon this relationship, and seeks to explain by mutual inhibition the discrepancy between Halverson's expectation and his finding (pp. 173 ff). It outlines a further problem of the dependence of intensity upon phase, which is as yet unsolved (pp. 175 f).

The nature of tonal volume as a prespatial attribute of extent is discussed (p. 176).

The paper then points out that, if the foregoing discussion of intensity is accepted as valid, pitch must be correlated with the frequency of excitation (pp. 176 f). It seeks, therefore, to formulate the problem of Ohm's law for a frequency-theory of pitch, contenting itself with the formulation of the problem in the lack of any satisfactory solution at present (pp. 177 ff).

It discusses the relation of a frequency-theory of pitch to the known times of the refractory period of nervous excitation, and concludes, although the known refractory periods of mammalian, somesthetic, afferent nerves are too long to account for human hearing by a frequency-theory, that there is some reason to believe that the refractory period of human acoustic fibers is shorter, and that the final decision must wait upon research (pp. 180 ff).

Finally the paper notes that the argument from tonal lacunae is not fatal to a frequency-theory, partly because the results of Yoshii's experiment are ambiguous as against the two types of theory, and partly because recent research has thrown doubt on the existence of tonal lacunae (pp. 183 ff).

Experimental Problems

In the argument of this paper the following problems for experimental attack have been outlined. All of these investigations have a critical relation to the view of the physiology of hearing set forth in the paper.

1. The direct equation of frequency and amplitude as conditions of volume. Can a weak low tone be equated to a loud high tone in volume? (p. 164).

2. The determination of the relative intensity and of the relative volume of a diotic frequency. To make this study general more than one frequency should ultimately be used (p. 168).

3. The plotting of the function that localization is of the time-interval between two discrete and temporally dichotic sounds. This investigation is an extension of the work of Hornbostel and Wertheimer and of Klemm. It should bring their discrepant values into accord, should show the form of the function between the critical points, should show in particular for what range of time-intervals the sound remains stationary in the lateral position, and should determine exactly the point of appearance of successivity (p. 171).

4. The determination of the exact time-difference or phase-difference during which bilateral localization of a sound or tone persists (pp. 171 f).

5. The determination of the function that localization is of phase-difference for different frequencies, and the treatment of the results with a view to showing whether localization, when phase-relation is varied, is dependent upon absolute time-

differences or upon relative phase-differences. Stewart has published limited conclusions with respect to this problem, but it might well be made the object of a concentrated attack (p. 172).

6. The determination of the function that tonal intensity is of phase-difference in a diotic frequency, and thus the formulation of a decision as to whether intensity varies at all when phase is altered (pp. 175 f).

7. The determination of the absolute and the relative refractory period of some mammalian acoustic nerve, together with the determination of the upper limit of hearing for the same kind of animal (pp. 159, 181).

8. The repetition of Yoshii's experiment on the degeneration of the organ of Corti with prolonged tonal stimulation, using a high tone of great amplitude and a low tone of less amplitude, in order to discover whether frequency or amplitude is the condition for the locus of degeneration (p. 184).

9. The repetition of Yoshii's experiment with subsequent discrimination tests of the animal, in order to discover whether after prolonged exposure to the stimulus, and presumptive degeneration in the organ of Corti, the animal can still hear the tone to which it has been exposed (pp. 184 f).

The paper suggests other problems, but these nine seem to me the most crucial.

THE PSYCHOLOGY OF *GESTALT*[1]

By HARRY HELSON, University of Illinois

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(26) *Foreword*

We are now to make a critical estimate of the chief contributions of the configurationists to psychology. In making this it will be necessary to discuss the fundamental problems raised by the configurationists and the facts upon which they are based. It is impossible to avoid these fundamental issues and questions of methodology if we are to be clear in our thinking about rival hypotheses and to be left free to face the facts apart from the mandates of authority which beset us on every side in psychology. Although we have a fairly respectable body of fact established by experimental methods, albeit from different points of view, we are still far from agreement about the subject-matter and treatment of data in our science. Just as the configurationists have made an advance in exposing the gaps and weaknesses of many of the old theories, so we may hope to profit by a discussion of the anomalies and problems suggested by the experimentation and interpretations of the configurationists. My chief purpose in this inquiry is to help clarify the issues raised by the introduction of the concept of *Gestalt* into psychology. I shall ask how far the configurationists have carried out their own programme of reform, to what extent they have contributed to basic problems, how we can assimilate the new theories, and in what direction we may look for further progress.[2]

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[1] This article is the fourth and last of the series on the psychology of *Gestalt*. The other three have appeared in this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62.

[2] In this article I must confine myself to fundamental and general considerations of the *Gestalttheorie*, avoiding as far as possible philosophical problems and detailed questions concerning particular experiments or hypotheses. The philosophical presuppositions and implications are of interest mainly to the philosopher since the scientist will be more interested in the working of a theory for particular purposes than in its truth. Thus a theory of mind-body will be open to criticism on philosophical grounds, yet one theory may be very much better than others for scientific purposes. Disputes over particular facts must be settled by experimental methods and these I leave for the future.

One of the most common "criticisms" made about *Gestalttheorie* is that "it is nothing new."[3] Such a statement dismisses further consideration of the theory on its own merits. Before proceeding with a discussion of configurationism from within the system itself, I wish to mention a few points wherein the configurationists have certain very definite contacts with the past and wherein they may be regarded as unique. First, several stock criticisms of traditional psychology made by the configurationists can be found in numerous sources: Cornelius objected to atomism and the introduction of special acts to unify the atoms in psychology; Herrick and Rahn[4] criticized the sensation; James[5] rejected an *actus purus* to explain spatial relations; Becher and von Kries[6] found the prevailing physiology inadequate to explain complex mental processes. Secondly, many of the solutions offered by the configurationists have been proposed in the past: Hering[7] mentions the autonomy of the eye as an explanation of certain phenomena; Ebbinghaus[8] postulated physiological processes having properties similar to the phenomenal intuitions of space and time, unity and plurality, constancy and change; Wundt[9] endeavored to account for new emergent qualities in perception. Thirdly, something like a configurational point of view has been growing in biology, physiology, and physics: Haldane[10] insists that the body is more than a sum of parts and that an organism and its environment are one; Herrick and Child[11] have stressed the total functioning of the nervous system; Whitehead[12] has advocated a configurational point of view in physics according to which "the concrete enduring entities are organisms, so that the plan of the *whole* influences the very characters of the subordinate organisms which enter into it. Thus an electron within a living body is different from an electron outside it, by reason of the plan of the body; but it runs within the body in accordance with the general plan of the body, and this plan includes the mental state. But this principle of modification is perfectly general throughout nature, and represents no property peculiar to living bodies." The concept of the *Gestalt*, it is readily granted, is not the property of any single group of investigators; but it is equally true that no other group of psychologists has taken the various problems and solutions offered to them and united them into such a far-reaching programme involving fundamentally new assumptions, new attitude and a new method of attack.[13] In dealing with certain aspects of both traditional and configurational psychology, the legend for this paper might well be the following sentence from Ritchie: "Positive increase in knowledge only comes when a proposition thought to be true turns out to be false."[14]

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[3] Thus Charles Fox believes that Ward gave a death blow to atomism and Stout stated the whole of *Gestalttheorie* in a nutshell, but calling a spade a spade turns up no earth! C. Fox, *Brit. J. Psych.*, 16, 1925, 56-61.

[4] H. Cornelius, *Einleitung in die Philosophie*, 1903, 204 ff.; C. J. Herrick, Introspection as a Biological Method, *J. of Phil., Psych., etc.*, 12, 1915, 543; C. Rahn, The Relation of Sensation to Other Categories in Contemporary Psychology, *Psych. Monog.*, 16, 1914, no. 67.

[5] W. James, *Principles of Psychology*, 1890, I, 244 f., II, 148 ff.

[6] E. Becher, *Gehirn und Seele*, 1911; J. von Kries, Ueber die materiellen Grundlagen der Bewusstseinserscheinungen, *Program der Universität Freiburg, i. B.*, 1898.

[7] E. Hering, *Grundzüge der Lehre vom Lichtsinn*, 1920, 11.

[8] H. Ebbinghaus, *Grundzüge der Psychologie*, 1905, 442 f.

[9] W. Wundt, *Introduction to Psychology*, 1912.

[10] J. S. Haldane, *Organism and Environment as Illustrated by the Physiology of Breathing*, 1907, 99.

[11] C. J. Herrick, *An Introduction to Neurology*, 1922; C. M. Child, *Physiological Foundations of Behavior*, 1924.

[12] A. N. Whitehead, *Science and the Modern World*, 1925, 111-112. More technical discussions will be found in Whitehead's earlier works: *The Concept of Nature* 1920; and *An Enquiry Concerning the Principles of Natural Knowledge*, 1919.

(27) *Methodological Considerations*

It is necessary to separate two distinct contributions which the configurationists have made to psychology. Questions concerning the subject-matter, treatment and goal of psychological efforts involve certain methodological considerations which the configurationists have emphasized along with their demands that the specific properties of wholes must be preserved and taken into account. While the concept of the *Gestalt* is closely allied with problems of method and is seldom separated from them in the configurational literature, we may discuss them separately for purposes of convenience and because problems of method are more fundamental than any particular scientific theories or hypotheses. My justification for making this distinction has been well stated by a logician as follows: "Theory may supersede theory and more accurate analysis may demolish our apparent facts, but there is a unity and continuity about the [scientific] method that the mind should be able to grasp and that is the very essence of science."[15] Furthermore, the answers given to methodological questions are of more than theoretical interest. Fundamental assumptions, beliefs concerning what and how a science should study, influence laboratory procedure and experimental results as well as systematic interpretations. Thus Klemm admits that the importance of the Würzburg school lies not so much in its insistence upon new mental elements as in extending the experimental method far beyond its original field in sense psychology to the most complex cognitive experiences. We must thank Külpe and his co-workers, then, for "the disappearance of the earlier and more restricted conception of the scope of the experimental method."[16]

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[13] Mach, von Ehrenfels, Meinong, Höfler, Witasek, Benussi, Schumann and a number of others had their chance to formulate a configurational theory in the grand style. But Mach's conception of science was simply to describe in terms of elements (sensations); von Ehrenfels allowed Meinong to transfer the problems of the *Gestaltqualität* to logic, epistemology, and *Gegenstandstheorie;* Höfler, Witasek and Benussi simply followed Meinong in the main; Schumann in four classical articles discussed many configurational phenomena at length but could not shake himself loose from the traditional point of view. One might go on *ad libitum* tracing connections and glimmerings of configurationism yet it remained for Wertheimer to give the theory a formulation which would lead to productive experimental research.

[14] A. D. Ritchie, *Scientific Method,* 1923.

[15] *Ibid.,* 14.

[16] O. Klemm, *A History of Psychology,* Eng. trans. 1914, 136. The advance on the side of method is certain even if the problem of "imageless thought" has never been settled.

The most important aspect of *Gestalttheorie*, as it seems to me, is the fact that it has broadened the scope of experimental psychology to include the more complex and integrated forms of experience and behavior which have been neglected by associationists, introspectionists, and behaviorists alike. The configurationists have advanced the cause of an objective psychology by striving to account directly for the data of observation without reducing them to mental elements, subjective categories or denying the existence of certain classes of facts. They have shown how it is possible to make fruitful assumptions and hypotheses in psychology on the basis of precise measurements and systematic variation of conditions, thus following the method of generalization, inference and verification which has proved to be productive in bringing order, unity and control of data in the other sciences. The configurationists have described certain facts, stated the conditions under which they were obtained, and formulated theories which have then been tested by further experimentation and variation of conditions. The communicability and verifiability of some such procedure as this, suggesting further problems and theories, may be said to constitute the very heart of (objective) scientific procedure.

The configurationists have seen that the chief difficulties of present-day psychology center about the assumptions underlying the methods of analysis and they accordingly make analysis the chief target of their criticisms. In so far as they reject *false* or *unfruitful* methods of analysis I find myself in complete agreement with them; but in so far as they reject *all* analysis, making no distinctions between necessary and valid analyses (which they themselves are making) I must part company with them. Let us first see wherein they have right.

The history of various psychological systems shows how narrowly the field of psychology was limited. The limitation of the sphere of experimental and scientific psychology to a description of experience or behavior in terms of mental or physiological elements rests partly upon a superficial knowledge of methods of analysis in the other sciences, particularly physics, partly upon metaphysical assumptions as to the inherent differences between mind and body, meaning and fact, or a dozen other artificial antitheses set up by members of various "isms." Behaviorists and introspectionists, Wundtians and associationists alike have erred in supposing that if a given event could be reduced to its lowest terms, physical or mental, it was thereby explained.[17] But a direct analysis can seldom guarantee the necessary and sufficient conditions under which a phenomenon may be held to be explained. Analysis for the purpose of describing data in terms of their most elementary constituents will at best give us only a limited set of entities obtained under restricted conditions. Real elements which will enable us to predict how a given phenomenon may be produced are obtained in a more roundabout and subtle fashion than many have supposed. Instead of chopping a given datum or event into its simplest components directly, the physicist and chemist owe their successful analyses to the fact that they have systematically varied the conditions affecting the object of study until a consideration of the observed facts demanded certain scientific objects (elements) and relations in order to achieve a coherent picture of all facts of the same kind. The molecular and atomic theories of matter have gained wider and wider assent not because physicists have chopped samples of every known kind of object into observed molecules, but because observation and measurement of gases, liquids, solids and electrical phenomena point more and more strongly to the existence of electrons and protons if we are to bring all the observed facts into a logical, coherent scheme. The real method of analysis is therefore indirect and has a definite methodological aim, namely, to subsume large classes of facts under general laws and theories based upon a set of assumptions acceptable to all on account of their simplicity, convenience, explanatory power and suggestiveness.[18] Whoever begins with *a priori* descriptive categories in modern science will soon find himself left in the rear while others march on to new conquests.

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[17] I mean by explanation only what any scientist means by it, divorced from metaphysical implications which have troubled no one since Hume: a statement of a fact by reference to the necessary and sufficient conditions which we are satisfied produce it. "Satisfaction" in science usually rests upon certain assumptions which are easily acceptable and prove fruitful.

Given the notion that the aim of science is direct analysis, the traditional psychologies have been further hampered by philosophical or metaphysical notions as to what the ultimate products of analysis should be. Behind Wundt's conception of analysis lurked the metaphysical belief that *causa aequat effectum* does not hold of mental processes, hence physical and mental facts must be eternally different and the task of psychology is to analyze experience into its psychic elements. Sometimes more complicated processes like thinking, judgment, and the perception of forms are explained by reference to special psychological causes having no counterpart in reality, as in the systems of Meinong, Witasek, Benussi and even Helmholtz.[19] It would seem as if the only "real" psychological data for widely differing schools of psychology are elementary qualities, all else falling on the side of meaning, value or what not. Strangely enough, while insisting, as psychologists, on the integrity of sensational qualities like red, sweet and fragrant, refusing to reduce them to ether vibrations or chemical reactions, the members of the analytic schools have reduced the perceptual and thought processes to elements from which they could be derived or predicted as little as a congenitally blind person could tell what red is from a knowledge of the corpuscular or wave theories of light.

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[18] We must take the terms simplicity, convenience and the like for granted in this paper. To show what they really mean would require a book dealing with scientific method, mathematical logic, and the history of science!

[19] As psychologists we do not rest content with naming things "illusions." We try to find objective explanations for them. So the special "higher" processes and many "psychological" factors used in explanation in psychology are but names to avoid the real issues.

If psychology is to pass beyond the elementary stage of descriptive classification, the stage of naming, it must be able to predict what will happen under any conditions affecting a given experience or reaction. More than a reduction into elementary units of mind or behavior is necessary, for it is evident that we cannot control the sensory elements constituting the understanding or the reflexes utilized in intelligent behavior. Prediction tells us whether we understand the mechanisms responsible for a given fact, if we can control its conditions and have hypotheses for future work. We must be able to compel, to produce phenomena, and not merely to enumerate or describe after the method of agreement, neglecting the intelligent reactions and the reports which do not fit into our scheme. When our descriptions help us to form a picture of the mechanisms underlying the production of facts so that we can vary and test them, we have widened our knowledge from the particular to the general, from the observed present to the unobserved future. A program of this kind is what is generally meant by science.[20]

What has all this to do with psychology proper! We may summarize it by saying that the old psychology, restricted by its concept of mental or behavior elements found itself unable to deal with large classes of phenomena which it forthwith threw into the wastebasket of "conventionalized habit responses," "meanings," "logical associations," and "value categories." Not that the importance of the problems hidden in these catchwords has always been overlooked by psychologists, for the behaviorist attempts to reduce meaningful responses to unit reflexes, Meinong invented a science of *Gegenstandstheorie* to account for objects, Husserl has developed the method of phenomenological reduction for the investigation of pure meanings or essences, and Münsterberg and J. S. Mill turned to a second science of character, value, or life to take care of the volitional, affective and logical aspects of behavior! What we can profitably deal with depends upon the general state of knowledge both in psychology and the other sciences upon which it depends. It is clear that we must avoid both the bewildering demands made upon science by everyday interests and the logical abstractions which creep into systematic points of view. I do not maintain that all meanings are worth investigating in psychology; for the first thing we have to do is to neglect most of our everyday interests and experiences. But every science deals with long-section, changing, dynamic events which psychologists for the most part have dubbed meanings, functions or acts, and then comfortably curled up in their own systems. Should the physicist discard the electron because it is a meaning? No one has yet observed it, and it therefore still remains a logical construct! Even the concepts of time, space and mass, which were formerly thought to be fixed observable "reals" in science now have to be modified in the light of relativity and quantum theory. The meanings which we assume as constant, *e.g.* the electron, atom, sensation, *Gestalt*, will be those which are pragmatically worth keeping and investigating; those which are not will either be replaced by or derived from still more fundamental entities, either observed or inferred. It is more important to measure and control conditions and to make assumptions which set one to work than to search for the ultimate, irreducible facts of existence.

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[20] It must not be thought that we can dispense with pure description, whether we mean by it merely naming or classification, or an accurate characterization of facts. The biological sciences have been predominantly descriptive for want of better means of handling their data. But the accurate description of fact, even though it be in terms which enable others to point to it, does not settle the scientific problem by any means. Then, too, there are at least two different kinds of descriptions: descriptions in terms of qualities and descriptions in terms of relations. One writer, C. D. Broad (*Scientific Thought*), goes so far as to say that description by any set of elements will do, provided the relations are of the right sort. Entities, qualities do not furnish the best means of explaining phenomena because they vary too much from individual to individual, they are unique and seldom communicable. Relations are general, common and controllable and hence are to be preferred to qualities. We shall have to return to this point later.

The configurationists have widened the scope of psychology by dealing scientifically with certain classes of phenomena which were formerly neglected or badly described under such headings as meaning. They have ventured to deal with dynamic processes extended in time, forms, complexes, comparisons, the "higher thought processes," and intelligent human and animal behavior. "All of which has been done before," one may say. But it has generally been for the purpose of reduction: spatial perception has been reduced to complexes of non-spatial sensations; intelligent learning is supposed to follow laws of nonsense memorization; intelligent behavior has been described in terms of chance fixations of unit-reflexes. The assumptions behind this kind of reduction are that only static phenomena can be described in science and only static terms can be used in explanation. But just as the mathematical equation may summarize a dynamic event in static form, so it is possible, if we approach our problems with the intent of formulating laws, to describe and explain many dynamic facts which are often dismissed on *a priori* considerations as unfit material for scientific investigation.[21]

The configurational touchstone, a functional one, is generally valid so far as any single criterion of the scientifically real can be applied. If any factor within a given situation makes an observable, measurable difference, it is real. The only test of existence must be functional. A causal factor can be verified only by observable effects. For this reason the configurationists have stressed the importance of forms, patterns and objects, since these modify the attributive characters of experience wherever found. The configurationist assails both the behaviorist and the introspectionist because the former leaves out of account causal factors (like phenomenal fields) which may influence the concrete data he is studying, while the latter emerges from his analysis with a number of elements having no real (causal, observable) connection with the original fact to be explained. Our task is to formulate laws governing the appearance of phenomena and not merely to reduce them to any particular type of element or refuse to recognize a whole class of existences by verbal periphrasis. A really scientific psychology will some day formulate laws in which the long-section events will be fully and adequately described and explained.

Having destroyed the original datum of perception by analyzing it into "psychic" elements upon which few laboratories could agree because the conditions under which the analysis was made depended almost entirely upon the training and conditions within the observer, the analytical psychologies have been forced to assume special psychological processes like apperception, judgment and production-process to bring the psychic atoms together again. Since both the methods and products of analysis were felt to be peculiar to psychology, any possibility of making it an objective science seemed hopeless—

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[21] Even those systems which attempt to rule out functional concepts from psychology seldom do so consistently. Thus the concept of clearness or "attensity" proposed as a descriptive term for attention is normative, functional, as Linke (*Grundfragen der Wahrnehmungslehre*, München, 1919) points out, since to describe in terms of clearness implies a knowledge of the fact at some other time as a standard. That no system of psychology has or can dispense with meanings a careful examination of them will reveal. The real point at issue is not the inclusion or exclusion of meanings but rather, what meanings shall be kept, what meanings are worth investigating? I hope to deal with this problem more fully in a later paper on psychology, logic, meaning and perception.

hence the flight of the behaviorist. But the behaviorists' dictum that objectivity consists only in speaking in terms of behavior, even though a great many of the behaviorists' response-mechanisms are to be found only in his writings, seems as dogmatic to one who would deal with phenomenal data as Wundt's belief that the facts of psychology must be reduced to psychic elements. If we are to be really scientific, measuring, varying and bringing order into facts, then we must deal with them all with the best instruments we can construct, be they conceptual or mechanical. We shall then be guided in the choice of subject-matter and descriptive categories not by preconceived opinions as to what the scientifically real consists in, but rather by the importance and implications of any given fact for the laws and theories of psychology. We shall then regard different points of view, fundamental assumptions, and rival theories as mere tools for getting at the facts and furthering experimentation and not as the expression of the one and only absolute truth.[22]

(28) *The Problem of Analysis*

In attacking the methods of analysis of contemporary schools in psychology the configurationists have pointed out their main weakness and have offered a better method of procedure. Nevertheless their wholesale denunciation of all analysis *qua* analysis has done as much to obscure some points as to throw light on others. For the term analysis is broad and covers a great many different things, good as well as bad. Either the whole universe is a system (*Gestalt*) or it is not. If it is not, then there is no problem of analysis. If it is, to study anything short of the totality of phenomena means that we must isolate certain phenomena for scientific purposes. Köhler admits as much when he says that we confine our observations to measurable effects since the sphere of influence of a given entity may be infinite. The configurational theory rests upon the usual view of (relatively) independent systems so the points at issue resolve themselves into the following: (1) What is to be the basis of choice of our supposedly independent system which serves as the unit of observation and explanation? (2) When and how do we arrive at a scientific solution which the problems of any system offer us?

(1) My answer to the first question, as opposed to that given by the configurationists, may be defined as one based entirely upon pragmatic considerations. The first task of science is to select from the large and infinite number of possible

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[22] In this Section, and perhaps in the following, I have separated the logical or conceptual aspect of science too sharply from its factual side. Both interpenetrate each other. I hope this mutual interdependence will be shown in spite of the necessity for separating them.

objects of investigation some which are of especial interest or importance. The assumption must then be made that "the determinate value assumed by any variable is dependent in any instance, not upon an indefinite number of conditions which might be in some sense exhaustive of the whole state of the universe, but upon a set of conditions capable of enumeration."[23] It may well happen that what was thought to be a relatively isolated, autonomous system, say the sensation, proves to be influenced by certain factors, either within or without, which have not been taken into account. The supposedly isolated system must then be subjected to stricter scrutiny in order to include all possible conditions which affect it. Thus we now know that form, context and past experience all influence the simple attributes of experience and the ways in which "reflexes are combined." Two tendencies thus appear in scientific work: on the one hand, systems are constantly being reduced to sub-systems, the molecule giving way to the atom and that in turn to the electron; and, on the other hand, the endeavor is constantly being made to describe and explain larger and larger groups of phenomena by means of simple sub-systems. Isolation then proves to be relative and the kind of simple object and relation chosen to define the sub-systems will be pragmatic.

The assumption, therefore, that the *Gestalt* is a natural "unit" for scientific observation and explanatory purposes has little weight in itself. The natural unit of today becomes the commonplace or artificial unit of tomorrow. Nature is full of objects which must be replaced by scientific objects whose status varies from day to day. Thus whether we regard the electron as a self-sufficient entity or a center of energy whose sphere of influence extends to infinity must rest upon considerations of economy and usefulness.[24] The natural system of science proves to be merely a scientific object of a particular kind which later may give way to scientific objects of another kind. Whatever unit of description or explanation is finally chosen will be by merit of its general implicative and explanatory power. I agree with Koffka that the value of the sensation and the unit reflex as useful concepts for the complex patterns of experience and behavior has largely passed; but they have not passed because they were assumed as constants in an ever-changing organism, as Koffka goes on to say.[25] Some constants must be assumed in science. The *Gestalt* has become one in a certain sense since it is regarded as a definite causal factor. The value of constants lies in the possibility of framing laws, since laws are simply statements that certain constant relations or conditions are at the bottom of certain phenomena.

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[23] W. E. Johnson, *Logic*, 1922.

[24] Fürth takes the same view in Wechselrede to H. Dexler's Das Köhler-Wertheimer' sche Gestaltenprinzip und die moderne Tierpsychologie, *Lotos*, 69, 1921, 227 ff. Similar "pragmatic" positions will be found in books on the logic of science, *cf.*, C. D. Broad, *Scientific Thought*, 1923; and Ritchie, *op. cit.*

[25] K. Koffka, Zur Theorie der Erlebnis-Wahrnehmung, *Ann. d. Philos.*, 3, 1922, 375.

We have come to the point in psychology where we desire a more satisfactory account of certain phenomena which have either been neglected or very poorly explained by traditional theories. There are many who feel that scientific psychology has been unduly limited both in its choice of subject-matter and descriptive categories. They feel that a more adequate account must be rendered of perceptual objects, intelligent behavior and a large class of facts which have been disposed of under the label of "meanings."

Traditional psychology has not been the only science to neglect whole classes of important facts—important for the progress of science itself as well as the technologies which turn to science for help. Many of the most important changes in the concepts of physics and chemistry have been due to the fact that, as Whitehead says, "the eighteenth century scientific scheme (which I may add persisted throughout the greater part of the next century and into our own in some quarters) provides none of the elements which compose the immediate psychological experiences of mankind. Nor does it provide any elementary trace of the organic unity of a whole from which the organic unities of electrons, protons, molecules, and living bodies can emerge."[26] Pragmatic considerations are thus constantly forcing science to adopt new descriptive categories for the sake of unifying, completing and ordering the world of phenomena. In every case scientific objects are pragmatic constructions serving this or that purpose, either in extending boundaries, explaining old facts better, or encompassing new domains within the realm of the ordered realities.

That there is mainly a difference in emphasis in my dissent from the pronouncements of the configurationists appears from the following quotation from Koffka: "Das Kriterium der guten Beschreibung ist ja ihre Fruchtbarkeit. Eine ausführliche Beschreibung, die mir keinerlei Anlass zu neuen Fragen bietet, die sich überhaupt nicht 'nachprüfen' lässt, ist für die Wissenschaft wertlos."[27] But since the configurationists have felt obliged to prove the *reality* of *Gestalten*, forms, meanings, perceptual objects, which I would not exclude from the sphere of psychological existents, they are apt to call the pragmatically ideal case the "natural" one. No doubt we are trying to get nearer and nearer to the "actual" facts, but what they are will forever be a matter of research and new conceptual formulations. Thus Köhler has taken the ideal cases of (quasi-) stationary physical states to prove the existence of physical *Gestalten.*[28] I am willing to accept these as useful fictions for purposes of generalization or whatever scientific purpose they serve. But if Köhler and those who follow him mean that such systems *actually exist,* can be verified by direct observation, then I must say on the contrary that descriptions of nature, even though in terms of differential equations, do no more than *approximate* the facts. It requires little courage to say that all laws are but statements of probability. The factor of chance comes in when we consider: (1) if we have observed and controlled all the factors within a given situation; (2) if we can exactly reproduce the conditions under which the law was formulated; (3) if the law of error accounts for those factors beyond observation and control, like the individual molecule or errors in measurement; and (4) if the assumption that a given law will hold under the (total) state of affairs at any given time.

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[26] *Op. cit.*, 104.
[27] *Op. cit.*, 388.

That the concept of the *Gestalt* must be investigated like every other concept in science, *i. e.* as a purely empirical concept, is attested by several facts. We must know what the configuration is in every case so that we can identify it, not only by naming it, but also by stating the conditions under which it is produced. We may then be sure of its presence or absence, existence or non-existence. Then we must determine if it exerts a causal effect upon the data studied, *e.g.* if differences in the fact appear in different configurations. Further, we must discover how the configuration exerts its effects, unless we are to remain satisfied with the concept as a primitive idea, the mere statement of which is deemed sufficient to explain a situation. If we assert that a given phenomenon is due to the configuration, we are merely uttering tautologies, for we are only saying that the total state of affairs is responsible for something in the total state of affairs, or that the total situation is what it is by virtue of being what it is! In so far as the configuration means a definite factor in a situation, like geometric form, or goal-insight, it can be tested. Experiments designed to determine whether the report of the presence or absence of phenomenal configurations is accompanied with a report of presence or absence of certain factors like brightness, color-contrast, are now under way at Illinois. Only on the basis of such correlations will the certainty grow of the effectiveness of the *Gestalt.*

It will be seen that this difference in emphasis leads to a very different position with respect to opposing theories. If the ideal case or the most useful point of view is taken to picture the "real" state of affairs then all other theories are wrong. I would prefer to say that some theories are badly stated rather than that they are wrong; that some descriptions and explanations are more precise, open to observational tests, or suggest more fruitful problems, than to say that some are "good" and others are "bad"—except as these adjectives embody only pragmatic considerations. Often, however, such terms are taken to imply absolute verities and facts which are not yet settled. Thus Koffka: "gute Phänomene sind gestaltete Phänomene, von ihnen unterscheiden sich die blossen 'Und-Verbindungen' und das Chaotische."[29] To which I would reply: "I agree that configurational phenomena and the value of the configurational concept is greater than that of the sensation and many other concepts; but reality may be viewed in an endless number of ways." Furthermore, reality presents us with both kinds of phenomena, unorganized as well as organized, and our task is to explain all with the best means at our disposal.

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[28] W. Köhler, *Die physischen Gestalten,* 1920.

(2) The second question asked above concerns the best description and explanation of observed data. The usual scientific procedure here is to find or postulate some entities that explain all phenomena of a given class or kind. The ordinary objects of perception, in physics as well as psychology, fail to satisfy the requirements of science for purposes of description and explanation, because they lack the simplicity, permanence and generality necessary for scientific formulations. "But the characters of their mutual relations disclose further permanences recognizable in events and among these are the scientific objects"[30] entering into the laws and theories of science. Descriptions and explanations in terms of relations are therefore to be preferred to those in terms of qualities. Terms like "good," "bad," "complete," and "simple," so often found in the configurational writings, are still in the adjectival stage lacking the precision and univocalness of scientific terms. Sometimes they seem to refer to aesthetic aspects of the object perceived; sometimes they seem to be a matter of geometrical form, and again the meaning may be logical, psychological or a matter for physics.

Köhler has objected to an explanation of the *Gestalt* in terms of "parts" and "relations" for a number of reasons, chief of which seem to be the following: (*1*) the relations and parts are not perceived as such; (*2*) the relations may change while the configuration is constant; (*3*) parts and relations furnish only

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[29] Koffka, *op. cit.*, 394.

[30] A. N. Whitehead, *The Concept of Nature*, 1920, 187.

"rules" not explanations of *Gestalten.*[31] While there is a great deal of truth in these arguments, it does not seem to me that they furnish an answer to the problems involved. Let us examine each in turn.

The keen desire of the configurationists to account for and deal only with the phenomenologically given, *von oben nach unten,* and to establish the existence and potency of the *Gestalt* have led to a hatred of abstraction and unobservables (of a certain kind) almost Berkeleyan in intensity. No one for a moment would hold that the observed entities and relations are and must always be the ones used in explanation. In fact the configurationists themselves speculate on the unobserved events in the nervous system in order to account for the observed facts! It is therefore not a matter of rejecting all entities and relations as explanations of the *Gestalt* but rather those (traditional) which do not enable us to account adequately for the given facts. The form of combination must not be confused with the formula of combination. The latter may explain the former and yet bear no more resemblance to it than the equation $y^2=4px$ looks like the parabolic curve which it determines. What is lost in concrete richness and variety is gained in definiteness, simplicity and generality by a statement in terms of fundamental relations. Not all parts and all relations will describe and explain all totalities; but some parts and some relations must certainly do it, else the possibility of formulating laws would vanish.

If we would solve the problem of relations we must distinguish between what is to be explained and the logic of the explanation. The mistake of the traditional abstractions lay not so much in their abstract character as in their sterility. Some abstractions are valuable and others are not. Abstractions become useless impedimenta when they allow fictions to creep into science, when they are not of such a nature that we can test their concrete implications by experiment, and when a different set of abstracta approximates the facts better. The configurationists are right in denying that new elements and relations will help the old analytic psychology to explain complexes; we must begin with a new kind of analysis, namely that which endeavors to account for the given facts without first reducing to *a priori* categories. The final result, however, will be an abstraction in some sense of the word. Whitehead has stated the logic of the matter most clearly: "Thus scientific objects are the concrete causal characters, though we arrive at them by a route of apprehension which is a process of abstraction., In the same way, what, in the form of a sense-object, is concrete for our awareness, is abstract in its character of a complex of relations between scientific objects. Thus what is concrete as causal is abstract in its derivation from the apparent, and what is concrete as apparent is abstract in its derivation from the causal."[32]

We may now examine the third argument, since it is implied by the first. It asserts that the parts and relations can give us "rules" of *Gestalten* but not explanations. It rests, I believe, although Köhler does not make himself altogether clear on this point, upon the fact that the configuration contains more than a "sum" of parts, is more than the parts and relations in addition. We shall discover the essential truth of this proposition in a moment, but it is not altogether true. It must be remembered that not all relations are additive. Only the simplest functions express additive relations and these are the exception rather than the rule. On this point the configurationists have warned us not to over-simplify, nothing more. But in so far as they assert another proposition in this argument their logic is open to criticism. We may state their case briefly as follows: the whole is more than a sum of its parts; the whole implies the parts but the parts do not imply the whole. Stated logically: A is said to be a part of B when the existence of B implies the existence of A; but the existence of A does not imply the existence of B.[33] But one-sided implication does not establish either logical or existential priority. Thus 'A is red' implies that A is colored, but 'colored' does not imply redness; yet colored and red are equally simple. It does not follow therefore that the 'whole' is given first either logically or materially. It all depends upon what parts and what relations are used in the explanation of wholes. What will be given first in experience is to be determined by experiment, since the simplicity or priority of the whole cannot be established by logic. The experimental evidence for the existential priority of the *Gestalt* is at present conflicting, as we shall see later.

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[31] A discussion of these points bearing directly upon the psychological problems involved will be found in my first article cited in Note 1. See W. Köhler: The Problem of Form in Perception, *Brit. J. Psych.*, 14, 1924, 262.

[32] Whitehead, *ibid.*, 188-189.

We must distinguish, as the configurationists often do not, wholes which are multiplicities (atomic structures) and wholes which are organic in nature (*Gestalten*). Köhler[34] points out that the treatment of atomic systems is simple and offers no (?) problems to the scientist. He does not state that it is generally the aim of science to reduce organic systems wherever possible to atomic systems because the investigation of the former is extremely difficult. The important consideration therefore must be in the *kind* of reduction that is made. Atomic structures are completely specified when their parts and some simple relation are named since there is no connection between the parts *inter se*. Organic structures are not completely specified when their parts are known because such wholes contain what we may call "qualifying" relations. We must be clear about the difficulty of explaining these wholes, for configurational discussions about the super-summative properties of wholes leave us somewhat unsettled and mystified when they should enlighten us. It is true that when the parts and relations are different from the whole, taking the parts and relations and difference does not give us the way in which the whole is different from the parts and relations. The explanation of this fact "seems to be that a relation is one thing when it relates and another when it is merely enumerated as a term in a collection."[35]

We see that the terms which form the explanation of a given event need not and should not be identical with the event itself. The explanation consists simply of a statement of the relations, or necessary and sufficient conditions, responsible for the observed fact. The further fact that certain relations (the "internal" relations) alter their relata leads into the problem of emergent evolution which may be left to the philosopher. The task of science generally consists in discovering and formulating the laws under which these emergents may be expected to appear. Analysis is merely for the purpose of stating these laws, and is largely logical in character. The error of the traditional schools has been fostered by the belief that a mere enumeration of parts and relations could be equated to the perception of organic wholes. Logic can never give us reality itself, whatever that may be. The conceptual side of science gives us a certain kind of truth which must be taken only for what it is. Thus the fact that silver chloride possesses properties over and above its constituents does not prevent us from saying that silver and chlorine are its constituents and that they combine, under certain conditions, to form a new substance. The error would lie in attributing the properties of silver chloride to its constituents, *i.e.* identifying two different things instead of correlating them with a statement of conditions. The parts and relations emerge in the attempt to formulate laws and not as elements out of which a logical synthesis will yield reality.

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[33] *Cf.*, B. A. W. Russell, *The Principles of Mathematics*, 1903, 137 ff. I have tried to avoid going into the logic and philosophy of the problems of whole and part, part and whole, new emergents, and integration. Here again we must make assumptions and go to work even though there are genuine problems remaining to be settled by philosophers. The configurationists have contributed abundant experimental material toward a solution of some of these problems, but sometimes their theoretical discussions verge on almost obscure interpretations.

[34] *Op. cit.*

[35] Russell, *ibid.*

The second argument against relations concerns the fact that some relations and parts may change while the configuration remains the same. Köhler seems to rest content with a proof about *some* relations as if it applied to *all*. It is true that a gray which is the lighter in one situation may be the darker in another, the relations of the given gray having changed. The strength of the argument lies in the use made of a single element which is kept constant in a different situation, hence the relations must change. But if we consider more general relations of both situations in which the given gray appears, and it is possible to get relations of any degree, then the invariant relations will be those expressed by the phrase "darker than" or "lighter than." These relations are just as independent of the elements in the situation as the configuration is. Furthermore, we must not confuse what the ape sees with the entities used in explanation. Here again the parts and relations used in explanation may or may not be the same as those perceived. We must not confuse the terms enumerated in an explanation with the fact to be explained. Whether we described the ape's behavior in terms of reaction to the configuration or invariant relations depends upon extraneous considerations. In either case we have to make certain assumptions. It happens that Bühler's and Jaensch's views of the animal's awareness of relations involve many difficulties which are avoided in Köhler's description, provided we accept his hypothesis of the *Gestalt*. At present that seems to be the simplest and most fruitful concept, but it too involves certain ambiguities which we must discuss in the next section. This brings us to the difficulties centering about the narrower concept of the *Gestalt*.

(29) *Five Questions Concerning Configurational Assumptions*

In taking the *Gestalt* as a primitive idea or datum which must be assumed in description and explanation the configurationists have left many questions unanswered which require further elucidation. The complexity of many of the phenomena described and explained as configurational effects prevents one from being entirely satisfied with such a designation. We have already found that some kind of analysis or resolution of configurational phenomena is not logically precluded by the nature of the configuration and its properties. We now have to show why such resolution is necessary if we are to rescue the concept from the obscurity which veils it. The ambiguities surrounding the concept can best be discussed in the light of the following questions: (1) What precisely is the configuration or *Gestalt* in any given case of perception, behavior, thought or feeling? (2) How does the *Gestalt* function to affect its members? (3) Is the configuration primary in the sense of being given first in experience or is it merely logically primary? (We have disposed of the second part of this question in the preceding section.) (4) Is there a constant relation between physical configuration and phenomenal pattern, or, in other words, can we control and predict by means of the configuration alone? (5) What are the effects of attention, past experience, set and the like upon configurational phenomena? The answers already given to these questions and the problems which remain may reveal some of the difficulties with the concept of the *Gestalt* as it has appeared in the theoretical and experimental contributions of the configurationists.

(1) The definition of the *Gestalt* as a transposable whole which possesses properties not derivable from its parts does not enable us to say that the configurational factor in a given case is the shape, form, pattern, qualitative structure, geometric configuration, general features or togetherness of the "parts." Köhler says that homogeneous fields are not configurations because they do not possess any structural properties, *e. g.* halving a homogeneous gray surface leaves the parts unchanged, whereas altering one part of a configuration affects all the rest. But Wertheimer and Koffka speak of all phenomena as configurational, in some sense or other, since "das gegebene ist an sich, in verschiedenem Grade 'gestaltet': gegeben sind mehr oder weniger durchstrukturierte, mehr oder weniger bestimmte Ganze und Ganzprozesse."[36] If all phenomena are configurations and structuration is but a matter of more or less, what becomes the differentiating factor within a given field? Here we are thrown back upon the first problem again: is the configurational factor shape, form, extent, direction, curvature, contour?

The difficulty of fixing precisely upon such factors as form, shape and pattern arose in this country as early as 1912 when Bingham pointed out that Katz and Révész were not warranted in concluding that the chicks reacted specifically to the form element in the environment.[37] Hunter asserted that Bingham had not distinguished between form and pattern, pattern being for Hunter a figure-ground phenomenon. Bingham replied that form and shape have to be kept apart because one may be constant while the other changes! There are genuine problems here which are obscured by the configurationists when they fail to make clear precisely what the *Gestalt* is in any given case.

(2) The second question concerns the conditions under which a configuration actually becomes effective. I have already

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[36] M. Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psych. Forsch.*, 1, 1922, 52; K. Koffka, *op. cit.*, and Introspection and the Method of Psychology, *Brit. J. Psych.*, 15, 1924, 149.

[37] H. C. Bingham, Size and Form Perception in *Gallus domesticus*, *J. An. Behav.*, 3, 1913, 109, and A Definition of Form, *ibid.*, 4, 1914, 136; W. S. Hunter, The Question of Form Perception, *ibid.*, 3, 1913, 329.

pointed out that merely to ascribe a given effect to the configuration means that we have failed to find the real causal factor in a situation. It is necessary here to test the configuration in the same way that any concept is tested: for example, if a phenomenal effect is ascribed to the configuration the observer must be asked if the configuration is present phenomenally. The experimentation with amblyopic and hemianopsic observers seems to show that while the perception of forms does condition the perception of qualities (red, green) to some extent nevertheless the power of perceiving forms may be lost while certain other properties of objects are seen.[38] The mechanisms for all of these various factors cannot therefore be identical, although they are related. We have then to determine the relation between the mechanisms responsible for form and the other aspects of experience.

There are several further difficulties with the causation of the configuration. They are mainly concerned with the relations existing between the physical and phenomenal *Gestalten.* The transition from physical to phenomenal field patterns cannot be made as easily as appears at first sight, for as soon as the simplest cases are disposed of by means of a copy theory various complicating factors destroy the simplicity of the scheme upon which so much of the physiological theorizing of Köhler rests. Köhler's accomplishments in the field of physiological psychology can hardly be overestimated so far as his straightforward handling of certain problems and the fruitfulness of that kind of approach are concerned. Yet I find some difficulty in accounting for several facts on the basis of certain admissions made by the configurationists and certain experimental findings bearing upon this topic.

We know, first, that phenomenal and physical configurations do not always maintain a one-to-one relationship, especially when we have so-called illusions, ambiguous figure-ground fields and unordered physical collections which may be structured in a variety of ways.[39] Somewhere between the physical stimuli and the psychophysical *niveau* events are structured, now in one way, now in another; or they may be disintegrated under the influence of "attention," habit and attitude. The mere assumption that phenomenal fields possess properties parallel to physical configurations does not help us because there is no agreement between what is perceived and the physical aggregate. In his 1913 paper Köhler insisted upon the difference between physical and phenomenal data, saying we must admit that they are different in order to avoid the constancy hypothesis.[40] I take it that this applies to patterns as well as isolated aspects of the stimulus and the sensations to which they were formerly supposed to give rise. In *Physische Gestalten* (p. 173) the legend becomes "*Denn was innen, das ist aussen.*" But in the case of phi-phenomenon and under conditions when stimuli and experience do not agree, Wertheimer has said that the phenomenal pattern cannot be compounded out of either physical or psychological units.[41] Somewhere events become transmuted and Köhler and Wertheimer have suggested certain laws supposed to suggest a common basis for changes in the physical, physiological and psychological realms.[42] The difficulty in applying these laws or criteria is two-fold: they do not help us in complicated cases—especially where the determining factors lie within the organism; and they mean different things—accordingly as they are used in physics, psychology or logic. The laws of simplicity, precision and stability may not be the same for both physical and phenomenal structures; what is logically coherent or simple may or may not imply physical and psychological coherence and simplicity. The bridges from physics to psychology and logic require many props before they will bear heavy loads.

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[38] References to Gelb and Goldstein and Poppelreuter are given in notes 54 and 55.

[39] Configurational anomalies have been emphasized by V. Benussi and form the basis of his production theory. *Cf.*, Gesetze der inadäquaten Gestaltauffassung, *Arch. f. d. ges. Psych.*, 32, 1914, 396.

Unquestionably any given configuration, whether it be a physical, physiological, logical or psychological structure must be the result of special conditions which must be determined in every case. We have no guarantee in advance of experience that a minimal distribution of energy in the physical system will be followed by similar distributions of energy in the nervous system. Some time must elapse before both would be in a state of equilibrium and during that time the factors within the organism may upset the direct connection between stimulus-configuration and phenomenal pattern or motor response. The geometrically simple figures do not always exercise the simplest configurational effects. Sometimes behavior is determined by logical demands in Wertheimer's sense; at other times it may be governed by the physical conditions which an environment may impose upon an organism (a living man after he has left the edge of the precipice from which he has fallen, is as good as a dead man so far as the curve is concerned which he will describe in his descent to the bottom!), finally, factors like memory, imagination or feeling may be operative to influence behavior. The concept of the configuration as such does not indicate the particular set of necessary and sufficient conditions governing particular events which we have to explain. The *Gestalt* may be a necessary but not a sufficient condition of an event or it may be sufficient but not necessary.

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[40] W. Köhler, Über unbemerkte Empfindungen und Urteilstäuschungen, Z. *f. Psych.*, 66, 1913, 51 ff.

[41] One of the difficulties with the Wertheimer short-circuit theory, which is supposed to be a configurational theory, is that it must deal with isolated stimulus units which have then to be transformed into unitary physiological processes in order for phenomenal unities to appear. See his Experimentelle Studien über das Sehen von Bewegung, Z. *f. Psych.*, 61, 1912, 161 ff.

[42] Köhler, *op. cit.*; Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psych. Forsch.*, 4, 1923, 301.

I have stated that Köhler advocates a copy theory of the relation between physical and phenomenal fields. Probably this is not true, except for the very highest phases of the neuro-psychophysical process where phenomenal fields are said to possess properties parallel with the physiological processes. But the phenomenal field is part of the total psychophysical configuration and exerts its effect along with the other parts upon the whole. The phenomenal process is therefore not a mere copy of physical structures. It may influence the total configuration and must be taken into account. This fact works against the assumption of one-to-one correlation between the physical and perceptual fields or the possibility of predicting from the stimulus-pattern what the response-pattern will be. For this reason one often feels that the configurationists fly too readily to the unobserved nervous system for explanations of phenomena which might yield to further scrutiny. Take the problem of successive comparisons: because several theories involving psychological processes have foundered in the attempt to explain why equal stimuli presented after different temporal intervals appear unequal, Köhler offers several physiological hypotheses without taking into account the fact that the physically "blank" interval may possess just as positive phenomenal characteristics as the filled times. Thus Werner[43] has shown that when a series of lights was flashed quickly the observers made their judgments upon the lights, but when given slowly, upon the pauses between. The same held true for auditory stimuli.[44] Werner has also shown that the ground possesses, in many figure-ground structures, just as positive a character as the figure, whereas Rubin ascribes negative character to the ground. On the basis of the stimulus-structure alone or the supposed physiological processes, we could never discover the positive character of a phenomenal field when the physical field was empty. In the matter of physiological theory the configurationists have too often departed from their avowed intention of sticking to observed entities. Where such theory leads to further problems and experimentation no one can question its value; where it neglects the factors within the phenomenal or behavior patterns it is inadequate; where it obscures the differences between the phenomenal and physical fields it sets up a new constancy hypothesis just as untrue as the old.

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[43] H. Werner, Über optischen Rhythmik, *Arch. f. d. ges. Psych.*, 38, 1919, 115.

[44] Werner, Rhythmik, eine mehrwertige Gestaltenverkettung, *Z. f. Psych.* 82, 1919, 198; Benussi, Zur experimentellen Analyse der Zeitvergleichs, *Arch. f. d. ges. Psych.*, 9, 1907, 366. Benussi found that in short intervals the noises were attended to, while in long intervals the time between was noted with the result that in short intervals the noises were heard better (not because of attention alone, however). The asymmetry of successive comparisons may finally yield to a theory based on the phenomenal facts after all.

(3) The third question has been answered in so many different ways, both by those who admit the configurational assumptions and those who do not, that it is impossible to discuss them at length. More discouraging still is the fact that the experimental evidence may be interpretated to favor any theory. What are the first data in perception? To what do the animal, the child and the pathological observer react? If the facts could be settled it would be easy to settle the logic. We have already shown that the logical priority of the configuration is by no means implied by the fact that a part does not imply the whole: two things may be equally simple even though one may imply or not imply the other. Hence to assert that the configuration must be taken as the fundamental assumption with which we begin to describe and explain in psychology does not rest upon a secure logical foundation. Let us see what foundation in *fact* the primacy of the *Gestalt* has. We shall have to consider evidence from animal behavior, normal adult psychology and reports from pathological observers suffering from various defects in perception and action.

Reverting to the experiments made by Bingham and Lashley in this country, we may say that animals do not react unmistakably to the form element in a situation. Bingham[45] found that his chicks responded first to the factor of size and next to that of general illumination, both of which were of almost total importance to the exclusion of form. Lashley concluded from his experiments that "the rat perceives brightness differences readily. Absolute size is not so easily recognized, but greatly different areas are distinguished without difficulty. The perception and recognition of form seem to be most difficult for the animal. . . . In these respects the rat's visual perceptions resemble closely human perception in the extreme visual field."[46] Köhler's own experiments show that the upper limit of the ape's performance was reached as soon as the configuration attained any degree of complexity. Indeed, Köhler states very explicitly that it is impossible to give priority to absolute or stepwise reactions.[47] A number of very different problems are hidden here which are concealed by reducing them all to configurational terms. If the configuration means form proper, then we can say very definitely that animals perceive sizes and qualities before they do forms. If the primitive configurations are taken to mean figure-ground structures, as Koffka[48] seems to imply that they are, then the problem of genesis and development remains to be settled. If a configurational response means that an animal can react to a part of a situation in the light of the whole, as Köhler sometimes expresses it, then we have a wholly different set of questions to consider. A concept which gives apparent unity by obliterating differences is in need of analysis.

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[45] H. C. Bingham, *opp. citt.*

[46] K. S. Lashley, Visual Discrimination of Size and Form in the Albino Rat, *J. An. Behav.*, 2, 1912, 329.

What are the facts from normal observers? Experiments upon the perception of groups show that either totalities or parts may be seen first. The differences among observers are striking. The observers fall readily into two opposed groups which many investigators have described as the analytic and synthetic types. One group tends to perceive and react to parts as such, requiring time in which to find the whole; the other group tends to perceive wholes and requires time to discover the parts. Either attitude may be "natural," but if one predominates in a person the other will be "unnatural." Thus Benussi[49] found that the time required by some observers to perceive an illusion of the Müller-Lyer type (which depends upon a perception of the whole primarily) was between 82–100 σ, while for other observers the time was between 1000–1600 σ. The one group saw parts when the time was short, the other saw wholes when the time was short. Benussi and Seifert[50] regard the synthetizing (*gestaltbildenden*) attitude and the analytic (*abstrahierenden*) attitude as mutually complementary. Kenkel and Wulf,[51] working with Koffka, have noted the important part played by the analytic attitude in the perception of movement, recognition and memory. Müller, Rubin, Wertheimer, Werner,

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[47] W. Köhler, Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, *Abh. d. könig. Preuss. Akad. d. Wiss.*, 1918, *Phys. Math. Klasse, Nr.* 2, 38.

[48] K. Koffka, *The Growth of the Mind*, 1925.

[49] V. Benussi, Versuche zur Bestimmung der Gestaltzeit, VI. *Kong. f. exp. Psych.*, Göttingen, 1914.

[50] V. Benussi, *opp. citt.*; F. Seifert, Zur Psychologie der Abstraktion und Gestaltauffassung, *Z. f. Psych.*, 78, 1917, 55.

[51] F. Kenkel, Untersuchungen über den Zusammenhang zwischen Erscheinungsgrösse und Erscheinungsbewegung, etc., in Koffka's *Beiträge zur Psychologie der Gestalt*, 1919; F. Wulf, Über die Veränderung von Vorstellungen, *Psych. Forsch.*, 1, 1922, 333.

Westphal[52] and a host of others have also described the differences resulting from the two attitudes. From a survey of all the evidence I cannot agree with Koffka that one is more natural than the other. The emergence of new properties in the whole in synthetic perception is accompanied by a loss in individuality of the parts. What is gained in one way is lost in another. Both attitudes require a "set" or organic readiness which has yet to be explained.

The configurationists have made the integrative functions their main concern, stressing those conditions which favor the perception and reaction to wholes. In doing this they have neglected the discriminatory responses and perceptions. The facts show that neither analytic nor synthetic aspects can be completely separated from the other. Which will predominate depends upon *Aufgabe*, interest, and the result to be attained. While it is true that some structures can best be comprehended as wholes, it is also a fact of no less importance that parts be abstracted and considered *per se*. The child which has not reached the age of discrimination calls every man "papa" and old people who are forever noticing similarities fail to see the new details experience presents. Insight demands analysis as well as synthesis. There are different kinds of analyses and part-activities which cannot be discussed here. A few illustrations may, however, make certain aspects of the problem clearer.

Theories based upon elements find difficulty in explaining how the form or totality can be preserved when the parts are different. The correct form of mathematical procedure may be followed even though every number in the solution is incorrect. But on the other hand, it is equally true that any given configuration with its formal properties requires certain parts and relations, otherwise it disappears. In Meinong's terminology, a given *Superius* requires certain *Inferiora;* even though the latter may be changed. Were the constituents of the *Gestalt* placed awry there would be no *Gestalt*. For this reason the skilled performer practises each part-activity necessary for the perfection of the whole. One cannot play or sing beautiful melodies if the notes are slightly out of tune; the artist must learn to mix colors before he can paint the unities we behold in his pictures. And even such a process as recognizing, which is mainly dependent upon the reappearance of similar forms, requires the finer discriminations that come only with an analytically *gerichteter Einstellung*. Take my recognition of my violin. In shape it is like many other violins I have seen. Were recognition wholly dependent upon the form, I could never pick it out from a number of others. Only by minute differences in shading, coloration and minor nuances of various sorts can I be sure that it is mine. Nor are these minute differences, strictly speaking, "smaller structures," for I apprehend them by functions which are entirely opposed to configurational perceptions. Indeed, the enjoyment and comprehension of wholes may depend most decidedly upon the perception of each and every part. People with a very good sense of pitch both enjoy and dislike music more than others with poorer powers of pitch discrimination accordingly as each note is played or sung in tune or out of tune.

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[52] G. E. Müller, *Komplextheorie und Gestalttheorie*, 1923; E. Rubin, *Visuell wahrgenommene Figuren*, 1921; Wertheimer, Werner, *opp. citt.*; E. Westphal, Ueber Haupt- und Nebenaufgaben bei Reaktionsversuchen, *Arch. f. d. ges. Psych.*, 21, 1911, 221.

If the parts reproduced the whole in the manner suggested by Wertheimer and Koffka, life would be an unending monotony and invention would be impossible. Creative insight depends as much upon breaking up configurations as in their preservation. The configurationists have over-emphasized the totality as much as the associationists underestimated it. Ribot[53] has pointed out that total redintegration can be a hindrance as well as a help. The person who can recite twenty pages of poetry but cannot single out a particular passage without going through the whole is as badly off as the one who has learned by piecemeal and forgets the whole. The organism is obliged to select portions of its environment to which it will react specifically and this necessitates rejection as well as acceptance of total situations. Discriminating, differentiating and analyzing have their functions and laws as well as comparing, integrating and synthetizing.

The evidence from pathological observers confirms the results with normal observers. Gelb and Goldstein's patient,[54] suffering from brain injuries which resulted in the loss of configurational functions, was able to build up forms only by means of head movements. Figures seen in direct vision were neither round, square, nor of any other describable form, yet he could see the color. Even such elementary forms as straight and crooked could not be distinguished. Poppelreuter,[55] working with different patients, found approximately the same results; the perception of form does not appear until very late. Simpler presentations seem to precede the actual configurational perceptions in some such order as the following: phenomenal fields may be present without boundaries, directions, contours or limits; then single factors within the field appear like direction, angles, or circularity; lastly, configurations proper emerge. The mechanisms responsible for the "higher" processes may be destroyed while the "lower" remain more or less intact. Unless we use the word *Gestalt* to cover everything, there is no conceivable sense in which it can be used to denote the primary data in experience.

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[53] Th. Ribot, *Essay on the Creative Imagination,* tr. by A. H. N. Baron, 1906.

[54] A. Gelb and K. Goldstein, Psychologische Analysen hirnpathologischer Fälle, *Zur Psychologie des optischen Wahrnehmungs- und Erkennungsvorganges* 1920.

[55] W. Poppelreuter, Zur Psychologie der optischen Wahrnehnung, *Z. f. d. ges. Neurol. und Psychiatrie,* 83, 1923, 26.

What, then, is to be the "primitive idea" or assumed datum for psychology? Something, to be sure, must be assumed in any description or explanation. The configuration seems to cover too many different phenomena to be taken as the fundamental postulate behind which we need not inquire. Paraphrasing a sentence from William James we may say: "Who calls a thing a first presentation admits he has no theory of its production." We must account for the facts covered by the concept of the *Gestalt*. The choice of primitive ideas, descriptive constants and fundamental postulates, I repeat, are pragmatic matters, matters of simplicity, economy and deductive value, but they must be univocal in meaning within any given system.

(4) The question regarding the constancy of the configuration has already been answered in several different ways; but it must here be broached again in order to introduce the fifth question asked above regarding the old concepts which the configurationists hope to supplant by means of the configuration. One of the main contributions of the configurationists has been to show that the relation between stimulus and sensation is not constant. Simple aspects of the stimulus cannot be correlated with simple bits of experience. Köhler[56] has called the assumption underlying the supposed stimulus-experience relation the "constancy hypothesis." But this is an unfortunate designation. Any uniformity must rest upon a constancy assumption, otherwise the laws and theories of science would have no value at all. Köhler himself has resorted to a constancy hypothesis in asserting that phenomenal fields copy the spatial structures of physical fields. To assume that the physical aggregate has a form like the phenomenal pattern involves the old view for large "chunks" of experience instead of for small ones. It is often as difficult to say what corresponds to the phenomenal pattern as it was to isolate that portion of the environment which produced the sensation. Thus :: may be seen as a square, two vertical lines, two parallel horizontal lines, or a number of other configurations. Which pattern will predominate in perception necessitates the introduction of some of the older concepts which the configurationists theoretically reject.

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[56] W. Köhler, *op. cit.*, *cf.*, note 40 *supra.*

(5) When configurational uniformities fail to appear the configurationists have resorted to the use of historical concepts which still remain to be elucidated: past experience, *gesamter psychischer Habitus*, *Einstellung*, attention and the like. We must remember that the so-called configurational laws hold only so long as all of the conditions comprehended under attention, set, and attitude favor the configurational phenomena. It is to be hoped that configurational factors may throw some light upon these obscure terms when the configurationists re-introduce them.

The configurationists have in many cases succeeded in replacing many of the old terms by more fruitful ones. In the light of their work we can approach problems in attention, attitude, association, conditioned reflex with fresh insight. But they have not succeeded in explaining them away nor in banishing them from psychology, much as we should all like to see them go in favor of better categories. Koffka's re-statement of the law of association by similarity adds little to our knowledge of the mechanisms operating to make past experience effective in the present, unless we regard his formulation in the light of the whole doctrine of the *Gestalt*; then its value is, as was pointed out above, chiefly methodological. Sometimes the same *Einstellung* may produce different configurational phenomena; attention may integrate or disintegrate; it may aid the phi-phenomenon and destroy other configurations. We find in Kenkel, Korte, Hartmann, Fuchs, and other members of the school, a frank admission of these intra-organic factors which remain to be explained.

One of the main sources of difficulty in present-day psychology is in the diverse terminologies which abound for the same facts. Intervalent descriptions can be found in other sciences as well, but they do not multiply so quickly. Misunderstandings are often due to the inability of a member of one school to understand the language of a member of another school. Thus Fuchs[57] re-interprets Katz's facts in the light of *Gestalt*: "Es lassen sich noch eine Reihe andere bei Katz beschriebenen Erscheinungen auf die Wirksamkeit des Gestaltfaktors zurückführen, wenn Katz es auch nicht ausspricht." Sometimes a different terminology or re-interpretation suggests new problems and opens up new fields; at other times it is merely a case of pouring old wine into new bottles. The choice of terms must therefore remain pragmatic. But new terms must not blind us to old problems nor should old concepts prevent us from seeing new problems!

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[57] W. Fuchs, Experimentelle Untersuchungen über die Aenderung von Farben unter dem Einfluss von Gestalten, *Z. f. Psych.*, 92, 1923, 311.

(30) *Conclusions*

Perhaps the chief advantage which the work of the configurationists enjoys lies in the fact that it is designed to illustrate and support an all-embracing theory. We see this at its best when the single, often uninteresting, fact becomes fraught with new significance as it is brought into relation with *Gestalttheorie.* For many people fact unsupported by theory and unproductive of theoretical consequences is valueless and dead. On the other hand, it is equally true that a theory which does not suggest new problems and answers, which does not help us to unify the facts already known and to bridge the gaps in our current knowledge, implying further propositions which can be tested by observational methods, has passed the stage of scientific usefulness and should be discarded for one more fruitful of results. We may even venture further to say that any psychology which neglects certain classes of facts, either because it cannot deal with them or because it turns attention to some very special set of interests, will fall eventually through the weaknesses engendered by its own limitations. For science is continually expanding its field of conquest to include more and more of the world of experience within the ordered world of exact knowledge. Science requires a certain amount of *Spielraum* for its efforts, a certain number of fluid concepts to point the way. The theory of the *Gestalt* has helped widen the borders of psychology so as to make possible a more fruitful attack on meaning, mind and behavior. It has indicated where different points of view and some apparently contradictory statements of fact may be brought into agreement. It suggests further experimentation to bridge the clefts in our science.

Yet withal one feels that the concept of *Gestalt*, to be of the most service, should be more precisely defined and limited in order that we may be able to know exactly what it is, how it affects a given problem, and where the causal factors are to be found. For some writers everything is *Gestalt* (Linke, Köhler); for other writers there are different grades of structuration (Koffka, Wertheimer); still others maintain that the *Gestalt* appears late in the individual and the race (Benussi, Poppelreuter, Jaensch). To apply the term to all experience and behavior, saying that everything is more or less structured, leaves the situation as it was before, since we need other classificatory and explanatory terms for the differences we find.

A single concept for all psychology is both a strength and a weakness. While it is true that we are striving for unity there are still many stubborn facts which refuse at present to be ordered within any set scheme. The work of the past and the configurational experimentation itself have revealed large classes of facts of apparently diverse orders. Ideally, we should logically be able to order facts under a single set of postulates or a single principle from which it would be possible to predict all others. But nature is not amenable to logic, not at least until our science has advanced far beyond its present stage of development. We have to use whatever concepts fit the facts best, even though it is at the expense of a unified system of psychology. Eventually we may hope to erect some universal postulates which shall do for psychology what relativity and mathematics have done for physics, but not until the various sub-classes of psychological phenomena and the different kinds of psychology have been more fully investigated and brought into closer relations with one another.

To what class or order of facts, then, should the concept of the *Gestalt* be limited? There have been a number of suggestions, but since all of them involve fundamental assumptions which do not harmonize with the configurationists' theories it is impossible to discuss them here without opening up once more the whole question of methodology and subject-matter. If we could apply the configurational methods to the investigation of our data in some such fashion as was pointed out above and at the same time employ whatever concepts seem best in any particular field, the problem might approach solution. In general this implies a systematic variation of conditions affecting the datum studied with a view toward accounting for it rather than reducing it to some set of preconceived entities and relations which alone are thought to be proper for psychological description and explanation.

My criticism of the doctrine of the *Gestalt* by no means implies that we should discard it. On the contrary, I hope that it may result in continued investigation of the class of facts which the configurationists have stressed. In opening up new domains and giving us a point of view which enables us to approach them more fruitfully the configurationists have given experimental psychology proper a new lease of life. The problems centering about the *Gestalt* may well take the efforts of a generation of psychologists if they are to clarify the concept, trace out its implications, account for its effects, and reduce it to something more general and fundamental.

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RECOGNITION OF CHINESE SYMBOLS[1]

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This Study was begun in the Psychological Laboratories of the University of Michigan in the Fall of 1917, carried forward to June 1918, and continued through the academic year 1923-24. The object of this work was to study the use of Chinese symbols for recognitive purposes. Chinese symbols appear to offer exceptional material for the study of mental processes, such as recognition and recall. Indeed, some workers believe that the Chinese characters, because of their symbolism, offer a very valuable material for psychoanalysis.[2] While the symbols chosen were not as carefully selected in regard to their relation to one another and to their basic radical as might have been desired, they proved to be of value in this Study. The symbols possessed the virtue of being meaningless like nonsense-material for all but one *S*—a Chinese girl. With this single exception none of our *S*s had had any previous experience with them.

Materials. The Chinese language is made up of characters, every one of which may represent a single word or several words of quite different meaning. The Chinese dictionary is not alphabetically arranged as in English because the language has not progressed to the syllabic or alphabetic stage but is arranged under radicals,—214 in number. These are classifiers of the component parts of every character. In the present Study the maximum symbols for any one *S* was 494 in the 1917-18 experiments, and 360 in the 1923-24 experiments.

A Chinese character may be made of up from 1 to 54 strokes. Every character is made up of parts of various meanings. It is interesting to note some of the combinations with the meaning given to them. For example: combination of the word "woman" and that for "to take" means "wife;" combination of the symbol for "woman" and "boy" means "good," that is a woman and son represent the greatest good known to man; "son" and "sin" give the symbol for "sorrow," that is a child committing a crime brings suffering to his parents.

Series used in the Experiment. Series I, II, III, IV and V were used in 1917-18 and were prepared for the use of the Chinese symbol alone. Every series was made up of 65 cards, 2¾ in. long and ¾ in. wide. The symbols, cut from a Chinese newspaper, were pasted in the center of the cards. After an exposure of the series, the cards were tested for recognition at intervals of 5 min., 20 min., 1 hr., 1 day, and 2 days; and in the order named. 13 old and 6 new symbols were shown at every test. The order of the appearance of the old and new was left to chance, consequently the test series was different for every *S*.

Series VI, VII and VIII were used in 1923-24. On the cards of these series Chinese characters were given with their English equivalents. The English appeared at the left, the Chinese at the right. The cards were 3½ in. by ¾ in. A space of 1½ in. was left between the center of the word and the symbol. The symbols, as before, were pasted upon the cards. This was on the whole satisfactory, but a blurring, a slight tear or an irregularity served at times as a cue for recognition, we therefore recommend that in future work the symbols be drawn. The English equivalents are typed on the cards. Because the symbols in the recognition tests were selected at random they were not of equal difficulty. The ratio for old symbols was: easy, 6; medium, 6; hard, 2; for the new: easy, 2; medium, 3; hard, 1. 14 old and 6 new symbols were given at every test. An 8 hr. interval was added to those used in the earlier experiments. The order of the cards in these test series was the same for all the *S*s.

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[1] From the Psychological Laboratory, University of Michigan.

[2] J. T. Sun, Symbolism in the Chinese written Language, *Psychoanalytic Rev.*, 10, 1923, 183-189.

Apparatus. The Wirth-Ach memory exposure apparatus was used in all the experiments. It was set in circuit with a Hipp chronoscope for the recording of the reaction times. In the experiments of 1917-18 a lip key was used to break the circuit, but as the lip key showed no marked advantage an ordinary telegraph key was used in the 1923-24 experiments.

Procedure. The *S*s were told that the experiment was one on recognition, that they would be shown a series of Chinese symbols, and that, after a stated interval, they would be shown a second series made up of a certain percent. of old and new. As soon as they had reached a decision they were directed to release the key, tell what their judgment was, and why and how they had reached it. In the experiments of 1917-18 they were allowed to report "old," "new," or "uncertain," but in the experiments of 1923-24 they were instructed to report only "old," or "new," and to give an estimate, in terms of percent, of the certainty of their judgments—25% if not at all certain, 50% if fairly certain, 75% if certain, and 100% if absolutely certain. Only rarely did the *S*s find themselves at a loss to give a positive report, thought they frequently said in their introspections that a 25% judgment was little better than a guess, that their judgments in those instances might just as well have been "old" as "new", or contrariwise.

A group of trained and of untrained *S*s served in the experiments. All were tested with the Chinese symbols. The untrained *S*s, in addition, were tested with the English equivalents. They were tested first with the Chinese symbols, and then, in exactly the same order, with the English equivalents.

No attempt was made to control the *S*s activities during the intervals between tests except to ask them to dismiss the symbols from thought. Introspections were taken at the end of every test series, and at the same time the *S*s were asked to comment on the series as a whole, whether they thought it was easy or difficult, whether they had a feeling of success or failure, and what methods of study they had used during the impression.

The majority of the tests were given in the late afternoon; usually on school days, and the reports on what had taken place during the experimental intervals showed nothing but the usual student activities. Occasionally fatigue periods or emotional upsets were reported as having occurred during the lapsed interval, but at the time of the test most of the *S*s reported that they resumed after seating themselves at the apparatus the attitude they had at the time of the original presentation.

RESULTS

The data reported in this paper were obtained from 25 *S*s as shown below:

Experiments of	*S*	No. symbols correctly recognized old	new	total
1917-18	5 (trained)	1287	594	1881
1923-24	6 (trained)	1176	504	1680
	13 (untrained)	1092	468	1560
	1 (Chinese)	84	36	120

Discussion of Results. We consider (1) the correct recognitions; (2) the reaction times; and (3) the introspections.

(1) *Correct recognitions; (a) Results for the various intervals and the different materials.* In the 1917-18 experiments the percent of "old" symbols correctly recognized as "old" was found to vary with the *S*, ranging from 15—100%. The averages for the group, however, show that the highest percent of correct recognitions was reported at the 20 min. interval, followed in descending order by the reports at the 1 hr., 5 min., 1 day and 2 day intervals. In the cases of the successful recognition of the "new" symbols as "new" the highest percent fell at the 20 min. interval, followed by the 5 min., 2 day, 1 hr., and 1 day intervals. With the "old" symbols uncertainty was shown by all *S*s generally at the longer intervals; whereas with the "new" uncertainty was felt by but 2 *S*s, and by them usually at the longer intervals.

In the 1923-24 experiments the trained *S*s gave the highest percent of correct recognitions of the "old" at the 1 day interval, followed in descending order by the 20 min., 8 hr., 2 day, 1 hr., and 5 min. intervals—very different results from those obtained in the earlier experiments. The results, however, may be due to the slight differences in technique, changes in the time intervals, or to individual differences. It is not safe to assume that the differences are due to the presence of the English equivalents on the exposure cards. In the "new" recognized as "new", the greatest percent of correct recognitions occurred at the 5 min. interval, followed in descending order by the 20 min., 1 hr., 1 day, 8 hr. and 2 day intervals.

The results of the untrained *S*s with the Chinese symbols showed the greatest percent of successes in recognizing "old" as "old" at the 20 min. interval, followed in descending order by the 2 day, 1 day, 5 min., 1 hr., and 8 hr. intervals. For the "new" as "new," the greatest percent of successes occurred at the 20 min. interval, followed by the 8 hr., 5 min., 1 day, 2 day and 1 hr. intervals.

The Table below gives a summary of these results:

Percent Of Successes

1917.18 experiments, 5 *S*s (trained), 1881 reports

Interval:	5 min.	20 min.	1 hr.	1 day	2 day
Old as "old"	72%	76%	74%	70%	68%
New as "new"	70%	75%	58%	54%	60%

1923-24 experiments, 6 *S*s (trained), 1680 reports

Interval:	5 min.	20 min.	1 hr.	8 hr.	1 day	2 day
Old as "old"	59.5%	62.8%	60.3%	61.8%	65.6%	60.8%
New as "new"	86.9%	84.5%	83.3%	76.2%	77.3%	72.6%

1923-24 experiments, 13 *S*s (untrained), 1560 reports

Interval:	5 min.	20 min.	1 hr.	8 hr.	1 day	2 day
Old as "old"	53.8%	58.7%	49.0%	48.3%	55.4%	56.6%
New as "new"	79.4%	83.5%	71.0%	82.0%	76.6%	74.3%

It will be seen from a comparison of these results that the 1917-18 experiments had a larger percent of successes with the "old" symbols than with the "new." This is thought to be due to the fact that the English equivalents were not shown in the earlier experiments and the *S*s in those experiments gave undivided attention to the Chinese symbols, while in the 1923-24 experiments the *S*s had to divide their attention between the symbols and the English equivalents. The "new" as "new" ran, in the 1917-18 experiments a little lower than the "old" as "old", while in the 1923-24 experiments all the *S*s were more successful with the "new" as "new." These results corroborate Strong's conclusion[3] that the ability to know what we have not seen is more strongly fixed than the ability to pick out what we have seen. When the English equivalents were shown the untrained *S*s, the longer intervals gave a greater percent of correct recognitions for the "old" as "old" than for the "new" as "new," as shown in the summary below:

13 *S*s (untrained) English equivalents

Interval:	5 min.	20 min.	1 hr.	8 hr.	1 day	2 day
Old as "old"	60.1%	62.9%	51.0%	51.6%	56.0%	55.6%
New as "new"	93.0%	74.2%	73.0%	67.6%	75.6%	65.6%

The untrained *S*s, when reporting on the English equivalents, were asked to draw the Chinese symbols. 10 out of 1092, or 0.9% of the symbols were correctly drawn. In addition to these 10, 1 was correctly drawn but not recognized as "old;" 90 were partially drawn; and 22 were described. Of those partially drawn, 45 were correct, 18 partially correct, and 27 wrong; of those described for which no drawings were made, 8 were correct and 14 wrong.

In this connection it is interesting to note the results of the Chinese *S*. Of 84 symbols shown her, she recalled correctly 52 of the English equivalents. She showed, however, a marked tendency to confuse the "new" English words with the "old." She did best in classing "new" Chinese symbols as "new," and next best with "old" chinese symbols as "old." In addition to the 52 words, she gave 21 words with equivalent meaning. This is not particularly surprising in her case as she came from southern China where every symbol is given 8 different meanings. When the *S* gave equivalent meaning *E* could not tell whether the report was a correct recognition or mere coincidence. It will be seen, then, that the problem is very different for the Chinese *S*. She correctly recognized 61.9% of the English equivalents as against 7.6% by the other *S*s, and drew 69% of the Chinese symbols as against 0.9% for the others *S*s. In order to rate a Chinese speaking or reading *S*, a different technique and method would have to be employed.

(*b*) *Localization.* A report of the series-position of the recognized symbols was not asked of the *S*s. A few, however, incidentally mentioned it. 23 such cases were given, and 9 or

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[3] E. K. Strong, The Effect of Length of Series upon Recognition Memory, *Psych. Rev.*, 19, 1912, 447-462.

39.1% were correct. 2 of the symbols occupying the first position and 7 occupying the last position were correctly placed.

(*c*) *Relation between actual and estimated success.* The *S*s were asked to estimate the success of their recognitions. Some gave their estimations in percent but most of the *S*s preferred the general expressions 'good', 'fair,' or 'poor.' Because of this, a definite correlation could not be worked out. A comparative study was, however, made in this way: if, in a series of 14 symbols, 4 or less were wrong, the report was scored as 'good'; if only 4 or less were correct it was scored as 'poor'; between these limits, scores were classed as 'fair.' The reports of the trained *S*s, classified according to this system, were 20% good, 76% fair, and 4% poor. Comparing the *S*s' estimation of their success with their actual success there was an over-estimation in 15% of the cases, a coincidence in 51%, and an under-estimation in 34%. The reports of the untrained *S*s on the English symbols were 4% good, 79% fair and 6% poor. No scores were obtained in 10% of the cases. In comparing the estimations with the actual results, 19% were over-estimated, 54% were coincident, and 14% were under-estimated. It will be seen in comparing the reports on the Chinese characters with those on the English equivalents that the *S*s show a marked tendency to over-estimate the English which is familiar and to under-estimate the Chinese which is strange.

(*d*) *Change of response and relation to success.* The statement is often made that the first answer is correct and that changing a response leads to greater inaccuracy. Our results do not bear this out. A total of 129 judgments were changed in our experiments; 66.6% were correct, 2.3% were bettered, and 31% were wrong.

(*e*) *Cues used and relation to success.* A special study was made of every response of all the *S*s in the 1923-24 experiments to discover what recognitive cues were used. Only 4 cases were given for which we could find no cue. All the cases showed cues which could readily be catalogued and classified. The cues discovered are listed below. Most of the headings are self-explanatory, but some need comment. Under "Nothing Analyzable" were classed those cases in which the *S* stated that there was nothing special, that the symbol was just familiar or not familiar, that nothing came back beyond that decision. Under "Alteration and Distortion" were placed such cues as shifting from a whole to a part, reversal, or inversion. Under "Error Cues" were placed those recognitions resulting from error in technique, that is where letters of the English word were exposed with the Chinese symbol, or there was a tear in the card, or faint printing interfered.

The cues employed were slightly more helpful with the "new" than with the "old" for 62.2% of the "new" was correct, as against 58.1% of the "old."

CUES USED AND RELATION TO SUCCESS

	Old as Old	New as New	Old as New	New as Old	TOTAL
Association	448	0	3	9	460
Would make Associations	0	62	48	0	110
Number of Associations	1	5	6	0	12
Spontaneity of Association	0	3	2	0	5
Lack of Association	0	9	10	0	19
Lack of Imagery	0	2	4	0	6
Reduction to a Principle	13	10	10	1	34
Meaninglessness	0	6	9	0	15
Special Parts	461	242	336	104	1143
Special Parts and Imagery	1	0	0	0	1
Whole	59	43	76	14	192
Complexity	28	36	44	10	118
Simplicity	31	48	35	11	125
Uniqueness	4	27	13	2	46
Nothing Analyzable	113	231	265	37	646
Feeling	60	18	28	7	113
Feeling and Association	4	0	0	0	4
Feeling and Imagery	4	0	0	0	4
Feeling, Imagery and Assn.	4	0	0	0	4
Imagery and Association	3	0	0	0	3
Visual Imagery	13	4	5	3	25
Verbal Imagery	9	1	0	1	11
Visual and Verbal Imagery	8	0	0	0	8
Muscular Adaptation	5	0	0	0	5
Alteration and Distortion	20	3	25	1	49
Error Cues	7	6	13	0	26
Reference to Other Series	16	11	15	3	45
Tendency for Meaning to Come	4	0	0	2	6
Relation to Number shown	0	0	1	0	1
No Decision	0	0	0	0	4
TOTAL	1316	767	948	205	3240

Degree of certainty and relation to success. In giving degree of certainty the *S*s stated that they were sure of their 75% and 100% responses but quite uncertain about their 25% and 50% judgments, adding that there was a wavering from one response to another resulting finally in a decision without any assurance of its correctness. With the trained *S*s it was found that they were slightly more certain of the "new" as "new", 39.8% being scored 75 and 100, whereas only 36.9% of the "old" as "old" received these ratings. With the untrained *S*s the certainty-values were higher, due, probably, to the fact that the trained *S*s were inclined to be more cautious and conservative than the untrained *S*s. The untrained *S*s were also more successful with the "new" as "new," the percent being 59.1% of 75% and 100% certainty while only 40.9% of the "old" as "old" had these percents of certainty. Where the responses were classed as 25% or 50% it was found, for both trained and untrained *S*s, that the "old" as "old," and the "old" as "new" occurred about equally frequent, thus verifying the *S*s reports that the response might as often be the one way as the other. With the "new" as "new", however, there was a difference with both groups of *S*s. The "new" as "new" was given at 25% and 50% certainty more than twice as often as was the "new" as "old."

(2) *Reaction times.* Accurate records of reaction time were kept for all responses but no analysis was made as the quantitative aspects of the problem were not under consideration. However, with both trained and untrained *S*s, the shortest reaction time was with the "old" as "old," the next shortest with the "new" as "new", the next with the "new" as "old," and the largest was with the "old" as "new."

(3) *Introspections.* (*a*) *Types.* In studying the responses of the *S*s in the 1923-24 experiments it was found that 76.2% of the correct responses were based upon some definite association either perceptual or ideational. Only 16.5% of the correct responses were classed as based on negative criteria, that is, nothing analyzable. Of the correct responses 4.4% were based on "feeling" while 0.2% were based on motor adjustment and 1.1% on attention shift. Both trained and untrained *S*s found it very difficult to analyze the process. They stated that the introspection seemed only to be a requirement for a decision.

Subject 6: decision; "new" (correct), degree of certainty, 75%. "Inferential judgment—something starts it—elusive to describe—simple—unusual character—surely I would remember that."

Subject 8: decision; "old" (correct), degree of certainty, 100% for symbol, 100% for word. "Commerce—foreign trade—Chinese gate in front and rear—coming in of things through series of gates—little attention to word—when this came mild feeling of familiarity followed by other perceptual cues as in original presentation—just after word commerce came."

Subject 8: decision; first, "new," degree of certainty, 100%, then change to "old" degree of certainty, 100% for symbol, and 100% for word. "Responded to as unfamiliar, then came as "horse"—four little dots at bottom—four legs and general streaming backward—when I saw top (it was) unfamiliar (with a) jumble below—a shift to the lower part, it flashed as familiar."

Subject 10: decision; "new" (correct), degree of certainty, 50%. "Don't know what newness consists in—don't remember—no familiarity."

(*b*) *Meaning.* It is interesting to note the words recalled by the various *S*s. There were 171 words remembered, 59 by 1 *S*, 28 by 2 *S*s, 9 by 3 *S*s, 6 by 4 *S*s and 1 by 5 *S*s. The word recalled by 5 *S*s was "curved" and in almost every case the association was by contrast. The words recalled by 4 *S*s were 'two," "police," "mountain," "army," "commerce," and "brook;" in every case definite associates had been aroused. Those recalled by 3 *S*s were "heart," "break," "sky," "book," "clothes," "tomorrow," "gold," "hail," and "minister." There were a number of words partially recalled and all of these show that a definite association had been made at the time of learning and had come back only in part. The importance of the presentation of the same situation presented at the learning period at the time of the test was noted frequently throughout this experiment. It has also been stressed by Strong[4] and Owen.[5] 16 words were given with new symbols, each of the new symbols having a part which was somewhat similar to a part in the old. 26 words were given which were not in the series and 23 of them with old symbols. 11 words were given with correct symbols and not recognized, apparently due to some distortion of the figure. For example, one *S* said in regard to the symbol for soldier: "Something about that gives the impression of one for the word 'soldier,' not the two lines underneath but the vertical line above it, something erect about it and there is a long line at top, not so pronounced as in 'soldier'." Another *S* said in regard to symbol for 'teacher': "There's a word association goes with it that begins with t—teacher comes but I immediately reject it."

SUMMARY

The results of this Study may be summarized as follows:

(1) Chinese symbols offer good material for psychological work.

(2) Better results were obtained with the Chinese symbols alone than when the symbols and their English equivalents are used together. This is probably due to a sacrifice of attention to the symbol in observing the word, and perhaps also in part due to the fact that efforts were sometimes made to associate symbol and word—the period often being too short for this—or to a farfetched association with some minor detail which proves disastrous at the test period.

(3) "New" as "new" seemed to be more readily recognized than "old" as "old", this corroborates the finding that the ability to know what we have not seen is greater than the ability to know what we have. The *S*s in their introspective reports state that they know what makes up "oldness" but could not say what makes up "newness."

(4) The greatest percentages of success in recognition were with the shorter periods—5 min., 20 min. and 1 hr. The results correspond to Ebbinghaus' curve of forgetting; a rapid decrease at first and then a gradual decline.

(5) Trained *S*s gave better results than untrained.

(6) No marked differences were noted for the sexes.

(7) The recognition of English words by the untrained *S*s was little better than for Chinese symbols.

(8) Although trained *S*s gave more English equivalents than the untrained, that is, approximately twice as many, the *S*s together recalled only 174 or 7.6% of the material presented.

(9) Untrained *S*s tested with the drawing of Chinese symbols at the test period had very little success. Only 10, or 0.9%, were correctly drawn.

(10) The problem with a Chinese student is quite a different one, being not so much a recognition of the symbol shown as of its English

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[4] E. K. Strong, *Psych. Rev.*, 20, 1913, 339-372.

[5] R. B. Owen, *Psych. Mon.*, 20, 1915, No. 86.

equivalent. For Chinese *S*s the experiment would require different technique.

(11) Incidental data in regard to localization showed successful localization with the first and last in the series, and a slight indication of more success with the last than the first.

(12) Coincidence of estimated with actual success with Chinese was noted in 51% of the cases; 34% were under-estimated. This was probably due to lack of familiarity with Chinese, since with the English words there was a tendency to over-estimation and a slightly higher coincidence.

(13) Change of response was for the better in 68.9% of the cases.

(14) Cues are used for judgment but do not represent the judgment process itself. In this study 58% of the cues were successful with "old" as "old", and 62% with "new" as "new."

(15) Study of reaction time shows shortest reaction for "old" as "old," next for "new" as "new," somewhat longer for "new" as "old," and longest of all for "old" as "new."

(16) The highest degree of certainty is shown for "new" as "new."

(17) The introspective reports are meager because of the fleeting nature of the experience and of the difficulty of analyzing the processes involved.

(18) The results of this Study seem to favor the association theory of recognition.

THE PROCESS OF MUSICAL CREATION

By Henry Cowell, Stanford University

Introductory note. Henry Cowell is the young musical composer described on pages 246 to 251 of my book, *The Intelligence of School Children*, (1919). The history of Mr. Cowell is in many respects of exceptional psychological interest. His case offers a remarkable example of inborn genius pressing to victory against the most adverse circumstances; he is of high IQ, unsupported by formal schooling of any kind, and of creative ability in musical composition combined with exceptional aptitude for scientific thinking. Mr. Cowell's school days ended before he was seven years old, but before he was 23 he had served as an instructor in a great university. At fourteen he had never "touched" a piano and had no musical training, but he has since played his own compositions to large audiences throughout the United States and in England, France, and Germany. He has published some twenty or more of his compositions and has created a new technique which has attracted wide attention among American and European musical critics. He is the author of a number of articles published during the last few years in *The Sackbut* (London) and *Musical America* (New York), and in collaboration with Professor S. S. Seward, of Stanford University, has written a book (not yet published) on *New Musical Resources*. It is a matter of special psychological interest that Mr. Cowell was composing music of an extreme "modernistic" type when he was a mere boy and unaware of the very existence of such a school of composers.

Mr. Cowell is now 28 years old. Nine years ago I wrote as follows: "It remains to be seen whether Henry will become one of the famous musical composers of his day; several musical critics of note hope for this outcome." The hope seems to be well on the way toward realization. Pitts Sanborn, in *The League of Composers Review*, May, 1924, writes: "I have no hesitation in saying that to me the outstanding American composer of the season was Henry Cowell, of tone-cluster fame." Adolf Weissmann, in *Die Musik*, Berlin, January, 1924, describes the appearance of Henry Cowell in Berlin in the autumn of 1923 as "the most remarkable event" of the local concert season. According to the *London Daily News*, December 23, 1923, "there is no reason . . . why Mr. Cowell's theory should not acquire a place in Grove's Dictionary of Music, and an honored position on the concert platform." The nature of Mr. Cowell's original contribution to musical technique is indicated by the following statement of Paul Rosenfelt in *The Dial*, New York, April, 1924: "Felicitations on the discovery of a method cannot be denied Henry Cowell; and in an age of small technical innovations he cuts a not unrespectable figure. Those tone-clusters of his, sounds produced on the pianoforte with the side of the hand, the fist and the lower arm, extend the scope of the instrument, and offer some new possibilities to composition. Concordances of many close-lying notes have been used by Leo Ornstein since he wrote his Dwarf Suite; and Percy Grainger calls for notes struck from the strings inside the box of the piano in one of the Nutshell movements; but it has been left for the young Californian to demonstrate completely the quality of sound to be produced on concert grands by the deliberate application to the key board of muscles other than those of the finger-tips, and by the application of the fingers to the wires themselves. New lovely rolling sounds occur in all of the pieces of Cowell which employ the new method of tone-production: "Dynamic Motion," "Antimony," and "The Voice of Lir" in particular. The piece for "Piano with Strings" has a fine dead quality of resonance not to be produced on any harpsichord. And it seems probable that writers for the pianoforte will profit by his experiments and enlarge the expressivity of the instrument. It is even possible that Cowell's method may find itself applied to the music of the past; that passages of Beethoven sonatas will be treated by it, and brought to greater effectiveness."

The following article, written at my suggestion, affords an interesting glimpse of the way in which at least one musical composer's mind works in its creative activities. It is a type of psychological report which is all too rare, and the reader will doubtless join with me in the hope that Mr. Cowell will sooner or later find time to present a more detailed record of his early musical developemnt and of the manner in which his compositions take form in his mind. LEWIS M. TERMAN

There are few things more mysterious to the non-musician than the process of musical creation. I rarely pass more than a few days that someone does not ask how I work: "Does it just come to you," people usually ask, "or do you work it out by rules?"

A popular misconception is that in order to be inspired a composition must have been improvised or played on the instrument for which it was written, and that when a composer writes music at his desk, without recourse to his instrument, he does so by means of some cut-and-dried formula or purely intellectual process. I have often wondered how a composer relying thus on improvisation is expected to write an orchestral work, when he could, at best, play only one instrument at a time out of the hundred or more in a symphony orchestra!

The misconception is doubtless caused by a lack of appreciation of the fact that the most perfect instrument in the world is the composer's mind. Every conceivable tone-quality and beauty of nuance, every harmony and disharmony, or any number of simultaneous melodies can be heard at will by the trained composer; he can hear not only the sound of any instrument or combination of instruments, but also an almost infinite number of sounds which cannot as yet be produced on any instrument.

Each composer, of course, has his own peculiar mental processes and way of working, yet I believe that in order to compose seriously he must have the type of mind that is capable of thinking as accurately in terms of sound as a literary author might think in terms of words.

In this regard one must distinguish between a composer and a performer who writes occasionally; for while the former is an indubitable rarity, nearly all professional instrumentalists write pieces once in a while, some of which contain much charm. There is, however, a great difference of quality between the work of a composer and that of a performer. I have never seen a performer who had developed the particular type of musical imagination described above, although many good performers have it to some degree.

It is doubtful whether any composer can have a well-working "sound-mind" without going through a rigorous process of self-training to make it so. I will give as an example my own development; several other composers have told me they went through a similar progress.

As a child I was compelled to make my mind into a musical instrument because between the ages of eight and fourteen years I had no other, yet desired strongly to hear music frequently. I could not attend enough concerts to satisfy the craving for music, so I formed the habit, when I did attend them, of deliberately rehearsing the compositions I heard and liked, in order that I might play them over mentally whenever I chose. At first the rehearsal was very imperfect. I could only hear the melody and a mere snatch of the harmony, and had to make great effort to hear the right tone-quality. I would try, for instance, to hear a violin tone, but unless I worked hard to keep a grip on it, it would shade off into something indeterminate.

No sooner did I begin this self-training than I had at times curious experiences of having glorious sounds leap unexpectedly into my mind—original melodies and complete harmonies such as I could not conjure forth at will, and exalted qualities of tone such as I had never heard nor before imagined. I had at first not the slightest control over what was being played in my mind at these times; I could not bring the music about at will, nor could I capture the material sufficiently to write it down. Perhaps these experiences constituted what is known as an "inspiration."

I believe, had I let well enough alone and remained passive, that the state of being subject to these occasional musical visitations would have remained, and that I would now be one of those who have to "wait for an inspiration." But I was intensely curious concerning the experiences and strove constantly to gain some sort of control over them, and finally found that by an almost super-human effort I could bring one of them about. I practiced doing this until I became able to produce them with ease. It was not until then that I began to develop some slight control over the musical materials. At first able to control only a note or two during a musical flow lasting perhaps half an hour, I became able, by constant attempt, to produce more and more readily whatever melodies and harmonies and tone-qualities I desired, without altering the nature of the flow of sounds. I practiced directing the flow into the channels of the sounds of a few instruments at a time, until I could conjure their sounds perfectly at will.

As soon as I could control which sounds I should hear, and turn on a flow of them at will, I was able, by virtue of studying notation, to write down the thought, after going over it until it was thoroughly memorized. I have never tried to put down an idea until I have rehearsed it mentally so many times that it is impossible to forget the second part while writing down the first.

I shall never forget the disappointment I experienced when I first wrote down a composition and played it. Could it be that this rather uninteresting collection of sounds was the same as the theme that sounded so glorious in my mind? I rehearsed it all carefully; yes, it was the same harmony and melody, but most of the indescribable flowing richness had been lost by the imperfect playing of it on the imperfect instrument which all instruments are. Since then I have becomed resigned to the fact that no player can play as perfectly as the composer's mind; that no other instrument is so rich and beautiful, and that only about ten percent of the musical idea can be realized even at the best performance.

I am able now to produce a flow of musical sounds at will, and to control just what they shall be. I am therefore able to work at any time, as the musical flow would continue indefinitely if I did not shut it off when I have not the time to work. The flow does not merely ramble on ambiguously, but centers about a germinal theme, which it proceeds to enlarge upon. I usually compose around a theme for several months before it develops into its final form as written. Because of devoting so much attention to finding the finest form beforehand, by trying the initial idea over mentally in every conceivable way, I rarely change a note after a composition is written.

Writing in form, I may add, is not a matter of pushing certain sounds into an unyielding mold; crudities of form tend to drop out unconsciously as further experience is gained. The experience of being in the throes of musical creation is distinctly an emotional one; there is a mere semblance of the intellectual in being able to steer and govern the meteors of sound that leap through the mind like volcanic fire, in a glory and fullness unimaginable except by those who have heard them.

The closest observation on my part has failed to reveal what the exact relationship is, if there be one, between my musical creations and the experiences which have preceded it, either immediately or remotely. I can only say that the musical ideas as they run through my mind seem to be an exact mirror of my emotions of the moment, or of moments which I recall through memory.

ENDING PREFERENCES IN TWO MUSICAL SITUATIONS

By Paul R. Farnsworth, Stanford University

I. The Resolution of Scale Notes

Problem. It is traditionally stated that there is a fixed resolution for the various notes of the scale.[1] More feeling of repose and finality is developed by ending on certain notes than on others. Thus, in the *'tonic sol fa'* system, *re* (d in the key of c) resolves to *do* (c) or to *me* (e), *fa* (f) to *me*, *sol* (g) to *do*, *la* (a) to *sol*, and *ti* (b) to *do*. This means that if the melody is resting on *re*, it will usually proceed to *do* or to *me* as a point of final rest. *Do* and *me* are 'magnet' tones and have no resolutions.

Resolutions

Scale Name	Scale Letter in Key of C
re to *do*	d to c
re to *me*	d to e
fa to *me*	f to e
sol to *do*	g to c
la to *sol*	a to g
ti to *do*	b to c

To the writer's knowledge, no explanation has been tendered for these resolutions. An attempt will be made in this paper, therefore, to analyze the problem, and to link these phenomena with known laws.

Introduction. Let us take as our type the diatonic major scale in the key of c.

c	d	e	f	g	a	b	c
16	18	20	21	24	27	30	32

The numbers below the scale denote the ratio frequencies. Thus, the frequency of c is to that of d as 8 is to 9. It is much simpler to reduce these ratio frequencies to ratio symbols,—that is, to extract the pure powers of 2. This can be done without changing the scale relationships. Our scale then becomes

c	d	e	f	g	a	b	c
2	9	5	21	3	27	15	2

To understand the explanation the writer has to offer, two psychological principles must be accepted. The first is that of the tonic effect. Max Meyer[2] has shown that human beings prefer the ending of a melody to fall on the tonic,—on ratio symbol 2. This means that in the key of c, providing no harmonization is introduced, c (ratio symbol 2) will be preferred over any other tone as a point at which to end. In the key of g there is a shift and g becomes the ratio symbol 2 and so has the tonic effect. The tonic is very striking as it calls forth a decided reaction of rest and finish.

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[1] C. A. Alchin, *Applied Harmony*, 1921.

[2] M. F. Meyer, this Journal, 14, 1903, 195.

The second is the habit principle. In musical esthetics, just as in other walks of life, habits are formed, and among these are several relating to ending effects. The writer has demonstrated in a previous article[3] that the discrimination of 'finality, rest and repose' depends also upon the association of a ratio symbol with the basal position in a tonal succession; that is, the more often one hears a sequence with x as its final note, the more one attaches the repose discrimination to note x.

Proposed Explanation. (1) *Tonic Effect.* Looking now at the scale relationships it can be seen that since the c (*do*) is the ratio symbol 2, it will attract the neighboring notes to it as a point of repose. This easily explains the resolution of *re*, *sol* and *ti* to *do*. *La* resolves to *sol* since 27 proceeds to 3 as its tonic; that is, 27 is to 3 as 9 is to 2 (2 and 1 are identical as ratio symbols).

Neither the resolutions of *fa* and *re* to *me*, nor the rest effect of *me* can, however, be explained on this basis. Were this the only factor functioning, *fa* would resolve to *sol* (21 to 3 equals 7 to 2), and *me* to *do* (5 to 2). The habit factor must now be introduced.

(2) *Habit Effect.* The important triad sequence of a scale is the tonic triad, which exists in three positions,—root position, first inversion, and second inversion. In the key of c, the tonic triad, ceg, displays itself as,—root position, ceg (arranged from lowest to highest); first inversion, egc; and second inversion, gce. The first two positions are the most important ones, since the tonic, c, is at the bottom and top respectively.

Besides the tonic, a habit factor operates here. In the root position, the c (*do*) is the bass of the e and g, and is heard in this arrangement very frequently. In the first inversion, egc, the e (*me*) appears as the bass of the g and c, and because of its frequent usage takes on the 'rest' characteristic of a bass. It is subsidiary, of course, to c, because of the superiority of the latter's tonic effect. The basal g of the second inversion, gce never becomes a bass in the same sense, since this inversion is not in as frequent usage. Thus, the *me* (e) because of its basal position in the first inversion, becomes a secondary bass in the scale. It attracts (if we may use the term) *fa* (f) and sometimes *re* (d) to itself.

II. TONIC EFFECT WHEN NO KEY IS INDICATED

Tonic effects are sometimes clearly discriminated when no key is indicated. For example, when the tones c and f are played together, there is a decided preference for ending on f. This means that it is now the f and not the c which is the ratio 2. The simple melody is heard in the key of f.

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[3] P. R. Farnsworth, this JOURNAL, 37, 1926. 116-122.

Earlier in the paper there were presented the ratio symbols for the key of c. To change these to the key of f, the ratio symbol 2 is shifted from c to f and the other symbols treated correspondingly.

Ratio Frequencies in Key of F with Notes Occurring in Key of C:

c	d	e	f	g	a	b	c
24	27	30	32	36	40	45	48

These reduced to ratio symbols become:

3	27	15	2	9	5	45	3

It should be noted that in the key of c, the ratio symbols of c and f are 2 and 21 respectively. In the key of f, however, they are 3 and 2. The question thus arises: Why are c and f given the ratio symbols 3 and 2 instead of 2 and 21 as in the former scale?

As has been pointed out previously, the tonic triad of the key of c is ceg. The note f is not a part of this triad. The tonic triad of the key of f is fac, with f as the bass and c the soprano (root position). Since the f occurs so frequently as the bass of c, the habit principle operates and causes the former to be reacted to as the tonic of the latter. We hear the notes c, f, c, f, *etc.* as in the key of f,—with f as the ratio symbol 2.

This effect may be easily changed. Repeated practice in the key of c, or the repetition of first f, then the next lower c for a large number of times, will make c appear eventually as the tonic of f.

When c and g are played together with no key indicated, the combination is usually heard in the key of c. This is true since c does not appear in the tonic chord of g, gbd, but is g's bass in the tonic chord of c, ceg (root position).

It may also be true that the organism natively interprets the tone combination in terms of the smallest possible ratio symbols (3-2, and not 2-21). However, proof is lacking on this point.

Bingham[4] has offered an explanation which is similar in some respects, but differs in others. For him, the first tone arouses a slight tonality feeling. If the second tone fits into the first's tonic chord, the first remains the tonic. If the first fits into the second's tonic chord, the second becomes the tonic. The difficulty here, it seems to the writer, lies in the first statement. Since tonality (ratio symbol 2) is a relational stimulus, how can it exist by itself and arouse a slight tonality feeling?

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[4] W. V. D. Bingham, *Psychol. Monog.*, 12, 1910, 39.

SUMMARY

(1) In the *tonic sol fa* system, the resolutions of *re*, *sol*, and *ti* to *do*, and *la* to *sol* can be explained by the tonic effect involved.

(2) The resolutions of *re* and *fa* to *me* can be explained by a habit principle. *Me* occurs as the bass note in the first inversion of the tonic triad,—a very important scale triad. It is therefore reacted to as a bass, secondary in character to *do*, but as having tonic properties.

(3) When no key is indicated, the tonic effect is obtained from previous habits. One of the notes involved has occurred very frequently as the bass of the other, and is at length reacted to as its tonic.

AN EXPERIMENT ON BACKWARD ASSOCIATION IN LEARNING

By H. E. Garrett and G. W. Hartman, Columbia University

This paper describes an experiment designed to study the importance of backward association in the learning of nonsense syllables. The notion of backward association in memory leads directly from the investigations of Ebbinghaus consequently, in order to put our problem in its proper setting, we give a resumé of his work and also a review of the criticisms that this work has received.

Resumé of Ebbinghaus' work. In his classical work on memory, Ebbinghaus showed that when series of nonsense syllables are learned in 1, 2, 3, etc., order to the point of one correct recitation, not only are associations formed leading from any given syllable to the one following from this to the next following, etc., but that indirect and remote associations are also formed between any given syllable and others in the series. "With repetitions of the syllable series," he says, "not only are individual terms associated with their immediate sequents, but connections are also established between each term and several of those which follow it beyond intervening members. To state it briefly, there seems to be an association not merely in direct but in indirect succession. The strength of these connections decreases with the number of intervening members."[1]

The discovery of remote forward associations led Ebbinghaus to postulate and investigate the existence of reverse or backward associations. The problem may be stated thus: "The idea *a* whenever and however it returns to consciousness has certain tendencies of different strengths to bring also with it to consciousness the ideas *b*, *c*, and *d*. Are now these connections and tendencies reciprocal? That is, if at any time *c* and not *a* is the idea by some chance revived, does this have, in addition to the tendency to bring *d* and *e* back with it, a similar tendency in the reverse direction towards *b* and *a*?"[2] To test this proposition, Ebbinghaus in relearning simply reversed the sequence of his syllables, or reversed the series skipping one. Thus the series which had been learned 1, 2, 3, . . . 15, 16 were relearned either as 16, 15, . . . 4, 3, 2, 1; or as 16, 14, 12, . . . 4, 2, 15, 13, 11, . . . 3, 1. For simple reversal the saving in time was 12.4%; for reversal skipping one it was 5%. These results led Ebbinghaus to conclude that certain connections are formed in the backward as well as in the forward direction. "These connections," he says, "are revealed in this way, that series which are formed out of members thus connected are more easily learned than similar series whose individual members are just as familiar, but which have not been previously connected."[3]

Criticisms. Because of the originality of his method and the prestige which his careful work immediately secured, Ebbinghaus' results have been generally accepted. James comments on them in the following words: "His [Ebbinghaus'] experiments conclusively show that an idea is not only associated directly with the one that follows it, and with the rest through that, but that it is directly associated with all that are near it, though in unequal degrees."[4] Adverse criticism of Ebbinghaus' work, however, has not been lacking. Thus Münsterburg insisted that there were a number of sources of error in Ebbinghaus' technique. He declared that the simultaneous presence of a number of syllables in the visual field was responsible for the remote associations, and that "had he [Ebbinghaus] kept the series covered, and constantly exposed only one syllable after the other, the result on this point would have been different."[5] Likewise, Ward, reasoning in much the same vein, states that cases of apparent regressive association really involve only relations of "coexistence." Referring to Müller and Pilzecker's use of anapestic rhythms, he writes: "We are concerned not with a series but with a *tout ensemble*, the foot in the one case, the line in the other. The same tendency to unify and organize which has made out of two syllables a foot has made out of six feet a line: in both cases the syllables in addition to their originally temporal order have acquired the relation of part to part in a coexistent whole."[6]

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[1] H. Ebbinghaus, *Memory, A Contribution to Experimental Psychology*, 1885 (trans. by H. A. Ruger and C. A. Bussenius, 1913), 101.

[2] *Op. cit.*, 110. [3] *Op. cit.*, 112 f.

[4] William James, *Principles of Psychology*, 1890, I, 677.

Quite recently Cason has reopened the question.[7] He repeats the objections made by other critics to the effect that Ebbinghaus did not control his conditions sufficiently well to guarantee that attention moved always from each syllable to the next following; and advances the opinion that practice consequently was in the backward as well as in the forward direction, and that the so-called backward associations were formed by this backward practice.

Problem. Analysis of the problem would indicate that the crux of the matter lies in the mode of presentation. With his lists spread out before him, Ebbinghaus had the advantage of a complete continuous presentation of the memory material. It is more than likely, therefore, that many perceptual aids—not accessible to introspection—were present, and these might have been sufficient *per se* to account for the backward associations found. If this explanation is a true one, we should then expect, given experimental conditions so controlled that learning can proceed *only* in the forward direction, no saving (or a minimum of saving) when the lists are relearned in the reverse direction. To test this hypothesis, however, the syllables of the list must be exposed singly and rapidly enough to prevent any "thinking back" on the part of the *S*. This is what we have attempted to do in the present experiment.

Apparatus and Materials. Lippmann's Memory Apparatus was used to expose the materials. This little instrument is essentially a rotary tachistoscope in which the time of each exposure as well as the interval between exposures can be accurately controlled. An adjustable slit permits the exposure of only one syllable at a time. As material for the experiment syllables of 4 letters were used and 12 of these syllables were presented in every list. Ebbinghaus used 16 syllables in most of his work, but reports that the relative saving—after 24 hours—was little different from that found with series of 12 or 13 syllables.[8]

Procedure. Ten *S*s took part in the experiment: 9 men and 1 woman. For comparative purposes, it was decided to match each list learned by the "discrete" (*i.e.* tachistoscopic) method with a list learned by the usual "continuous" method. The procedure of the "continuous" method was as follows. Every *S* was given a list of syllables and the following instructions: "Read carefully and in regular order the list of nonsense syllables given you. When you have completed the list once, turn over the paper and attempt a recitation. Repeat your readings until you are able to give a correct reproduction. If you make an error you will not be corrected. Always read the list through from beginning to end, and not in sections."

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[5] Hugo Münsterburg, Die Association successiver Vorstellung. *Zeit. f. Psych.*, 1, 1890, 101.

[6] James Ward, *Psychological Principles*, 1919, 230 ff.

[7] Hulsey Cason, The Concept of Backward Association, this JOURNAL, 35, 1924, 217-229.

[8] *Op. cit.*, 95.

As soon as *S* declared that he understood what was expected of him, he was given a list of syllables (List I in the Table). In order to avoid overlearning, an attempt at recitation was required after every reading. When *S* had learned the list up to the point of one correct recitation, the experiment was stopped and the number of repetitions necessary for learning noted. *S* was then asked to make a written report of the aids used, difficulties met, etc., in learning the list. After a lapse of 20 minutes, the list was relearned and the number of repetitions again noted.

In the learning by the tachistoscope, *i.e.* the "discrete" method, a list of 12 syllables (List II in the Table) was presented to *S*, one syllable at a time. After some preliminary trials, it was decided to allow 10 sec. for the presentation of the 12 syllables—about 0.8 sec. per syllable. This seemed slow enough to allow *S* to learn the list with reasonable facility, and also fast enough to prevent any "thinking back." List II was exposed until it could be just correctly reproduced, and 20 min. later was relearned. The number of repetitions were recorded in both cases.

Lists I and II are—in a sense—control lists. Following these lists, list III was learned by the "continuous" method and 20 min. later was relearned by the same method as list IIIr which was obtained by reversing the syllables in list III. Similarly lists IV and IVr were learned by the "discrete" method. One other variation was made. List V was learned by the "continuous" method, and after 20 min. relearned as List Va. Va was obtained by reversing every pair of syllables in List V: if the original series read 1, 2, 3, etc., the list to be relearned read 2, 1, 4, 3 . . . 12, 11. Lists VI and VIa, corresponding to Lists V and Va, were learned by the "discrete" method.

Every effort was made to eliminate error in the conduct of the experiment. The lists were always read straight through from first to last; the intervals between experiments (*i.e.* between the different lists) were constant, attention was held at the highest level throughout, and the time of day at which experiments were held was strictly regulated.

Results. The results of the experiment are given in Table I. The Roman numerals indicate the different lists; the odd numbered lists, I, III, and V, were learned by the "continuous" method, the even numbered, II, IV, VI, by the "discrete" method. The number of repetitions required for learning and relearning the different lists and the percent saved are given in the body of the table. At the bottom is given the average number of repetitions for each list and the average percent of saving for each type of arrangement and mode of presentation.

The most striking thing in Table I is the relatively small difference in the percent of saving between the "continuous" and the "discrete" methods. There is an average of only 1% in favor of the "continuous" method when the lists are unchanged in relearning; an average difference of about 4% in favor of the "discrete" method when the syllables are reversed; and an average difference in saving of about 10% in favor of the "continuous" method when the syllable-pairs are reversed.

TABLE I

Lists Subj'ts	*Learned* I	*Relearned* I	% saved	*Learned* II	*Relearned* II	% saved	*Learned* III	*Relearned* IIIr	% saved	*Learned* IV	*Relearned* IVr	% saved	*Learned* V	*Relearned* Va	% saved	*Learned* VI	*Relearned* VIa	% saved
A	14	5	64	14	7	50	13	8	38	13	3	77	12	7	42	10	9	10
B	16	3	81	25	3	88	11	9	18	21	6	71	20	7	75	15	10	33
C	15	5	66	28	8	71	16	5	69	12	2	83	10	4	60	13	9	31
D	20	2	90	16	1	94	11	1	91	10	1	90	13	5	61	12	2	83
E	13	1	92	9	2	78	11	3	73	14	3	78	10	8	20	11	5	55
F	12	2	83	13	1	92	23	6	74	12	7	42	9	8	11	14	11	22
G	7	1	86	9	1	89	9	2	78	7	4	42	6	3	50	6	4	33
H	25	4	84	24	8	67	23	6	74	17	7	60	13	7	46	27	29	−7
I	12	3	75	6	1	83	9	7	22	7	4	42	13	6	54	9	7	22
J	17	1	94	13	1	92	12	1	92	11	2	82	8	3	62	17	1	94
Av. rep.	15.1	2.7		15.7	3.3		13.8	4.8		12.4	3.9		11.4	5.8		13.4	8.7	
Av. % saved			81.5			80.4			62.9			66.7			47.1			37.7
S.D.			9.7			13.4			25.6			17.1			17.4			29.8

Let us consider these results in order. In the first place, one might have expected the number of repetitions required in learning, or in relearning, to be considerably less in List I than in List II because of the overlapping which is possible in the "continuous" method. Probably the almost identical results, therefore, are due to the greater possibility also for interference in this method. Granting that there was little, if any, overlapping in the learning of List II, interference might still have served to lengthen the learning somewhat. In List I, however, while learning would be expedited by overlapping, interference would very probably be much greater than in List II, and hence the two would serve to cancel each other. The end result in the two methods would accordingly be about the same.

For the specific problem of backward association, the Lists III, IIIr, IV, IVr, V, Va, and VI, VIa are, of course, of greater importance then Lists I and II. From Table I we find that a reversed list is relearned somewhat more quickly by the "discrete" than by the "continuous" method; and in the lists in which the pairs of syllables were reversed, the "discrete" method was but little worse than the "continuous." These results, at first glance, are unexpected and somewhat disconcerting. Instead of no saving when the reversed lists or the reversed syllable-pair lists are relearned by the "discrete" method, the percent of saving is as great by this method as by the "continuous."

The simplest explanation of the relatively high percent of saving in relearning List IVr and List VIa would seem to be that in the original learning there was considerable practice in the backward as well as in the forward direction, despite the use of the tachistoscope. This backward practice might have occurred as a result of the continuous "circular" presentation of the syllables, *i.e.* the repeated exposure of the list until it was learned. Again, practice in the backward direction may have easily taken place during the fairly numerous attempts at reproduction before learning was accomplished. No doubt this is what actually did occur in relearning the reversed list and the reversed-pair list (IIIr and Va) by the "continuous" method; and there seems to be no reason for believing that such practice was not present also in the "discrete" learning. The reports of the subjects may throw some light on this question. When the reversed lists appeared, every *S* immediately guessed the order of the new arrangement, while the order of the reversed-pairs was at once evident to three *S*s, the others thinking at first that the arrangement was haphazard. One *S* reported that he simply selected definite points of reference in the lists from which he could move in either direction; another reported that he "shifted the gears of his mental set" when confronted with the changed list and learned the already familiar syllables in the new order. In the attempted recitations, *E* noted that syllables 1, 2, 3, and 10, 11, 12 were nearly always learned first and recited almost as a unit. Halts and repetitions, no doubt, afforded opportunity for reverse associations. Moreover, as there were only 12 syllables and the interval between learning and relearning comparatively short (20 min.), it was possible for the syllables to become very familiar as separate entities. This might well be tested by using longer lists and a greater interval between learning and relearning.

It is possible, of course, that true backward associations in the sense of regressive connections between response and stimulus actually occurred though this in view of other considerations seems improbable. For example, Cason, in the article previously mentioned,[9] offers the principle of one-way conduction across the synapse as evidence of the difficulty of a neural explanation of backward association. Furthermore the results of experiments in animal learning indicate little or no backward elimination of errors in maze running.[10] In learning a list of nonsense syllables, it is evident that the S-R bonds are VS^{10} (first visual stimulus, *i.e.* syllable) $\longrightarrow R_1$ (subvocal response of pronouncing the syllable); $VS_2 \longrightarrow R_2$; $VS_3 \longrightarrow R_3$, etc. It is highly improbable that any neural connections are formed from R to VS. In reciting the list—aloud or subvocally—however, the fact must not be lost sight of that R_1 (first vocal response) is the stimulus for R_2, that R_2 is, in turn, the stimulus for R_3, etc., and that the associations here may be in either direction. Thus it is not at all uncommon for a *S* to give R_1, then R_2, then R_4 and back again to R_3; which indicates that he has a general familiarity with the list as a whole, or as a total experience. An experiment of Wohlgemuth may be cited as a case in point.[11] This investigator, who worked on a problem somewhat similar to the one here described, reported that although associations formed between syllables are predominantly in the forward direction, associations between colors and diagrams are often equally strong in both directions. Wohlgemuth introduced a variation into the usual procedure of learning lists of nonsense syllables, by having his subjects say "ten" every time that a syllable appeared. When the motor factor of verbalization was thus suppressed, the results with nonsense syllables were identical those from experiments with colors and diagrams. From this fact Wohlgemuth concludes that in motor memory associations are in the forward direction *only*, while in visual and auditory memory they are equally strong in both directions. He writes: "It seems probable that we have here a fundamental distinction between what I propose to call *physiological memory* on the one hand and *psychological* memory on the other. In the former the associations are quite irreversible; in the latter they are completely reversible." It is doubtful whether the distinction between physiological and psychological memory made by Wohlgemuth is a useful or a valid one. All memory of whatever kind consists of the capacity to make learned responses, and as such must be basically physiological. "Psychological memory" as defined, seems to be merely a case of grouping into larger units. The distinction between motor memory and visual and auditory memory, on the other hand, is worthy of more extended study.

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[9] *Op. cit.*, 221. Cason writes: "This motor response (*i. e.*, subvocal response of syllable 1) later becomes associated with nerve cells in the brain which in turn cause the speech organs to pronounce (subvocally) syllable number 2, etc." This use of the term *association* seems to be an unwarranted use of a "mentalistic" term. Connection or integration is probably what was meant.

[10] C. J. Warden, Some Factors determining the Order of Elimination of Culs-de-sac in the Maze, *Jour. Exp. Psych.*, 6, 1923, 192-210.

While our results confirm Ebbinghaus' insofar as they indicate that indirect associations—even in the backward direction—are often formed, they neither prove nor disprove the existence of "true" backward association. Unfortunately, therefore, no definite conclusions can be drawn. In future work, we plan to make conditions more rigorous. If the time of exposure is still further reduced, the interval between learning and relearning increased and varied, the list lengthened, and recitations reduced, results would certainly be more conclusive.

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[11] A. Wohlgemuth, On Memory and the Direction of Associations, *Brit. J. Psych.*, 5, 1913, 447.

A PRELIMINARY STUDY OF THE RANGE OF ATTENTION

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Two methods of investigation have been used in studying the range of attention: the simultaneous and the successive; and in both the range has been determined by the number of stimuli correctly apprehended. Specifically, the range has been determined in the simultaneous method by noting the number of objects (marbles, beans, lines, letters, dots, colors, numbers, geometrical figures and words) that can be correctly cognized during an exposure so short that eye-movements are excluded;[1] and, in the successive method, by noting the number of consecutive stimuli (sounds, touches, lights, etc.) that can be correctly apprehended without counting.[2]

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[1] The list is long; only the most important are given:

Wm. Hamilton, *Lectures on logic and metaphysics* (edited by H. L. Mausel and John Veitch, 1859), I, 253 f.

W. S. Jevons, The power of numerical discrimination, *Nature*, 3, 1871, 281 f.

J. McK. Cattell, Ueber die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern und Farben, *Philos. Stud.*, 2, 1885, 635 ff.; Ueber die Trägheit der Netzhaut und des Sehcentrums, *ibid.*, 3, 1886, 121 ff.; The inertia of the eye and the brain, *Brain*, 8, 1886, 248, 404.

W. Wundt, *Grundzüge der physiol. Psychol.*, 4 ed., 1893, II, 286 ff.; 5 ed., 1903, III, 351 ff.; 6 ed., 1911, III, 324 ff.

W. O. Krohn, An experimental study of simultaneous stimulations of the sense of touch, *J. Nerv. & Ment. Dis.*, 20, 1893, 169-184.

B. Erdmann & R. Dodge, *Untersuchungen über das Lesen*, 1898, 166 ff.

J. Zeitler, Tachistoskopische Untersuchungen über das Lesen, *Philos. Stud.*, 16, 1900, 410 ff.

J. P. Hylan, The distribution of attention, *Psychol. Rev.*, 10, 1903, 393 f.; 498 ff.

F. N. Freeman, Untersuchungen über den Aufmerksamkeitsumfang und die Zahlauffassung bei Kindern und Erwachsenen, *Päd. Psychol. Arb.*, 1, 1910, 88-168.

C. Kraskowski, Die Abhängigkeit des Umfanges der Aufmerksamkeit von ihrem Spannungszustande, *Psychol. Stud.*, 8, 1913, 271-326.

K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exp. Psychol.*, 3, 1920, 228 f.; Dr. Oberly on "The range for visual attention, cognition and apprehension," this Journal, 36, 1925, 154-156.

S. W. Fernberger, A preliminary study of the range of visual apprehension, this Journal, 32, 1921, 121-133.

H. S. Oberly, The range for visual attention, cognition and apprehension, this Journal, 35, 1924, 332-353; Further results in "The range for visual attention, cognition and apprehension" experiment, *ibid.*, 37, 1926, 132-138.

[2] G. Dietze, Untersuchungen über den Umfang des Bewusstseins bei regelmässig aufeinander folgenden Schalleindrücken, *Philos. Stud.*, 2, 1885, 362 ff.

W. Wundt, *op. cit.*, 3 ed., 1887, II, 248 ff.; 4 ed., 1893, II, 292 ff.; 5 ed., 1903, III, 360 ff.; 6 ed., 1911, 330 ff.; Über die Methoden der Messung

These procedures are justifiable only on the assumptions that all clear processes are cognized or apprehended, and contrariwise that all the processes cognized or apprehended are clear; but neither of these assumptions, as recent experimental work has shown, is warranted.[3] An inventory of the experimental results reveals no invariable relation between attributive and cognitive clearness. The conditions for these two kinds of clearness are not the same: an impression may be attributively clear and cognitively clear, attributively clear and cognitively unclear, attributively unclear and cognitively clear, or attributively unclear and cognitively unclear; hence the extent or range of one kind of clearness cannot be employed as an index or measure of the other. The experiments that have thus far been made upon the problem of 'range' have been concerned with cognition; consequently the term 'range of attention' has been mistakenly applied to them. These experiments give us merely the range of cognition, and "the statement in current textbooks that the 'grasp of visual attention covers from four to six simultaneously presented simple impressions' is a statement concerning visual apprehension, not attention."[4]

The problem of 'range' has been further complicated by the fact that different investigators have required from their observers, reports at very different levels of cognition. Some, for example, have asked their *O*s to report merely the number of stimuli perceived (in the experiments with marbles, beans, dots and lines), others have instructed their *O*s to name the stimuli (in the experiments with letters, words, and numbers), while yet others have required their *O*s to describe the impressions (in the experiments with colors, geometrical figures, and forms). The various experimenters have obtained different constants for the 'range'—and it is small wonder, since their *O*s were working at different levels of cognition.

Not only have a variety of materials and procedures been used, but matters have been complicated still further by the

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des Bewusstseinsumfanges, *Philos. Stud.*, 6, 1890, 250-260; Zur Frage des Bewusstseinsumfanges, *ibid.*, 7, 1891, 222-231.

F. Schumann, Über das Gedächtnis für Komplexe regelmässig aufeinander folgender, gleicher Schalleindrücke, *Zsch. f. Psychol.*, 1, 1890, 75-80; *cf.*, 2, 1891, 115-119; 4, 1893, 234.

J. Quandt, Bewusstseinsumfang für regelmässig gegliederte Gesamtvorstellungen, *Psychol. Stud.*, 1, 1906, 137-172.

H. Lehmann, Aufmerksamkeitsumfang für sukzessive Lichtreize, *Psychol. Stud.*, 10, 1916, 260-264.

[3] Dallenbach, *op. cit.*, 228 f.

[4] Dallenbach, *op. cit.*, 229. Fernberger (*op. cit.*, 133) arrived at a similar conclusion: "From a consideration of the introspections," he says, "we are convinced that the range of attention is an erroneous title for this sort of experiment."

fact, as Fernberger pointed out, that different methods of computing the constants have been used. Methods of all degrees of accuracy and complexity but with no accepted principle underlying them. Because of this, Fernberger proposed that the so-called range of attention be determined according to the principle of the statistical limen, and defined as "that value of stimulus the sensing of which has a probability of 0.5," *i.e.* as frequently cognized correctly as not.[5]

PROBLEM

We are not at all certain whether the question of range is a proper one to ask regarding attention—defined as the state of consciousness divided into the clear and unclear—for the attentive consciousness is an integrated whole and from that point of view the rational psychologists were certainly correct in regarding the range of attention as one. The question, however, may still be asked, how many contents or part contents may be at the focus of attention, and the question thus stated is explicit and straightforward enough. What is demanded is the answer to the question, how many impressions may, in a single consciousness, be experienced at the upper level of attributive clearness; or to put it more concretely, what is the limen of the upper level of clearness; how many stimuli may be placed upon an exposure card before they are experienced 50 percent of the time at two or more levels of clearness. We may escape the cognitive complications of the earlier experiments by asking the *O*s to report, not upon the number or kind of stimuli observed, but upon the distribution of the attensity of their impressions.[6]

METHOD AND PROCEDURE

We used the simultaneous method, but instead of asking our *O*s to give the number of stimuli on the exposure card or to name them—both tasks implying a high degree of cognition—we instructed our *O*s to report merely whether their impressions were at one, two, or more levels of clearness.

Instructions. The following instructions were read to the *O*s at the beginning of every experimental hour:

"At the signal 'Ready,' fixate the black dot on the pre-exposure field. At 'Now' you will be shown a white field upon which there is a number of black figures. After the experiment report upon the clearness, that is the vividness of the impressions, during the exposure period. You may use the following terms: 'one' or 'equal,' meaning that all the impressions were at one level of clearness; 'two' or different,' meaning that two levels were observed; and 'three' or 'multiple,' meaning that three or more levels were observed." Doubtful judgments were permitted, but the experiments in which such reports were given were later, without *O*'s knowledge, repeated. No detailed introspections were called for but they were occasionally volunteered.

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[5] Fernberger, *op. cit.*, 122.

[6] E. B. Titchener, The term "attensity," this JOURNAL, 35, 1924, 156.

Observers. The observers were Dr. H. P. Weld (Wd), professor of psychology; Dr. A. K. Whitchurch (Wh), fellow in psychology; and the senior author (D). All were highly practiced in introspection and in the observation of attensity.

Materials. The exposure cards were presented by means of the Dodge tachistoscope,[7] the exposure time was 60σ. The exposure cards were 11.5 cm. long and 5.5 cm. wide—the size adapted to Dodge's instrument. For the experiment 130 cards were prepared which differed in the shape and number of stimuli. With respect to shape of stimulus, the cards were divided into 10 sets of 13. In Series A, B, and C the stimuli were circular; in Series A the circles were small, in B, medium sized, and in C, large; in Series D, semi-circles were used; in Series E, triangles; F, clubs; G, diamonds; H, squares, and in I, spades. The tenth series, Series J, was composed of forms haphazardly selected from among the nine. In exposure cards with 9 or less stimuli no single form was used more than once; in cards with 10 or more stimuli no single form was used more than twice. With respect to number, the 130 stimulus-cards were divided into 13 sets of 10. Cards with from 3 to 15 stimuli were prepared for every one of the 10 Form-Series (Series A-J). In the exposition which follows we shall, when we wish to designate the *shape* of the stimuli, refer to the cards serially by letter as Series A, B, C, etc.; and when we wish to designate the *number* of stimuli we shall refer to them by number as Series 3, 4, 5, etc. We have no Number-Series 1 and 2, as no cards with so few stimuli were prepared.[8] The cards were exposed in haphazard order—they were shuffled and given to the *O*s in the order drawn from the stack. All the series were repeated 10 times, making a total of 1300 observations for every *O*.

The cards were carefully prepared. In planning them we tried to eliminate all the factors except number which might influence clearness. We controlled the factor of quality by making all the figures black. We first attempted to print the figures with rubber stamps, but this proved unsatisfactory because the printed figures, even when the utmost care was used, varied in quality from print to print, and even within the single print. This we very soon discovered was important, for the slightest variation in the quality of any one of the figures would, perhaps due to novelty, set that figure at a different level of clearness. We finally solved the difficulty by using punches of the forms described and by cutting holes in the exposure cards. When these cards, thus prepared, were placed in the holder, which was painted a dead black, the surface showed through the punched holes giving a uniformly black and sharply defined figure.[9]

We attempted to eliminate the effect of position by distributing the figures over the card so that they occurred equally often at the left, center and right, and at the upper, middle and lower parts of the exposure field.

We tried also to eliminate the effect of pattern, for where grouping is possible, as has been shown in the earlier experiments on this problem, the range is greatly extended. We found after repeated efforts that it was impossible to arrange the figures so as to eliminate entirely subjective grouping; nevertheless, we hoped by altering the arrangements that gave obvious patterns, and by using a large number of different forms and a large number of exposure cards to reduce this factor to a minimum. We hoped also to be able by fractionating the data to tell when and where pattern and position were effective, and thus *a posteriori* to eliminate these conditions by omitting such data from the computation of the final results.

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[7] R. Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.

[8] We used 13 stimuli ranging in value from 3-15. We thought, in view of the traditional constant, 4-6, and Fernberger's limens, 6-10, that the critical range in our work would fall somewhere between our extremes.

[9] We are indebted to Dr. H. G. Bishop for the suggestion of this method.

RESULTS

The results of the experiment are shown in Table I, which gives the number of times the impressions were reported at one level of clearness. This table is the only one required to present the gross results, since the multi-level type of consciousness, in spite of the fact that such a consciousness was tacitly assumed and suggested in the instructions, was not reported a single time by any one of the *O*s. Reports of 'one' and 'two' only were given; and since they are complementary, a table showing the distribution and frequency of one kind of report would show the distribution and frequency of the other.

TABLE I

Showing the Number of Times for every *O* that the Impressions from the Various Cards and Series were Reported at One Level of Clearness; also the Average and m.v. of the Number of Times that the Different Form Series were Reported at One Level, and the Deviation of every Form from this Average.

O	FORM-SERIES	FORMS	NUMBER-SERIES													TOTAL FORM-SERIES	DEVIATION FROM AVERAGE
			3	4	5	6	7	8	9	10	11	12	13	14	15		
D	A	Small Circles	10	10	10	10	10	9	10	10	7	8	10	6	7	117	+ 4
	B	Medium Circles	10	10	10	10	10	8	6	6	8	3	5	9	8	103	−10
	C	Large Circles	10	8	10	10	9	10	9	10	5	7	9	6	6	109	− 4
	D	Semi-Circles	10	10	10	10	8	10	9	10	10	8	6	10	7	118	+ 5
	E	Triangles	10	10	10	10	9	10	10	7	9	8	6	8	8	115	+ 2
	F	Clubs	10	10	10	10	7	7	8	7	9	9	9	9	6	111	− 2
	G	Diamonds	10	10	10	9	8	10	4	10	8	9	8	8	7	111	− 2
	H	Squares	10	9	10	8	9	8	9	7	8	10	5	4	8	105	− 8
	I	Spades	10	10	10	10	10	8	10	9	9	9	8	9	6	118	+ 5
	J	Combinations	10	10	10	10	10	10	9	10	10	10	7	8	9	123	+10
TOTAL NUMBER-SERIES			100	97	100	97	90	90	84	86	83	81	73	77	72	Av. 113	m.v. ± 5.2
Wd	A	Small Circles	9	10	10	10	8	9	10	10	10	9	10	8	9	122	+ 5.8
	B	Medium Circles	9	10	10	10	10	10	10	9	10	8	10	9	10	125	+ 8.8
	C	Large Circles	10	10	10	10	10	10	10	10	10	10	8	10	10	128	+11.8
	D	Semi-Circles	10	9	10	10	10	10	10	10	10	9	9	9	10	126	+ 9.8
	E	Triangles	10	9	10	10	10	10	10	10	9	10	8	10	10	126	+ 9.8
	F	Clubs	9	9	9	9	10	9	9	8	9	10	10	9	10	120	+ 3.8
	G	Diamonds	9	10	10	10	10	10	9	8	10	10	9	9	10	124	+ 7.8
	H	Squares	10	10	10	8	10	10	10	9	8	9	9	9	8	120	+ 3.8
	I	Spades	10	10	10	9	10	10	10	10	10	10	9	9	9	126	+ 9.8
	J	Combinations	5	5	2	8	2	8	5	2	2	1	2	1	2	45	−71.2
TOTAL NUMBER-SERIES			91	92	91	94	90	96	93	86	88	86	84	83	88	Av. 116.2	m.v. ±14.2
Wh	A	Small Circles	7	9	9	8	8	7	7	4	9	6	9	7	7	97	− 7.2
	B	Medium Circles	10	9	9	9	5	9	9	4	0	6	4	9	3	86	−18.2
	C	Large Circles	8	10	7	9	10	5	5	4	9	6	4	2	6	85	−19.2
	D	Semi-Circles	9	10	10	10	10	10	10	10	8	8	8	8	8	119	+14.8
	E	Triangles	10	10	10	10	9	10	9	5	4	10	10	7	8	112	+ 7.8
	F	Clubs	10	10	10	10	5	10	8	8	6	9	4	5	7	102	− 2.2
	G	Diamonds	10	10	10	10	9	10	7	5	10	7	6	10	8	112	+ 7.8
	H	Squares	10	9	10	10	10	10	9	8	6	9	9	7	4	111	+ 6.8
	I	Spades	10	10	9	10	10	10	10	10	9	10	5	5	2	110	+ 5.8
	J	Combinations	10	9	10	9	10	10	8	9	10	4	3	7	9	108	+ 3.8
TOTAL NUMBER-SERIES			94	96	94	95	86	91	82	67	71	75	62	67	62	Av. 104.2	m.v. ± 9.3

Before turning to the exposition and discussion of the results presented in this table, we might point out that the law of two levels is again confirmed:[10] a clear focus and an obscure background were reported by every *O* in every observation. In the experiments in which the impressions from the exposure card were reported at the focus, certain obscure images and organic and kinaesthetic sensations formed the background; in the experiments in which the impressions from the cards were themselves divided into two levels of clearness, some were clear and at the focus, others obscure and in the background.

The frequencies of the one-level judgments are given in Table I for every *O* opposite the captions "Total Number-Series." These data show that the range of clearness decreases in general as the number of impressions increase. The decrease is small and irregular, and inversions of both first and second order occur. The inversions we believe are due to the fact that the stimulus values differed by too small amounts. Had stimuli been selected which differed by 5 steps instead of by 1, it is very probable that no inversions, at least none of the first order, would have occurred. When the results are smoothed by combining the series into 4 groups, as in Table II, the decrease is constant in its general trend and direction. It would thus appear that if we had greatly extended the number of stimuli we should have obtained the lower frequencies. We have treated the data upon that assumption. Regarding our frequencies, therefore, as a sector of the psychometric function, we have computed by the method of constant stimulus the theoretical liminal values of the one-level clearness reports. These values are shown in Table II. The limens of the one-level reports are 16.4, 17.3, and 35.3 for Wh, D and Wd respectively.

TABLE II

Showing, for every *O*, the Average Number of Times that One-level Clearness Reports were given for the Four Different Groups of Stimuli; also the Limens and Measures of Precision Obtained from these Data.

O	AVERAGE FOR SERIES				L	h
	3-6	7-9	10-12	13-15		
D	98.5	88.0	83.3	74.0	17.3	0.108
Wd	92.0	93.0	86.6	85.0	35.3	0.034
Wh	94.7	86.3	71.0	63.6	16.4	0.097

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[10] *Cf.*, E. B. Titchener, *Feeling and attention*, 1908, 220-242; K. M. Dallenbach, this JOURNAL, 24, 1913, 506-507; also *J. Exp. Psychol.* 3, 1920, 225-227.

We do not venture, however, to state that these values represent the ranges of our *O*s' attention, because it is evident from the variability of the reports in the different Form-Series that the data are heterogeneous—that some factor or factors besides number of stimuli are affecting the reports. What these factors are we may possibly determine, and thus eliminate, by studying the voluntarily offered introspections and the fractionated data.

It is evident from an inspection of the fractionated data in Table I that the reports in Series J and those in the other Series are, for Wd, of different orders. The total number of one-level reports given by Wd in Series J is 45; the total number of these reports in the other 9 series varies from 120-128 with an average of 124. From external evidence alone it is therefore clear that these data are heterogeneous—that Series J should not be included with the others; but we find other grounds for the exclusion. Wd reported in his introspections that "there are times when the judgment is difficult; at such time I am not at all sure that my attitude has always been constant." "The most difficult judgments of all are when the field is made up of impressions differing in form and size" (Series J). Wd was uncertain of his attitude in difficult judgments; the most difficult judgments occurred in Series J; whence it follows that he was most uncertain of his attitude in this series. The objective results are thus corroborated by the subjective. But we have still more certain grounds for the exclusion of the results of Series J from the computation of Wd's one-level limen.

Of the unusual conditions of clearness, all were excluded in the first 9 series, Series A-I, except position and pattern. In the tenth series, Series J, the conditions of form (9 forms were used) and size (the forms were of different sizes) were included. The effect of size and form is most clearly shown in the data of Wd, for his reports in Series J, as we have just seen, are not comparable with his reports in the other series. The discrepancy was correlated with an attitudinal shift, but the cause may be sought further; the shift in attitude may be an effect rather than a cause, for it is quite evident from Wd's introspections that the factors of size and form conditioned his reports in Series J. He says: "When the field was made up of impressions differing in size and in form, the larger impressions seemed to catch the attention—by that I mean they were the first ones seen. At times such figures 'stood out,' but whether this means more than a greater intensity or a clearer apprehension of the relative size I cannot say. As a rule, however, I reported such experiences as of two levels. The same may be said about form, when the difference is size was not so manifest. In these cases one form seemed to catch and hold the attention in much the same way as in the case of size." Because of this balance of evidence we have computed Wd's limen of one-level reports from the results of the first 9 series. Wd's revised data appear in Table III.

TABLE III

Showing the Revised Data, the Limens and the Measures of Precision.

O	Series omitted	One-Level Reports	NUMBER OF FIGURES ON EXPOSURE CARDS													L	h
			3	4	5	6	7	8	9	10	11	12	13	14	15		
D	B H J	Number	70	68	70	69	61	64	60	63	57	58	56	56	47	19.8	.081
		Percent	99%				88%			85%			76%				
Wd	J	Number	86	88	89	86	88	88	87	84	86	85	82	82	86	42.5	.036
		Percent	97%				97%			94%			93%				
Wh	B C D	Number	67	67	68	67	61	67	58	49	54	55	46	48	45	16.6	.118
		Percent	96%				89%			75%			66%				

With the exception of Series J, Wd's data show less variation than those of either of the other *O*s. The variations of D and Wh are more numerous and more difficult to explain. Moreover, neither of these *O*s gave such complete voluntary introspections; they gave no reports of attitude, neither did they mention the conditions of size and form. These omissions, since introspections were not required, are of course of little significance in themselves; but as the objective results stand we are led to believe that neither attitude, size nor form affected these *O*s. If these conditions were effective for D, they were at least effective in a different direction from what they were for Wd, as D reports one level more frequently for Series J than for any other. Wh, on the other hand, seems not to have been affected at all; her reports in Series J are of a kind with those in the other series. The variations of D and Wh must therefore be accounted for on other grounds. We have still to consider the conditions of position and pattern.

In order to ascertain whether position was effective we took the stimulus-cards of the Form- and Number-Series that gave the largest variations and examined them in order to see whether the distribution of the figures over the field could in any way be correlated with the reports. We could find no correlation nor any other evidence that position was effective. Our endeavor, in the preparation of the cards, to eliminate the effect of position was objectively successful at least. The same procedure was followed in studying the effect of pattern. We again took the cards that gave the largest variations and examined them to see if they differed in the tendency to fall into groups. We could find, however, no evidence that any of the *O*s were objectively influenced by pattern.

These findings might have been anticipated by a study of Table I, for if an objective condition, such as position or pattern, were responsible for the variations we should expect, since the exposure cards were common to all the *O*s and presented to them in exactly the same order, to find the same variations in the reports of all the *O*s. But such is not the case. None of the variations is common to all the *O*s; and few are common to any two of them. In Series B, 11 and 12, for example, the one-level report is given by Wh 0 and 6 times respectively, by Wd 10 and 8 times, and by D 8 and 3 times—a variation by Wh which is not parallelled in the reports of D or Wd. Similarly for the reports of the other *O*s. Such discrepancies indicate that the variations of the different *O*s are not due to objective causes, and that any attempt so to account for them would at the outset be unsuccessful.

The failure to discover objective causes merely means, we suppose, that the conditions of the variations are subjective, and that we should, if we had wished to discover them, have required after every experiment a complete introspective report. Lacking these, we are at a loss to account for the vagaries of our data. We may, nevertheless, without definitely knowing their cause eliminate them by omitting from consideration those Form-Series in which the total number of one-level judgments deviates from the average by an amount greater than the m.v. On this principle, which is based on the supposition that the series most heterogeneously determined would show the largest variation, the results of Series B, H and J have been omitted from D's data, the results of Series J from Wd's, and the results of Series B, C and D from Wh's data. The deviation from the average in these series is approximately twice the m.v. The data thus corrected and revised are given in Table III.

The limens computed on the basis of the revised data are 16.6, 19.8 and 42.5 for Wh, D and Wd respectively. They are slightly larger than the limens obtained from the crude data; and they should be larger, for the factors we sought to eliminate—and by these results apparently did in part eliminate—were factors which in addition to number conditioned the clearness reports. The more conditions of clearness operative in any given experience, the more likely are the part-contents of that experience to be at different levels of clearness. These limens, therefore, more nearly represent the range of the upper-level than the values given in Table II.

Wd's limen differs so greatly from the limens of the other 2 *O*s that it seems as though we were dealing in his case with a phenomenon of a different order. Since detailed introspections were omitted we cannot say whether this conjecture is true or whether Wd's range does in reality so greatly exceed the ranges of Wh and D. Such large variations among normal *O*s, however,

THE FACTOR OF MOVEMENT IN THE PRESENTATION OF ROTE MEMORY MATERIAL[1]

By CARL JOHN WARDEN, Columbia University

The attentional advantage of a moving stimulus over a stationary but otherwise similar stimulus is well known. Our knowledge rests upon a large body of experimental evidence. Movement is extensively used, for example, in advertising and in other branches of psychotechnology to attract and to hold the attention. In the present Study we attempt to determine, within certain limits, the influence of movement in the presentation of rote material upon memory.

In most laboratory experiments, movement of the stimulus presented for fixation is regarded as a distraction. Consequently, the standard exposure apparatuses are constructed to give a stationary presentation. In the tachistoscope, for example, the stimulus material at the moment of exposure is stationary, even though accompanied by unavoidable movements of the screen. So also in the various types of memory apparatuses, though the drum containing the exposure-material revolves it moves not continuously but step-wise and intermittently, thus also giving stationary presentations at the moments of exposure. Since we wished to study the effect of a moving stimulus on memory we had to devise and to construct our own apparatus.

Apparatus and Material. The exposure apparatus used in the present Study was relatively simple. An opening was cut in the partition between two laboratory rooms. Into this was fitted a ground-glass window 55 cm. wide and 48 cm. high. The lower sill of the window was approximately 30 in. from the floor. The glass was covered from behind with black cardboard. A horizontal exposure area, 55 cm. in width and 15 cm. in height, was left at the center of the window. The stimulus-cards were exposed through this opening.

The exposure materials, consisting of series of letters, digits and simple geometrical designs,[2] were painted in heavy white lines (7 mm. broad) on black cardboard cards of the size of the exposure area and similar in quality to the cardboard forming the background of the glass.

Series of 7, 8 and 9 letters; 8, 9 and 10 digits, and 2 and 3 geometrical designs were used. In the series of letters, consonants alone were used, and of these, J and Y were omitted. The letters were arranged in chance order and painted in connected vertical script. The one-space letters were 4.5 cm. in height. The digits were also painted in vertical script and of proportionate size. The digits 1 and 0 were omitted, otherwise the rules laid down by Reuther as summarized by Whipple[3] were followed in the construction of the digit series. The geometrical designs were each composed of 3 lines; 2 long and 1 short.

When the stimulus-cards were placed in the exposure area against the ground glass, the white lines appeared very similar to and fully as clear and distinct as chalk lines on a blackboard. The cards were manipulated from behind by an assistant of whose presence the *S*s were entirely unaware.

The window was covered in front with a roll curtain of neutral gray cloth. This was operated for the exposures by *E*. The time of exposure was 3 sec. for digits and letters, and 5 sec. for the geometrical designs. Timing was done with a stopwatch. Care was exercised in order to insure a uniform exposure time; the slight unavoidable variations probably followed the normal curve of distribution, so the exposure times were approximately equal for the different methods of procedure used.

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[1] The experiments here reported were performed in the Psychological Laboratory of the University of Nebraska.

[2] C. E. Seashore, *Elementary Experiments in Psychology*, 1909, 131.

[3] G. M. Whipple, *Manual of Menta land Physical Tests*, 1915, II, 154.

Procedure. Two methods of presentation were employed for the digits and letters. In Method A, the cards were placed in position before the curtain was raised and the entire series exposed simultaneously for 3 sec. In Method B, the assistant placed the cards in position with a blank cardboard (black) between the card containing the material to be memorized and the glass. When the blank strip was drawn slowly from in front of the exposure-card, the units of the series came into view in an orderly succession, each remaining exposed after having appeared. Although the operation was simple, the *S*s did not guess the mechanics of it. It appeared as if one unit was being added to another very much after the manner of the freak moving picture films. Under Method A, the series of stimuli were presented simultaneously and without movement; under Method B, they were presented successively and with movement.

In addition to Methods A and B, a third method, Method C, was used in presenting the geometrical figures. An assistant operating from behind the glass painted the designs directly upon the glass with a rapid continuous movement of the brush during the 5 sec. exposure time. The *S*s thus saw every design grow from a point to a completed whole, the factor of movement being emphasized even more than in the presentation of Method B. The designs were drawn by the assistant so the direction of motion was the same as the *S*s would make in drawing the designs themselves. The results for any given method were obtained at a single observation period at which 8 exposures were made in the following order: 8 digits, 9 digits, 10 digits, 7 letters, 8 letters, 9 letters, 2 designs, 3 designs.

Subjects. The experiments were given to 135 students from a second semester class in psychology. The group was composed mostly of juniors and seniors, and included about an equal number of men and women. The *S*s were tested 6 at a time. The experiments were given as a regular part of the laboratory work. The experiments proper were preceded by several weeks of practice in memorizing similar material using the same exposure apparatus, hence the *S*s were thoroughly familiar with the experimental conditions.

The *S*s were instructed to reproduced every series in the exact order given, indicating omissions by dashes. Reproduction was made immediately after the exposure. An interval of 30 sec. was allowed for this. Before every exposure the content of the series was stated and a "ready" signal given about 2 sec. before the screen was raised. An interval of 1 week intervened between the use of the 3 methods which were given in the order A, B, and C.

Results. The results are given in the accompanying Tables. The values of Tables I and II (letters and digits) represent the scores obtained for the different methods by the use of Whipple's modification of Spearman's "foot rule" formula for correlation.[4] This formula takes into account not only the omissions but all deviations from the order of exposure series which occur in the attempted reproduction. The values of Tables III are the percents. of designs correctly drawn. An error in the design might consist either in drawing a line in the wrong direction or in some distortion involving the relative length of the three lines composing each figure. No significant sex difference was discovered, hence the results for the men and women have not been separately reported.

A marked decrease in the score arising from increasing the length of the series occurred for both digits and letters. Series of digits scored higher than series of letters of equal length regardless of the method of presentation employed. In fact a series of 7 letters scored about the same as a series of

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[4] *Op. cit.*, 161.

8 digits, etc. This may be accounted for, in part at least, by the fact that the letters were connected which doubtless had the effect of decreasing their legibility.

Comparing the results for the two methods, a considerable difference is indicated in favor of Method B, this relationship holding consistently for all series of letters and digits regardless of length. The average score is nearly 5% higher for Method B for the digits and over 6% higher for the letters. A continuously moving successive presentation evidently has a measurable advantage over a purely simultaneous, stationary one for material of this sort when the memory value of a single presentation is measured by immediate reproduction.

TABLE I

Comparison of the Two Methods of Presentation of Series of Digits

Method	8 digits	9 digits	10 digits	Average
A	83.8	74.9	64.8	74.5
B	87.4	78.8	67.8	78.0

TABLE II

Comparison of the Two Methods of Presentation of Series and Letters

Method	7 letters	8 letters	9 letters	Average
A	81.5	76.0	57.8	69.1
B	83.3	80.9	64.5	76.2

TABLE III

Comparison of the Three Methods of Presentation of Geometrical Designs

Method	2 designs	3 designs	Average
A	66	45	56
B	68	47	58
C	80	60	70

A plausible explanation of the greater efficiency of Method B is suggested by the report of many of the *S*s to the effect that under Method A they would be in the midst of a second reading of the series when the curtain was drawn, while only a single but more intensive reading occurred under Method B. The moving into place of the units of the series under the latter method would likely have the effect of guiding the fixation during the exposure. There should be less tendency for the eyes to ramble back and forth over the material, and hence fewer reversions and displacements of the units in the reproduction, than in the simultaneous presentation. It is also probable that the moving series had the greater stimulation value because of added interest and a more constant attention.

Little or no significant difference was found between Methods A and B in presenting the designs, as will appear from the data of Table III. The series were very short compared to those of the letters and digits, and instead of a succession of units moving into view, there was only the gradual exposure of relatively large designs. The movement required in the reproduction of the form of these figures was altogether different from the sweep of the eye in observing the gradual exposure of a given design. In the simultaneous presentation, moreover, there would be less tendency for the eyes to wander, when only 2 or 3 figures were exposed, than when long series of from 7 to 10 units were exposed. Probably these and other unknown factors operated to decrease the advantage of Method B over Method A for this type of material.

It will be noted that Method C stands very much higher than the other two methods for the designs. The figures were painted upon the back of the glass in such wise that the sweep of the eyes in perception was in the same direction as the normal movement of the hand in reproducing the form. Everyone of the 3 lines of a given design was made by a single sweep of the brush. Every line as a part of the larger design emerged into being from a point, and the lines succeeded one another in the order in which most subjects would likely draw them. Movement within the presentation was thus strongly emphasized. The results show that this method of exposure is much superior to that of either Methods A or B. About 25% of the *S*s reported either movements of the eyes or head or incipient movements of the hand during the presentation, and this motor reaction of the *S*s during exposure may be in large part accountable for the greater efficiency of this method.

Whatever may be the explanation, the more the factor of movement is brought into the stimulus situation the more efficient the stimulation, so far as immediate reproduction under conditions here employed is a measure. The relative value of a continuously moving successive as over against a stationary simultaneous presentation of rote material doubtlessly varies with length of exposure, number of presentations, time of recall and other such important conditions, but these are moments that we have not as yet had an opportunity to consider.

APPARATUS FOR MEASURING CHANGES IN BODILY POSTURE[1]

By Samuel Renshaw and A. P. Weiss, Ohio State University

The apparatus described in the following pages and by the plates and drawings, forms part of a dissertation on "Apparatus and Experiments on the Postural Stability Component in Human Behavior." The reasons for publishing it separately are: (1) it is now being used for investigations which will not be a part of the dissertation; (2) its complicated character and the many details make it desirable to describe it "once for all" so it will be readily accessible to psychologists; (3) visiting psychologists who have seen it in operation have asked that it be described in sufficient detail so that it may be duplicated; and (4) the dissertation of which it is a part may not be ready for publication for some little time.

The apparatus is essentially a device for recording the changes in the center of mass of the body under various stimulating conditions. In this article the stimulating conditions were a pursuit task, although any other form of stimulation may be used. The apparatus as described yields records under the following conditions: (1) the subject may maintain a relatively free and easy posture while seated on the stabilometer; (2) the type of response (pursuit) is simple and requires very little original learning; (3) the response does not require continuous fixation and thus eye and other accommodatory strains are avoided; (4) the cycle of the task is long and involved but identical for all subjects. It requires constant effort and does not become "automatic;" (5) the movements of the subject are recorded on a paper strip in the form of curves varying in frequency, duration, amplitude and form; (6) the errors may be measured in any degree of complexity, varying from a mere difference in time between the passive and active series to a careful analysis of the amplitude and duration of the various movements of the subject.

The Stabilometer or Posturograph

The stabilometer or posturograph provides a means for determining the relative stability of the subject's posture while seated. It consists of a stationary triangular platform (A; IV)[2] each side of which is 57½ in. in length. The frame work of this platform is made of 2 x 6 in. lumber securely bolted together at the ends. Reinforcing stays of the same material 2 ft. apart, extend from the base apex-ward. The top of this frame is covered with pine boards ¾ in. thick. This frame rests on the floor and serves as the base for the moving parts of the instrument. Near the angles of this stationary platform, brass plates (IV) 5x5x⅛ in. are mounted to the top by countersunk flat-head wood screws. These are centered in line with the bisectors of the angles. The plates serve as the surfaces which support the ball bearings (1; IV) upon which a movable platform rests.

This platform (B; III, IV, V) is also a triangle. Each side is 51 in. long. It is constructed of a 2 x 4 in. cypress framework bolted together at each angle. On the under surface of each angle is screwed a wood support 6x6 x 1 in. on which in turn is mounted the companion brass plate (IV) 5 x 5 x ⅛ in. forming the other bearing surface for the steel ball bearing (1; IV) 1¼ in. in diam. Fitted to the angle bolt (2; IV) is a brass hasp (3; IV) to which a spring (4; IV) made of No. 13 piano wire (0.072 in. diam.) is fastened. These springs are 2½ in. long and are carefully matched. The inner end of each spring is attached to a brass machine screw (6; IV), which in turn passes through holes in an anchor post (5; IV). The tension on the spring is regulated by the wing nut (6; IV). After the tension is regulated two lock nuts (not shown) fix the position of the screw (6; IV). The floor of the top of the platform (B; III) is made of pine boards ¾ in. in thickness, nailed to 1 x 2 in. wood strips which give a snug fit over the movable triangular frame. Thus the springs and spring tension adjustors, bearing plates, bearings and anchor posts are made readily accessible by lifting off the floor (B; III).

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[1] This apparatus was completed after many trials and failures. At times all the technical talent of the psychological laboratory was focused on one or more of the various difficulties that were encountered. The task of assigning proportional credit to different individuals is impossible. Without attempting this, we wish to acknowledge our indebtedness to Dr. C. N. Rexroad, Mr. A. Lee Henderson, and Mr. G. E. Weigand. To Dean G. F. Arps we owe much for making available the funds for materials and technical assistance.

[2] The references to the plates accompanying the article are of the following form. Every detail is given a number and these numbers are identical when they appear on more than one plate. In refering to a detail its number is given in arabic numerals and the plates in Roman, all in parentheses. Thus (44; I, II, V) refers to the pendulum timer shown on plates I, II, and V. The larger units are given Roman letters, as A, B, C, instead of arabic numerals.

A cane seated chair (20; III, V) with back removed and suitably braced is mounted in snug fitting receptacles in the center of the floor (B; III) of the moving platform.

The tension on the three springs (4; IV) must be kept about equal to avoid distortions in the movements of the subject, and to return the platform to the center of the stationary platform when the subject is not moving. The sensitivity of the moving platform (B; III, IV) will depend on the tension of the springs (4; IV).

A platform 72 x 72 in. square, supported by 4 in. square posts at the corners, stands 14 in. high and flush with the top of the movable platform (B). In the center of the square platform is a triangular opening which is larger than the triangular floor on top of the triangular frame (B). This permits a motion of the movable platform, in any direction up to 1¼ in. The wood hasps (22, 23; III) and iron pins serve to stablize the moving platform while the subject is being seated. Any movement of the subject on the stabilometer will engage one or more of the springs (4; IV). The return to the zero or neutral position will be oscillatory in character. In general it may be said that the platform is very sensitive in the right-left and forward-backward directions, but less well adapted to rotary movements of the subject. A record of the shifts of the subject seated on the chair (20; III) is transferred to the recording unit (C; II, III, V) by the mechanical transmission of the radial and circular movements of the platform (B; III) through a system of braided silk fish lines (24a, 24b, 24c; III, V). One line (24c; III, V) is attached to a short lever (21; III, V) and passes over a guide pulley (29; III, V) and is fastened to another line (24b; III, V) one end of which is attached to the subject's chair (20; III, V) and (24a; III, V) leads to the smaller pulley of a wheel and axle pulley lever (25; III, V). The diameters of the two pulleys are 3 and 18 cm. respectively, giving a magnification of 6:1. The ends of the lines (24a, 25a; III) pass over their respective pulleys to the counterweights (26, 27; III) by which the tension on the lines may be adjusted to balance the effect of the tension of the rubber band (37; II, III, V) and since they are snubbed once around their respective pulleys, the position of the recording pen (39: II) can be easily controlled. The lines and the grooves of the pulleys are occasionally treated with a saturated solution of rosin in turpentine to prevent slipping. From the larger pulley (25; III, V) the line (25a; III) passes to the middle bar (33; III, V) which carries the writing pen (39; II) known commercially as the Inkograph No. 11. A light rubber band (37; II, III, V) keeps the pen at about the middle of the paper tape when the subject is motionless.

The Pursuitmeter

The essential parts of the pursuitmeter are assembled on a 26 x 42 in. heavy maple kitchen table 37 in. high. with an extension leaf 16 x 42 in.

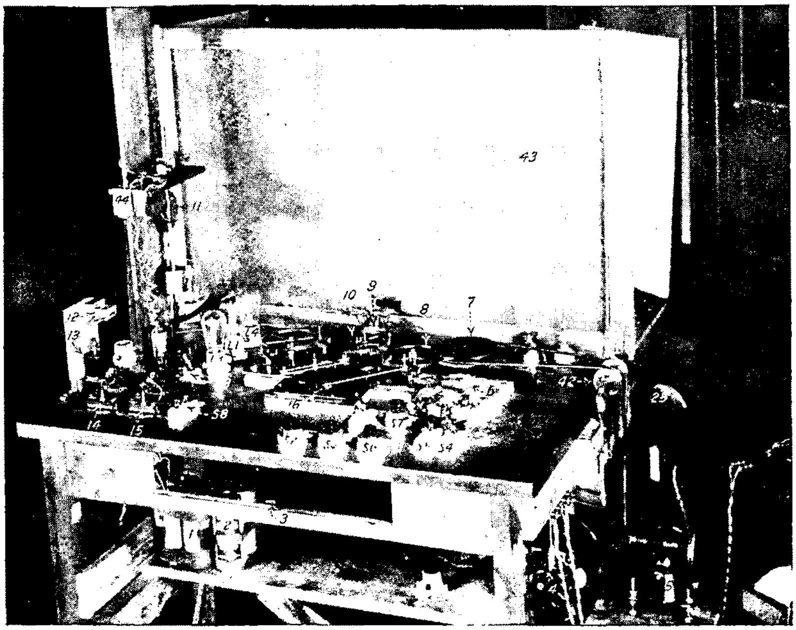

PLATE I

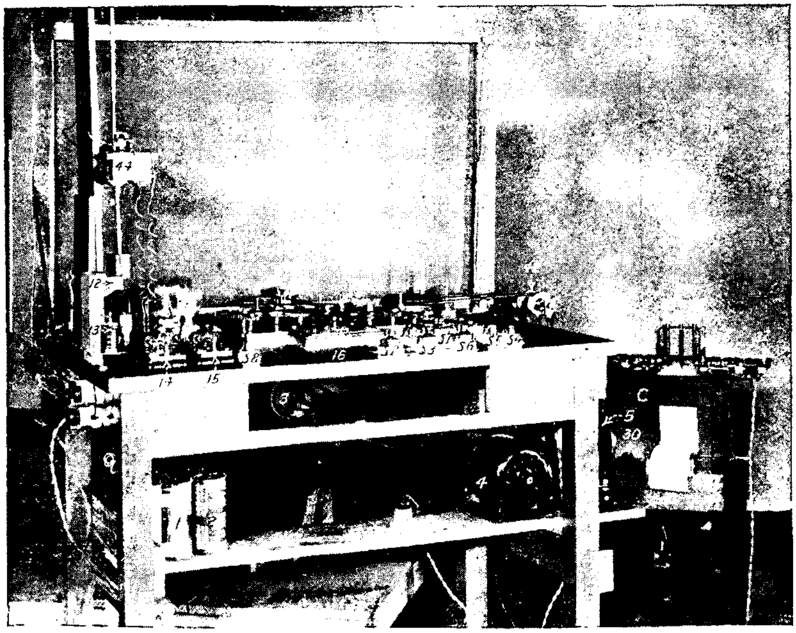

PLATE II

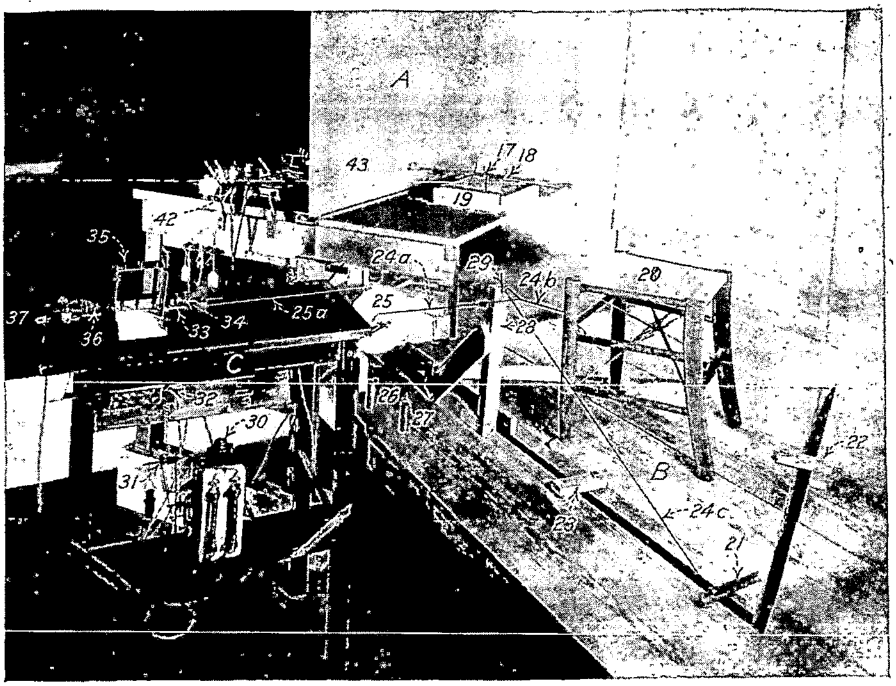

Plate III

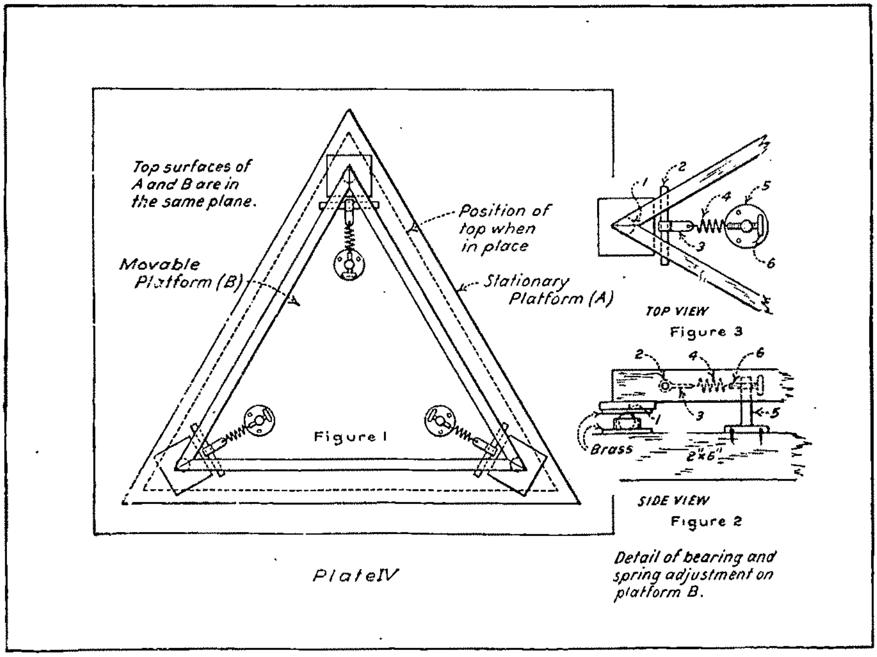
Top surfaces of A and B are in the same plane.
Position of top when in place
Movable Platform (B)
Stationary Platform (A)
Figure I
1
2
3
4
5
6
TOP VIEW
Figure 3
2
4
6
1
3
5
Brass
2"x6"
SIDE VIEW
Figure 2
Detail of bearing and spring adjustment on platform B.
PlateIV

Plate IV

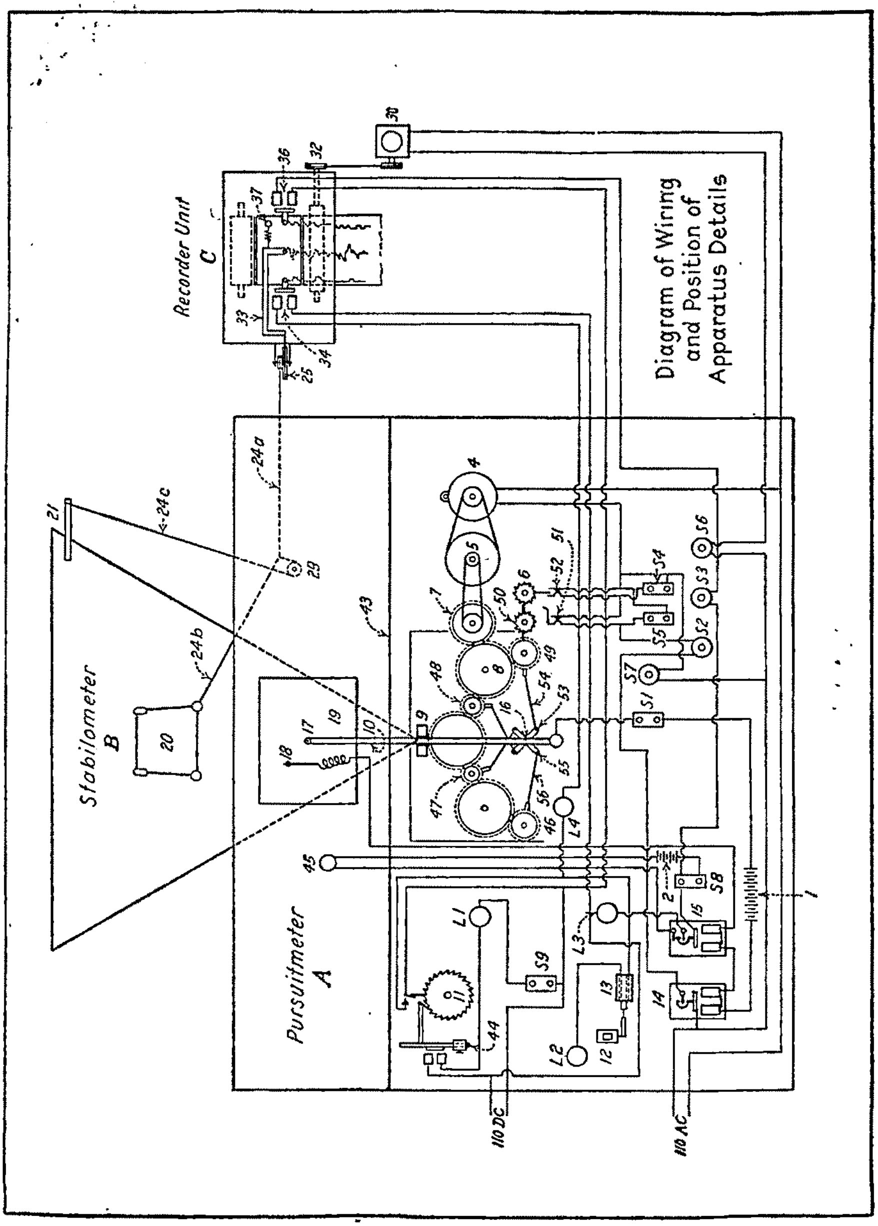
Stabilometer
B
Pursuitmeter
A
Recorder Unit
C
Diagram of Wiring
and Position of
Apparatus Details
110 DC
110 AC
L1
L2
L3
L4
S1
S2
S3
S4
S5
S6
S7
S8
S9

PLATE V

which carries the subject's part of the apparatus (17, 18, 19; III, V). Screens (43; I, II, III) hide the working parts from the subject.

The task of the subject is to keep an electric terminal (18; III, V) located in a hammer-shaped stylus, in contact with another terminal called the "bead" (17; III, V) carried at the extremity of the bead-bar (10; V). In operation the bead-bar moves about continually through a diamond shaped area about 4 x 5 in. on the glass plate (19; III, V). The bead-bar continues to move as long as contact between stylus and bead is maintained. When contact is lost the bead-bar decelerates and comes to a complete stop in 2.5 sec. unless the subject reestablishes contact. The stylus (18; III, V) is made of a piece of round red fiber rod ⅜ in. in diam. and 8 in. long. The head of the stylus is a piece of round red fiber ¼ in. in diam. carrying a 2 mm. iron wire electrode through the center. The face of the stylus and the head are slightly convex allowing about 18° of inclination from the horizontal before contact is broken. The stylus connects with a local 12 volt battery circuit through a flexible litz wire. Either right or left-hand subjects may operate the instrument with equal facility. The bead (17; III, V) is made of a short length of red fiber tubing ¾ in. long and ⅜ in. diam. The inside of the tubing is threaded and the end of the screw which attaches it to the bead-bar is made flush with the convex surface of the upper face. The bead electrode is 2.5 mm. in diam. Actual operation makes the contact-points self-cleaning. One-half inch in front of the bead a brass ball-foot support is mounted on the surface of the bead bar. This support slides in a thin film of castor oil over the surface of the 8 x 10 in. double strength window glass plate (19; III, V).

The driving mechanism of the bead-bar (10; I) consists of four small gears (46, 47, 48, 49; V) which mesh with the intermediate gears (8; V). All gears are standard stock brass of 16 pitch and are sold by the Boston Gear Works, Quincy, Mass. The gears (46, 49; V) have a pitch diameter of 1⅜ in. and 22 teeth. Gear (47; V) has pitch diameter of ⅞ in. and 14 teeth. Gear (48; V) has pitch diameter 1⅛ in. and 18 teeth. Connecting gears (8; V) have 5 in. pitch diameter and 80 teeth. The crank arms on the gears (46, 47, 48, 49; V) have 1½ in. radii. The parallel connecting rods (54, 56; V) are 6½ in. from center to center of the crank pins. The double toggle levers (53, 55; V) are 5 in. from center to center of the connecting rod bearings. A pin (16; V) passing through the center of the toggle levers and through the bead-bar (10; V) transmits the irregular motion to the bead-bar. A triangular stabilizing bearing (16; I) moves over a brass plate.

A driving gear (7; V) mounted under a wooden pulley (7; I) has a pitch diameter of 3½ in. and 56 teeth. The wooden pulley is 6 in. in diam., and is driven by a round leather belt which passes through reducing pulleys (4, 5; V) over guide pulleys (42; II, III) to a worm-wheel speed reducer (5; II) not shown in detail. Another reducing pulley (3; II) finally connects with the 1/10 H. P. A. C. motor 3400 R. P. M. (4; II). The reduction is such that the bead-bar driving gear (49; V) makes one revolution in 3.63 sec.

Since the common divisor of 22, 18, 14 and 22 is 2 the ratios of the teeth in the gears (49, 48, 47, 46; V) is 11, 9, 7, 11. As the two 22 tooth gears move in unison, the cycle of the toggle lever and bead-bar system will be the product of 7 × 9 or 63. Thus after 63 turns of the 22 tooth gears (49, 46; V) the series of irregular movements of the bead-bar and consequently the pursuit bead (17; V) will repeat itself. A pivot bearing (9; V) supports the bead-bar (10; V) and allows free play in the forward-backward direction, but only rotation in the right-left direction.

In order to present every subject with the same series of movements of the bead (17; V), gear (49; V) carries a pin in the rim of its under surface. At every revolution this engages a tooth of a 21 tooth star-wheel (50; V). This wheel, in turn, for every complete revolution, advances a 12 tooth star-wheel (6; I, V) one tooth. Thus one complete turn of this star-wheel requires 252 turns of gear (49; V). The total time for one complete revolution of the second star-wheel (6; I, V) is 15.24 min. at the uninterrupted rate of 3.63 sec. per revolution of gear (49; V). From the under surface of the second star-wheel (6; V) a short arm engages the circuit breakers (51, 52; V) in series with the main 110 volt A. C. line to the motor (4; II, V). Two knife-switches (S4, S5; I, V) are wired to the circuit breakers. When both S4 and S5 are open and the main switch to the motor is closed the apparatus will automatically run to the starting position and then stop. If S4 is closed and S5 is left open the apparatus will run one complete cycle (3.81 min.) and then stop—this is the practice series and represents 63 turns of the gear (49; IV). If S4 is closed and S5 opened the remaining 3 cycles (11.43 min.) are run and the system will automatically stop with all gears, cranks, etc., in the original starting position. Thus every subject responds to the same stimulus pattern. If a longer series is desired both switches S4 and S5 are closed and the experimenter reads the time in minutes directly from the Veeder counter (12; I, V) stopping the series at his option by opening (51; I, V) which breaks the local battery stylus-bead circuit through relay (15; I, V) to the motor (4; II, V).

The manner of operating the instrument and the switch controls may be described as follows (All references are to Plate V). First, S2 is closed; this closes the main 110 v. A. C. line back of A. C. Relay (14). (See that S7 is open. This is an emergency cutout for circuit running the motor (4) and is always left open, except when the experimenter wishes to run the driver motor independently). S3 is closed, this closes the main 110 v. D. C. line back of D. C. Relay (15). S4 and S5 are set, as described before, for the running of the appropriate practice or test series. Ratchet wheel (11) of the pendulum timer is set about 10 teeth back of its contact, S9 is closed energizing the magnets of the pendulum (44) the bob of which is given a gentle forward swing, thus starting the timing device, which at its contact each minute (*a*) energizes the timer magnet (34) of the recorder unit C and (*b*) energizes the solenoid (13) which operates the Veeder Counter (12). This counter keeps constantly before the eyes of the experimenter the exact number of minutes that have elapsed from the start of the series. S6 is closed, starting the constant speed driver motor (30) which feeds the paper tape through the recorder unit. S1 is closed, placing the control relays at the disposal of the subject's stylus-bead 12 volt local D. C. circuit. S8 is finally closed which energizes the buzzer magnet through the 4 volt local battery circuit. The sound of this buzzer (45) is the subject's signal to make contact between the stylus (18) and bead (17). As soon as he does this, A. C. Relay (14) throws on, and motor (4) starts, moving bead-arm (10). It also throws on D. C. Relay (15), in series with Relay (14) which simultaneously energizes the contact recorder magnet (36). When the subject, through inadvertence or inaccuracy of timing of his movements, breaks his contact through the stylus-bead circuit, A. C. Relay (14) throws off, breaking the main 110 volt A. C. circuit through the driver motor (4) and simultaneously, D. C. Relay (15) throws off, breaking the 110 volt D. C. circuit through the magnet (36) of the recording unit. The spring on the armature of this relay pulls the armature contact back against the copper contact (replacing the ivory buffer) of the lead which makes through the 4 volt battery and energizes the buzzer magnets—thus warning the subject that he is "off contact."

To stop the instrument by removing it from the subject's control it is only necessary to open S1, the local stylus-bead circuit. The series timer, (6), however, takes care of this automatically, and once the apparatus is started it requires no further attention. This instrument has caused no trouble in more than 120 hrs. of use.

The driver motor used is an Emerson, 1/10 H. P., 60 cycle, 110 volt, A. C., 3400 R. P. M. This motor has given excellent service for this type of work. As many as from 15 to 80 breaks per min. in its full 110 v. current supply in continuous runs of 2½ hrs. failed to heat it to the danger point. The constant speed motor (30) used to drive the paper tape rolls of the recorder unit is a special motor manufactured by the Emerson Electric Company, for Leeds and Northrup. It operates on 110 v., 60 cycle A. C. current. The main shaft of the motor, driven through a fiber worm gear runs at 55 R. P. M. Two reductions reduce this to a speed of about 4 turns of the platen paper-feed roll of the recorder per min.

The pendulum unit is fully described in the Psychological Review, 23, 1916, 508-516. The accessory ratchet and pawl timing device (11; I, V) is a star wheel of 60 teeth which moves one tooth per sec. per swing of the iron bob. Once each revolution (each minute) a silver contact on the rear face of the wheel makes the D. C. circuit through timer magnet (34; II, III, V) of the recorder unit. Proper resistances were used in all circuits employing 110 volts.

Some of the characteristics of this pursuitmeter are: (1) Its operation is almost automatic. It requires practically no attention from the experimenter except at the beginning and end of a sitting. (2) The stimulus pattern may be increased in complexity almost to infinity by selecting larger ratios of the 4 small gears, which may be kept at hand and changed very quickly. One small cotter pin is the only adjustment needed for a change of a gear. (3) The size of the field of transit of the bead and in part, its speed, may be regulated by the location of the two-way pivot (9). (4) The 4 x 5 in. field of the instrument subtends a rather small visual angle. It is not necessary or even efficacious for the subject to keep the moving bead constantly in foveal vision. The best performers usually learn to rely upon cutaneous pressures and kinaesthesis for following the transit of the bead. No eyestrain was reported by subjects working continuously from 1 to 2½ hrs. (5) The inertia of the weighted armature of the motor decelerates the bead movement at a rate such that if any subject is reasonably alert his task is truly continuous. Even though he loses contact, he may make it again if he tries quickly before a complete stop. The series is not discrete. (6) The auditory stimulus of the buzzer and the decelerating motor not only warn the subject, but often serve as an "annoyer" to subjects of a particular category. Others may quickly "get adapted" to it, while still others remain indifferent to it throughout the series. (7) The task looks much easier than it really is, although the coördinations involved are simple and are assumed by most persons to be already a part of the reactor's habit system. The wide differences we have found indicate, however, this is not strictly true. (8) The subject never is able to predict in what direction and at what rate the bead will turn next. The more he tries to "anticipate," the more he finds himself in trouble of his own making. (9) The entire instrument is constructed of standard parts—even the gears are "stock" gears—readily assembled in any good laboratory at a comparatively small cost.

The Recorder Unit

Since Ludwig's first use of the kymograph, this instrument has been a favorite recording tool, used by physiologists and psychologists. Within limits it gives good results. Beyond these limits, such as when the record becomes very long, more than 10 or 15 feet, the problem of handling, sorting, and shellacing becomes a big one. Paper-tape has been used with varying degrees of success. The advantages of the tape are manifest; the difficulties of obtaining satisfactory inditing upon the tape by some form of ink pen has been the most perplexing problem.

We present here a device which goes a long way to overcome these difficulties. In more than 120 hrs. of running, often as much as 7 or 8 hrs. per day, not one inch of the triple line record has given trouble—missed, blotted or required tinkering or adjustment. The only limitations to the instrument, since it uses both magnetic and mechanical means of moving the ink pens, are the length of paper in the roll and the amount of ink stored in the ink sacs of the pens.

The table on which the unit is mounted measures 16 x 25 in. and stands 31 in. high. The top is made of two pieces, one of which carries the recording mechanism. This consists of a bridge made of a brass plate whose sides are in the form of an H. Through slots in a brass plate at the lower opening of the H pass the levers, connected with the armatures of the timer (34; V, II) and pursuitmeter (36; II, III, V) recorder magnets, which move the pens (38, 40; II). The light aluminum bar (33; V) moves independently in a parallel slot of its own and is connected at one end to the cone pulley (25; III, V) and stabilometer (B), and at the other end to a light rubber-band spring (37; V). As previously mentioned "Inkograph No. 11" stylographic pens are used (38, 39, 40; II) in the vertical plane through holes in the bridge-roof which gives them a clearance of about 2 mm. The writing points pass through small cupped holes in sheet metal stripes attached to the brass lever arms connected to the magnet armatures (34, 36; II, V) and to the aluminum bar (33; V) and thus the pens "ride freely" on the tape of their own weight. Sanford's blue-black fountain pen ink was used in the Inkograph pens. The paper tape used is a standard 3½ in. cash-register tape of newsprint grade. It is supplied by most stationers in rolls weighing on the average, about 415 gms. per roll and containing about 235 feet of paper.

The tape is placed on a "rover" spindle and the end threaded through the brass recorder bridge beneath the writing levers. It then passes through the tractor device (41; II) at the front. This consists of a 1½ in. strap iron frame which carries two rubber rolls; a driver roller, made of a discarded typewriter platen 3¾ in. long x 1½ in. diam,. which runs in a slot bearing, and is held by two springs in contact with the driver. About three-fourths of the surface of the platen are in contact with the tape. The idler roller is made of wood and covered with soft para rubber tubing. This device prevents any slipping or skipping of the tape, since very little traction is required to draw it through the recorder bridge. The tape moves at the rate of approximately 11 in. per min. A system of two brass chains running from the sprocket (of ⅛ in. face), mounted on the platen axle, to the countershaft reducer sprockets below, (31; III) and thence to constant speed motor sprockets (30; III, V), furnish the power for pulling the tape past the recording pens at a highly constant rate. The motor (30) has a centrifugal governor and is said to be constant in speed to one percent. Tension on the brass chains is kept constant by weighting the countershaft (31; III).

Simplicity, portability and freedom from need for tending, adjustments, etc., are features which commend this instrument. We are now working on the construction of an 8 pen recorder of the same type, which will use a much wider tape and permit 8 simultaneous variables to be recorded. With this type of instrument continuous records may be taken for many hours or even days without a pause. It offers many advantages to the experimental psychologist over the kymographic method of recording. These records come from the instrument complete, permanent and ready for counting.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE.

LIII. INDIVIDUAL DIFFERENCES IN THE SENSE OF HUMOR[1]

By POLYXENIE KAMBOUROPOULOU

I. Discovery of Types.

(1) *Method of Humor Diaries.* The aim of this investigation was to find out if possible, whether certain individuals laugh at certain situations, rather than at certain other situations, consistently enough to be grouped into types.

Since a group of college students of the same sex is as homogenous a group in experience and environment as can be found anywhere, their differences, if any became apparent, would be due largely to native individual makeup, rather than to national or social inheritance or training. To supply material for classification, the students of a class in Introductory Psychology, all young women, were asked to keep 'humor diaries' for a week, recording every day the things they had laughed at that day. They were not asked to explain, analyze, or classify in any way these instances of laughter, though some of them did so of their own accord. The dates of the diaries were either seven successive days anywhere from December 10th to December 20th, that is, just before the Christmas recess, or seven successive days shortly after the recess. It was requested that the diaries should not be kept in a vacation week, as it was desirable that the diary authors should have, as far as possible, equal chances for laughter and amusement. Seventy humor diaries came in, and the first task was to analyze and classify their contents.

There were differences noticed before the classification was made. One was the *varying length* of the diaries, the shortest containing eight instances of laughter, and the longest one hundred and nineteen instances in the week. Most of the diaries contained from fifteen to twenty-one instances; the average for the group of seventy is 30.1.

(2) *Relation of number of instances to the general ability of the authors.* (a) *Measured by academic success.* The seventy authors were arranged in the order of the number of instances of laughter reported. They were also arranged in order of their 'credit ratios', a measure of academic success obtained by dividing the sum of a student's credits, in which the mark A counted five points, B three, C two, and D one point, per semester hour, by the sum of semester hours completed. A rank difference correlation between the two arrays was found to be +.20, P. E. 0.07. The average credit ratio for the whole group was 2.70; the average for those who gave more than 30 instances of laughter was 2.86; the average for those with 30 or fewer instances was 2.58. The student with the longest diary had a credit ratio of 4.08; the student with the shortest diary had a credit ratio of 2.08. On the other hand a student with a credit ratio of 4.27 had a diary with only 12 instances of laughter, though these were in the form of stories and very well written. We may say that there is some tendency for the better students to produce the longer diaries; such a tendency may well be due to conscientiousness.

(b) *Measured by Psychological (Thorndike) tests.* A rank-difference correlation coefficient was also found between the rank of the authors in length of diaries and their rank in their Freshman psychological tests; it proved to be only +.14, P. E. 0.08.

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[1] Acknowledgment is made of suggestions on problem and method from Professor Washburn.

(3) *Classification of Sources of Laughter.* Another difference noted before classification of the material was attempted was in the style of the diaries. The consideration of style did not enter into this experiment at all; but in the classification into types it offered great difficulty. Where an instance of laughter, or a joke, was merely stated, it could be more easily classified, though there was always the possibility of two persons laughing at the same thing for different reasons, which reasons could not be known when a joke was only briefly stated. On the other hand, where events were fully written up, they often belonged to two or more types at the same time, or progressively changed their type, if they were stories. This may be a large source of error in the determination of the types, particularly as my analysis was not tested by combined opinion; that is, different people might have classified the same events under different types. Where the diary-authors were analytical, as a few of them were, they stated their own motivation, and their diaries were easier to classify.

In the first attempt at classification, it seemed as if every single event belonged to a division of its own. After a process of elimination and condensation, the following six divisions, or types, were decided upon:

(*a*) Instances of laughter where there is no objective cause, no humorous event or situation to arouse it; laughter because of the pleasant weather, or because of the company, or because everyone else was laughing. Here are included a few instances of nervous laughter, instances where the diary-author acknowledges that she does not know what she was laughing at, or where she acknowledges the situation not to have been humorous, and instances where she remembers that she laughed a great deal, but does not remember the cause. These last were put here on the supposition that there was more laughter than amusement. Also instances where laughter was used as a personal defense; not where the defense prompted a clever retort, but where there was only laughter to conceal or make up for irritation. These were put together as being purely subjective and emotional, and not depending on an external humorous situation. They are really instances of *laughter without humor*.

The following are examples of (*a*) quoted from various diaries:

1. "C. and I went for a slushy walk; but the air was positively spring-like and we abandoned the muddy, wet road for the drier, more romantic but tippier top of the stone wall, and were really highly amused at each other and with ourselves. We took a long walk and giggled lots. It wasn't until we were almost back at college that we became serious and started to settle the affairs of the world. I am really afraid I am becoming a chronic giggler, but haven't quite reached the point where I am not able to be serious when necessary."

2. "We got so keyed up about deciding about the dogs (previous game) that we couldn't calm down in time for Chapel, and enjoyed ourselves at Chapel more than we should have."

3. "Other remarks were made at the time which amused me, but none of them made a definite enough impression upon me that I can remember them."

4. "I had to tell my room-mate that her uncle died; it was the first time I did such a thing. I practiced different ways of telling her, but every time I came to the door, I giggled and could not go in."

5. "We played cards the whole evening and laughed ourselves hoarse. Now I come to write it I cannot think of a single funny remark, but we laughed at the time."

(*b*) Instances of laughter which have an objective cause, but where this cause is a physical, not a mental one. Instances where people fall down, or are physically awkward or ugly and are laughed at, are probably examples of superiority laughter, but they do not belong to the world of ideas; they are physical causes. Other physical causes, such as sudden noises and some accidents, do not owe their funniness to any feeling of superiority consciously or unconsciously aroused, but rather to the unusual presented to the senses. These also were included here. Also all "slap-stick" was considered as belonging here, though in motivation it may partake either of superiority or of the surprise of the unusual to the senses, or of both. This was a class difficult to limit.

Examples of (*b*):

1. "Girls trying to sing a song made up on the spur of the moment. They were laughing themselves, were off the key, and almost broke down in places. Of course, the rest of us laughed."

2. "Little man walking through puddles. He had very big feet and suggested a machine on flat-boats."

3. "Girl trying to learn to swim."

4. "At luncheon-table several of the girls nearly upset dishes,—one the bread plate, another, a glass of water, and still another something else. Finally, when the third thing happened, we all began to laugh, wondering what would happen next.

5. "She said that on the evening that these two men were there, they were listening on the radio, and that the expression on the faces of the two of them had made the other two laugh. I was given an example of some of the expressions that had caused the amusement at the time, and I found that anyone would have been justified at laughing at such contortions."

6. "We had a Christmas party in the dining-room. Two of the girls tried the stunt of hooking the ends of the candy canes together and pulling them. We all laughed when one of them succeeded in pulling the whole cane from the other's hand."

7. "Was showing F. our horrid trick in Chapel. You wet one finger with your tongue, then rub the other on somebody's nose. Of course the other person thinks you are touching them with your wet finger. We made considerable commotion. F. was shocked, and Prexy watched us from the pulpit."

8. "In the padding about the Gym., which covers a very large area, is a little tiny dark hole, in which are the buttons for the electric light. They look like little eyes peering out."

(*c*) Instances of laughter where the objective cause is the mental inferiority of another person: stupidity, mistakes, ignorance, simplicity, social breaks, blunders, absent-mindedness, naïve remarks and remarks of children—except where the last create a funny situation and are not ludicrous because of their stupidity.

In all these, the inferiority of the person laughed at is obvious, and the laugher knows that she is laughing *at* somebody from her own superior position, though she does not analyze her feelings. Laughing at one's own foolishness is also included here; it is a superior self that laughs at an inferior one, recognizing the inferiority.

There were so many of these instances that attempts were made to subdivide them. A subdivision according to the motivation would have been the most desirable; namely, whether they were kindly humorous or cruelly so, or with a conscious satisfaction at the other person's inferiority. However, it was impossible to determine this, except in very few cases. Another attempt at subdivision was based on whether or not the ridiculous person had been previously felt as superior to the laugher. This also proved impossible, as the only recognizable previously superior people were the members of the faculty. So these instances had to remain under the general head of laughter at inferiority.

Examples of (*c*):

1. "Professor White was telling us that he had a good joke happen in his Freshman class the day before. He asked, 'What is the sine of zero?' The girl hesitated, then said, 'Do you mean zero degrees, or plain zero?' "

2. "After being in church, one little boy was afraid to go again. No one could imagine what the trouble was for a long time, until it was dis-

covered that he had been frightened by the words, 'The zeal of Thine House hath eaten me up.' He thought that 'zeal' was an animal."

3. "I noticed on the door of the room of an extremely unpopular girl the following sign, 'This room is not open to visitors.' "

4. "My room-mate had some leather novelties for sale, and one of the girls came in to look at them, making the following remark, 'My sister's rather dippy. What shall I get for her?' "

5. "K.—'Someone wanted to know you, but I forget who it is.' A few seconds later—'I had a weird dream last night, but I don't remember it.' "

6. "I was amused by a story I heard of a girl here and her prejudices. She was discussing sectarian and non-sectarian schools with some friends. 'Oh,' she said, 'you see I am a Unitarian and Mother wouldn't let me go to that school because it was so narrow. There were no Unitarians there, and Unitarians are the most broad-minded people there are.' "

(*d*) This is on the same basis as (*c*); that is, caused by the inferiority of another person; it differs from (*c*) in that here it is by a personally directed answer, a directed witty remark, or teasing, that the other person is *made* inferior. It is Bain's active "degradation of some person or interest possessing dignity in circumstances that excite no other strong emotion."[2] To have Bain's definition fit this type perfectly, we should leave out 'interest.' In this class I have not included the belittlings of ideas which have no people back of them. (The question of the possibility of undirected wit will be discussed further on.) Here too, as in (*c*), it was impossible in most instances to discover whether the teasing was kindly or cruel. The few openly satirical or ironical remarks are also included here.

Examples of (*d*):

1. "A Freshman told me this tale of woe: She fell down the stairs and a sympathetic witness ran up with the usual question, 'Are you hurt?' 'I broke my neck!' replied the Freshman. 'Oh!' said the witness, 'isn't it nice you have enough pride to hold your head up!' "

2. "Dr. MacCracken in his lecture said he was very glad to see that the college girl was so uninterested in food, and so interested in acquiring knowledge, that she would not leave a lecture until twenty minutes after the whistle blew! He was referring to Mr. Andrews' lecture of the afternoon before."

3. "Miss ———: 'How many know the definitions? Will those who know the definitions raise their hands?' After a long pause a few of us raised our hands, and Miss ——— took down our names. Then she said; 'All those who raised their hands will get B's. The others will get D's,—or would you rather have E's? I will not call on you for the definitions!' "

4. "At lunch we decided to complain of Miss ——— to the curriculum committee. Then we decided it would be more truly Christian to raise a fund and send her to Teachers' College."

5. "A friend tells me of a problem in mechanics, about a clothesline and a boy hanging in the middle. Professor White told them the boy was put in to divert their attention."

6. "Coming out of the class in Epic and Heroic Poetry, three students were discussing what mythical figures they wanted to be.

A. 'I'll be Aphrodite. I like her.'
B. 'All right, I'll be Minerva.'
C. (loftily) 'You can be goddesses if you want to. *I'll* be Helen.'
A. (with emphasis) 'Is *this* the face that launched the thousand ships?' "

(*e*) The determination of (*e*) and (*f*) was by elimination of all the rest. Where an unlooked-for event or a turn of the conversation, whether voluntary or not, creates an incongruous situation, but the incongruity is not due to stupidity or ignorance; or when the laughter is not at a *person*, but is enjoyment of an unexpected incongruous situation, it is classified under (*e*). This can be called the *incongruity of situation* type.

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[2] *The Emotions and the Will*, 248.

The idea that there can be an incongruous situation without personal degradation is a much debated one; all humorous situations have people in them. When, for instance we laugh at the deaf librarian's answer to the question, "Did Dr. Taylor come in?"—"Yes, and he went under the desk and I cannot get him out!"—a cat having given the Library some trouble during the previous days—there is the librarian's physical defect, and the belittling of Dr. Taylor in imagining him under the desk, but it is really neither of these two people that we are laughing at. It is the incongruity in the unexpected situation rather than a feeling of superiority that gives pleasure. Of course, the incongruity in the situation is "a descending incongruity";[3] else the situation would arouse some other emotion incompatible with amusement.

Examples of (*e*):

1. "The literary member of our Math. class says if we don't stop talking about ther-mo-dy-nam-ics, she is going to get up and lecture on Romance in the Seventeenth Century!"

2. "We were speaking in class of how the American people as a whole were so much more inclined to hurry than other races. Professor Taylor told us of an experience she had in the Vatican Library. Professor Nogara had got a book for her, and she was looking through it hurriedly, when he said to her, 'Signorina, you have to look with a great deal of calm.' "

3. "My aunt ordered our Christmas presents (for my sister and me), and when they arrived there was a slip in the box, saying that it enclosed two rattles and a teething-ring."

4. "I laughed at a girl whose man is in the stock-market and who can't come to Prom. unless sugar breaks."

5. "In Psychology class we were told that may be some day we would have an apparatus for looking into people's brains and seeing the molecules running around."

6. "T. ——— 'Is your aunt married yet?'
"D. ——— 'No, but she is going to be engaged in April.' "

7. "H. B. discovered in her room, doing an Ec. note-book and clad in bloomers and brassiere: 'Can't do it otherwise,' she says, 'I've gotta feel free!' "

(*f*) When the incongruity is only an incongruity in *ideas*, it is put in this class. These instances are comparatively very few. They are puns, clever remarks not directed at anybody in particular, and nonsense in general, which is simply absurd and illogical, seen as such, and therefore humorous.

In this connection it is recognized that there is controversy as to whether there is such a thing as undirected wit. Greig[4] in his chapter on wit argues that all wit is directed at some person. He takes the familiar examples which Freud gives as illustrating directed and undirected wit, and finds in all the same underlying motivation. These are two of his examples:

Directed: Wendell Phillips, according to a recent biography by Dr. Lorenzo Sears, was on one occasion lecturing in Ohio, and while on a railroad journey going to keep one of his appointments, met in a car, a number of clergymen returning from some sort of convention. One of the ministers, feeling called upon to approach Mr. Phillips, asked him, "Are you Mr. Phillips?" "I am, sir." "Are you trying to free the niggers?" "Yes, sir; I am an abolitionist." "Well, why do you preach your doctrines up here? Why don't you go over into Kentucky?" "Excuse me, are you a preacher?"

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[3] H. Spencer, On the Physiology of Laughter. Essays on Education and on Kindred Subjects, *Everymans Library*, 309.

[4] J. v. T. Greig, *The Psychology of Laughter and Comedy*, 199-221.

"I am, sir." "Are you trying to save souls from Hell?" "Yes, sir, that's my business." "Well, why don't you go there?"

Undirected: Commenting on the saying, "Never to be born would be better for mortal man", the Fliegende Blätter remarked, "But hardly one man in a hundred thousand has this luck."

By an argument that involves the stretching of every joke to fit his theory, Greig makes the first and second alike, in that there is a person back of the first part of the second joke, a sentimental, rather stupid person, whose foolishness in saying that it were better for mortal man not to have been born, is brought out and made ridiculous by the very absurdity of the second part. Therefore the second part is also a directed remark and contains the personal element. In this way, everything can be made to include a personal element. We, however, are free to disagree with Greig, and hold to our belief in the existence of "nonsense", simple and impersonal.

Examples of (*f*) from the diaries:

1. "In informal debate: 'We are here to discuss, not to argue!' "
2. "That 1200 young women to get $150 for the Near East ate beans, proves that the end justifies the means!"
3. "K. reading, 'I must pause for breath.'
   "F. immediately, 'Cat's paws!' "
4. "Let us cut across the grass and make it shorter."
5. "One of our problems was to test how much of a cone a hyperbolic paraboloid occupied. Before performing the experiment there was great discussion as to the results of the work. Some thought it occupied a third, others thought a half. Finally one girl said; 'I'll bet you a nickel it's a half!' Thereupon Professor White remarked, 'The odds are even!' "
6. "Did you know there was a grave situation at Vassar?"—"What is it?"—"Everyone is just dying to go there!"

(4) *Distribution of Types and its Correlation with Ability*. The instances given in each of these seventy diaries were divided into these six classes, and the percentage of each class, or the percentage of that type of humor, was found for each person. The obvious criticism is that probably no two people would classify instances exactly alike, and that at times the classification seems arbitrary, as the material is often variable enough to defy classification.

The total number of instances of humor in the seventy diaries was 2107. The following are the numbers belonging to each type from the seventy diaries:

In type (*a*) 81 instances, or .0384 of the total.
" " (*b*) 539 " " .2558 " " "
" " (*c*) 815 " " .3820 " " "
" " (*d*) 248 " " .1177 " " "
" " (*e*) 400 " " .1898 " " "
" " (*f*) 102 " " .0484 " " "

The predominance of type (*c*) is noticeable. It has been mentioned that the same event sometimes seemed to belong to two classes; or, if an event was a story, that it sometimes passed from one class to another as the story went on. Such instances were put in both the classes to which they belonged, so that the sum of the instances in all the classes is greater than 2107. The sum of the percentages for each class is thus greater than 1.00. There were not many such diaries.

Taken in each class separately, the diaries were arranged in the order of diminishing quantity of humor of that class. The "quantity" of each class contained in each diary was determined by the percentage of humorous instances belonging to that class contained in the total number of humorous instances given by each person. This method compensated for the great differences in the length of the diaries, as it left only the relative amounts to work with.

After ranking the diaries in the order of their diminishing quantity of class (*a*), a rank difference correlation was found between this and the ranking of the authors in credit-ratios. The coefficient of correlation was —.37, P.E. 0.10. This negative correlation was a surprise. It tends to prove by a number, what is generally surmised, that laughter at nothing n particular, is not a sign of mental ability.

However, as only 36 of the seventy diaries reported instances of (*a*) laughter, the average credit-ratio for the group reporting (*a*) and the average credit-ratio for the group not reporting (*a*), were calculated and compared. They came to almost the same; from which we infer that it is not necessarily the tendency to laugh with no objective cause which correlates negatively with academic standing, but the tendency to laugh *only* with no objective cause; or, that it was the absence of other classes and not the presence of (*a*) that resulted in the negative correlation. These people, who laugh only at nothing, are Meredith's "hypergelasts."[5]

There was also a rank difference correlation found between the ranking of these diaries in (*a*) and the rank of their authors in their Freshman Psychological Tests. The coefficient of correlation in this case was —.29 P.E. 0.11. This is not conclusive in itself, but it perhaps strengthens the validity of the former negative correlation.

A rank difference correlation was found between the diaries as ranking in quantity of (*b*) and the credit-ratios of the authors. The coefficient of correlation was —.20, P.E. 0.08. which is too small to mean much. The average credit-ratio for those below the group average in (*b*) was 2.81; for those above the group average, 2.51. There is a slight difference here, suggesting that (*b*) is not a very intellectual type of humor.

For classes (*c*), (*d*), (*e*) and (*f*) there was no appreciable correlation either with credit-ratios or psychological tests.

If (*a*) is physiological and (*b*) has a physical cause which is objective, we can say that both are without thought in humor, and put them together in a class which on the whole correlates negatively with academic standing.

(*c*) and (*d*) could be put together into the personal superiority class, and (*e*) and (*f*) together into the impersonal class. Thus far we cannot call either of these classes more or less intellectual, since the intellectual factor as represented by academic ability seems to bear no relation to either of them.

II. Do Individuals Remain True to Type when Tested by another Method?

(1) *Method of Ranking Jokes*. The following experiment was primarily the way in which the consistency of twenty-five of the diary authors to the type of humor preferred in their own diaries was tested.

The material for the experiment consisted of 16 jokes, four representing every one of the four last classes in the diaries. Class (*a*) in the diaries is of course unrepresentable in jokes, while events of class (*b*) depend on their *happening*, and not on their being narrated, for their fun. They could not be tested with material of this kind. The jokes are the following in the order in which they were presented to the *O*s; they were selected from a much greater number. In this case the classification is more reliable than that of the sources of laughter in the diaries, as two critics, the author and Professor Washburn, were responsible for it.

Jokes 4, 5, 9 and 13 represent class (*c*); they are based on stupidity and mistakes; jokes 2, 7, 11, and 16 represent class (*d*), in being directed at a person who is thus made inferior; the two together represent the personal type. Jokes 1, 8, 12 and 14 represent class (*e*); there is incongruity in the situation. Jokes 3, 6, 10 and 15 represent class (*f*); the incongruity is in ideas, that is, they are nonsense. The last two together represent the impersonal type of joke.

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[5] G. Meredith, *An Essay on Comedy and the Uses of the Comic Spirit*, 76.

Jokes

1. Teacher: "What is a skeleton? Can you tell me, children?" Puzzled class. Finally one hand up. "Pleathe, mith, it ith a man without any meat on it."

2. "Colonel, do you remember the time you proposed to me and I refused you?"

"Madam, it is one moment of my life that I remember with the greatest pleasure."

3. "Begorry," said Pat, as he tried to stop a leak in his roof, "it's a true sayin' that it niver pours but it rains!"

4. In a rural community of Kentucky a family was in desperate circumstances as a result of continued illness. The church board and the preacher met to plan for their relief. A deacon, called on by the preacher to pray, waxed eloquent. "O Lord," he prayed, "help us to act as Thy messengers here on earth to these people. Help us not only to pray for them but to supply their need of food, O Lord. Put it in our hearts to carry them a barrel of flour, a barrel of pork, a barrel of sugar, a barrel of pepper—oh hell, that's too much pepper!"

5. Miss N.: "What is that you just played?"
Violinist: "An improvisation, Madam."
Miss N.: "Ah, one of my old favorites!"

6. A virgin forest is a wood where the hand of man has never set foot.

7. A certain lawyer had found the witness difficult to manage, and finally asked whether he was acquainted with any of the men on the jury.

"Yes, sir", replied the witness, "more than half of them."

"Are you willing to swear that you know more than half of them?" demanded the lawyer.

"Why", retorted the witness, "if it comes to that, I am willing to swear that I know more than all of them put together!"

8. "Pa?"
"Yes, my son."
"Pa, can the Lord make everything?"
"Yes my boy."
"Every, everything?"
"There is nothing, my son, that He cannot do."
"Pa, could He make a clock that would strike less than one?"

9. At the table in a certain boarding house, a student boarder who had been reading the scientific notes in a publication on a side table, remarked:

"More than five thousand elephants a year go to make our piano keys."

"My land!" exclaimed the landlady, "isn't it wonderful what some animals can be taught to do!"

10. "I enjoy a walk best when I walk alone.
"So do I! Let us walk together!"

11. "I wish you would correct your statement that my text was, 'Alas for the rarity of Christian clarity.' The last word was 'charity' not 'clarity'!"

"I know that, sir; but on reading your sermon over, I thought the other version, though not exactly correct, more highly appropriate and descriptive."

12. A lad was kept after school and required to write an essay of 100 words. He wrote the following composition: "Jack went to the door and called, 'Kitty, kitty, kitty, kitty.............. (93 times)' ".

13. Abroad:

"Baedeker says here, Mamma 'At this point in the road is visible probably the finest bit of scenery in all Europe, etc.,' "

"Ah, yes! Do you know, I always like scenery. It adds so much."

14. Three small boys were earnestly discussing the ability of their respective fathers. The son of a song writer said, "My father can come home in the evening and sit down after supper and write a song and take it in town the next morning and sell it for twenty-five dollars."

"But my dad," eagerly spoke up the young heir of a short story writer, "can write a story in an evening and take it in town the next morning and sell it for fifty dollars."

The preacher's son was a bit non-plussed until he had an inspiration. "My father," he announced triumphantly, "gets up in the pulpit and talks half an hour, and then it takes twelve men to carry the money up to him!"

15. At the Agency:

"Are you a good cook and laundress?"

"Do Oi look loike twins?"

16. "Staggers is thinking of writing his confessions."

"He ought to entitle them 'Wild Animals I Thought I Met'."

The *O*s were tested individually; every *O* was told to read the 16 jokes and arrange them in the order of their funniness, beginning with the funniest and ending with the least funny. She was told that she could take all the time she wanted, but she was requested not to read the jokes over too many times, and not to study them or try to give to herself the reasons for their funniness in the process of arranging them. With the knowledge that there are waning and waxing jokes, this caution had to be observed, as it was not desirable to have the waning and waxing processes enter into the experiment.[6]

The rank given to each joke was recorded for each *O*.

(2) *Group Ranking of Jokes*. The first results to be noted concerned the material rather than the *O*s. By adding the ranks given to a joke by everyone of the 25 *O*s, a number is found which represents the value of that joke for the group. The smaller this number, the higher must have been the ranks assigned to the joke in question, and hence the funnier it is in the combined judgment of the group. The groups ranked the jokes in the following descending order: 14, 15, 1, 9, 7, 6, 12, 11, 8, 5, 2, 10, 3, 4, 13, and 16. Jokes 1, 8, 12, and 14 were the situation jokes (*e*): the sum of their combined rankings is 20. Jokes 3, 6, 10, and 15 were the nonsense jokes (*f*): the sum of their combined rankings is 33. Jokes 4, 5, 9, and 13 were the stupidity jokes (*c*): the sum of their combined rankings is 43. Jokes 2, 7, 11, and 16 were the directed jokes (*d*): the sum of their combined rankings is 40. Thus the situation jokes were liked best, the nonsense second, the directed answers third, and the stupidity jokes least. This is inconsistent with the diaries, which report a much greater proportion of (*c*) and (*d*) than of (*e*) and (*f*). The discrepancy may be due to more frequent occurrence of (*c*) and (*d*) events in the course of a day. If this were so, a point would be gained for the superiority theories of humor over the incongruity and shock theories. On the other hand, (*e*) and (*f*), being of the waning kind, may be easily forgotten, or not appreciated as much when recollected, while (*c*) and (*d*), having the personal element in them, may tend to be remembered and enjoyed longer.

The variations in the ranks assigned to a given joke were sufficient to prove that our *O*s differed from one another in the sense of humor. Jokes 8 and 1 were given first rank four times each; jokes 11 and 12 were ranked first three times each; jokes 7, 9, 6, and 15 were ranked first twice each; jokes 13, 2, 16, 3, and 10 were not placed first by anybody, and they represent three different classes. Joke 14, which is first in the group ranking was given first place by one *O* only; it was given fifteenth place by another *O*.

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[6] The (*f*) jokes, that is the nonsense ones, would have certainly waned very fast; nonsense loses all its fun when it is analyzed. Class (*f*) belongs wholly to Dr. Hollingworth's "subjective" jokes. Class (*e*) belongs mostly to his subjective jokes too, though there may be some incongruous situations which are impersonal but objective. On the other hand, some of the (*d*) jokes may be considered subjective. The two classifications do not correspond exactly. *Cf.*, H. L. Hollingworth, Experimental Studies in Judgment; Judgment of the Comic, *Psych. Review*, 18, 1911, 132-156.

The average deviation of the four stupidity jokes taken together was 4.07; that of the directed jokes was 3.7; that of the situation jokes was 3.7; that of the nonsense jokes was 3.31. There is thus closer agreement on the nonsense than on the stupidity jokes. As in Professor Hollingworth's experiment, there was an indication that people agree more closely in their preferences than in their dislikes. The average deviation of the first four jokes in the group ranking taken together was 3.35; that of the last four 3.67. The average deviation of the middle eight jokes was 3.88; the *Os* differed most where they were most indifferent.

(3) *The Self-Consistency of the Individual.* The consistency of our *Os* to their own diaries was the most important point in this study; it was investigated in the following way. The 25 *Os* were ranked in the order of their appreciation of (*c*) jokes, and also in the order of the proportion of (*c*) reported in their diaries. The same was done for the (*d*) and (*e*) classes of jokes, and rank difference correlations were found between the appreciation of a given class of jokes, and the proportion of the same class reported in the diaries. The following coefficients of correlation resulted:

R. of consistency in (*c*)—+.31, P.E. 0.12
" " " " (*d*)—+.36, P.E. 0.12
" " " " (*e*)—+.31, P.E. 0.12

The (*f*) material in the diaries is too scanty to be used as a basis for ranking the *Os*.

Then (*c*) and (*d*) were added in the diaries, and again in the appreciation of the jokes, representing in both the proportion of the personal type. A rank difference correlation was found for (*c*) and (*d*) thus combined, between diaries and appreciation of jokes. If the correlation showing consistency had been destroyed by the adding of the two classes, the supposition that the two classes really represented one, would have been disproved. The coefficient of correlation however, was +.32, P.E. 0.12. (*e*) and (*f*) were also added, and the consistency of our *Os* in diaries and in appreciation of jokes was +.49, P.E. 0.10. This rise in the correlation may mean that (*e*) and (*f*) are more closely related than (*c*) and (*d*) are, and are harder to distinguish.

When we consider the variability of the material, these consistency correlations are fairly satisfactory. They tend to establish the fact that individuals are fairly consistent in preferring on the whole either the personal, or the impersonal type of the humorous.

(4) *The Relation of Type to General Ability.* As with the diaries, so with the appreciation of jokes, rank differences correlations were found between every class and the rank of the *Os* in credit-ratios. This time the result was a correlation of +.30, P.E. 0.12 between academic standing and appreciation of nonsense jokes. Though this is small, it is suggestive, and the relation of credit-ratios to the appreciation of each class of jokes was further calculated by comparing the average credit-ratios for those over-appreciating and for those under-appreciating each class of jokes. The results are:

1. Average credit-ratio for those over-appreciating (*c*), 2.68
" " " " under-appreciating (*c*), 3.26
2. " " " " over-appreciating (*d*), 2.75
" " " " under-appreciating (*d*), 3.17
3. " " " " over-appreciating (*e*), 2.76
" " " " under-appreciating (*e*), 3.17
4. " " " " over-appreciating (*f*), 3.14
" " " " under-appreciating (*f*), 2.75

Os of better academic standing tend to enjoy the nonsense jokes more than do the other *Os*. In the experiment the over-appreciation of any one class would mean the necessary under-appreciation of some other class. The differences in credit-ratios show that any other class may lose by the over-appreciation of (*f*) but that class (*c*) loses the most.

When we combine (*c*) with (*d*) and (*e*) with (*f*), the average credit-ratio for those over-appreciating the first and necessarily under-appreciating the second, is 2.82. The average credit-ratio for those over-appreciating the second and necessarily under-appreciating the first, is 3.06. The difference is small enough to show that it is the influence of the nonsense jokes that causes it, and that there is no relation between academic ability and the personal and impersonal types of humor, except for the nonsense jokes.

The variability of the *O*s from the group-judgment was calculated, but there was no relation between this and academic standing, nor between either of these and the consistency of the *O*s to their own preferences. If several such experiments had been tried on the same *O*s, it would have been interesting to find out whether or not individuals who vary from group judgment in one experiment vary also in another.

CONCLUSIONS

There is a fair degree of consistency to types of humor shown by our *O*s.

These types are primarily the personal and the impersonal. The former can be subdivided into passive personal and directed personal; and the latter into the perception of incongruity in situations and the perception of incongruity in ideas, or the perception of nonsense. The consistency shown by our *O*s justifies the two main types as well as their subdivisions.

The consistency of our *O*s as far as laughter with no objective cause, and laughter with a physical objective cause are concerned, could not be tested in our experiment.

Mental ability, as represented chiefly by academic standing, has some influence in the determination of the predominance, relatively, of certain types, in decreasing the proportion of physiological laughter and of laughter with a physical cause, and increasing the appreciation of the nonsense jokes.

Mental ability bears no relation to the personal and impersonal types of humor except for the nonsense jokes.

The personal and the impersonal types of bumor need to be experimentally related to temperament and character, and the relative influence of these and mental ability determined, before the sense of humor can be further analyzed.

LIV A COMPARISON OF DIRECTED AND FREE RECALLS OF PLEASANT AND UNPLEASANT EXPERIENCES, AS TESTS OF CHEERFUL AND DEPRESSED TEMPERAMENTS

By. M. F. WASHBURN, M. E. BOOTH, S. STOCKER, and E. GLICKSMANN

Two methods have been used in our laboratory to test cheerful and depressed temperaments. The first, reported in this JOURNAL (28, 1917, 155-157), was as follows: the *O*s were given five stimulus words in succession with the instruction to rap on the table when each word had suggested an unpleasant idea; the intervals between giving the word and the *O*'s rap were measured by a stop watch; then five words were given with the instruction to recall a pleasant associated idea; then five more with the instruction to recall unpleasant ideas, and so on. The average reaction time with the 'unpleasant' instructions was then divided by the average reaction time with the 'pleasant' instructions. Our hypothesis was that an *O* inclined to depression would recall unpleasant ideas more readily than one inclined to cheerfulness, so that his ratio, as found in the manner above described, would be relatively small. Indications were found in the 1917 study of some correlation with temperamental differences.

The second method, also described in this Journal (30, 1919, 302-304, and 36, 1925, 454-456), consists in giving the *O* a series of stimulus words with the instruction to report whether a given word recalls a pleasant or an unpleasant idea; if the idea suggested is indifferent, he is to 'continue thinking' until a pleasant or an unpleasant idea suggests itself, and report which it is. The scoring is done in terms of the total number of stimulus words that suggest pleasant ideas; the hypothesis being that a person of cheerful temperament will recall more than the average number of pleasant ideas.

In the present Study our aim was to compare these methods as regards their efficiency in distinguishing between a cheerful and a depressed group of *O*s. The groups were obtained by the method used in the 1925 Study; that is, the members of a large class of young women students were asked to judge themselves as either steadily cheerful (counting 4 points), variable tending to cheerfulness (counting 3 points), variable tending to depression (counting 2 points), or steadily depressed (counting 1 point). Two groups, representing opposite extremes of judgment, were selected (there were very few who classed themselves as steadily depressed, so the depressed group had to include those who described themselves as variable tending to depression). Every individual in these groups was then judged by three of her friends, using the four scale-divisions mentioned above. For every individual there was thus obtained a cheerfulness score ranging from 16 for one who judged herself and was judged by all three friends as steadily cheerful, to 4 for one who judged herself and was judged by all three of her friends as steadily depressed.

The cheerful group as finally constituted included those persons with scores from 14 to 16 inclusive; the depressed group those with scores up to and including 10. There were 64 in the cheerful group and 59 in the depressed group. The method described in the first paragraph of this article will be referred to as Method I; that described in the second paragraph as Method II. In connection with Method I, three series of stimulus words, each containing six groups of five words each, were used, one series on each of three consecutive days. In connection with Method II, three series of thirty stimulus words each were used, one series on each of three consecutive days. Extending the experiments over three days aimed to minimize the effect of temporary mood. An individual's score by Method I was obtained by finding for every day the quotient of the sum of the unpleasant association times divided by the sum of the pleasant association times, and adding these three quotients. Her score by Method II was the total number of pleasant ideas suggested in the three days' experiments.

The following results were obtained by Method I. Among those *O*s who were in the first quartile, that is, who took relatively longest to recall unpleasant ideas, there were 25, or 39%, of the cheerful group; and 6, or 10%, of the depressed group. Among those *O*s who were above the median, there were 41, or 64%, of the cheerful group, and 20, or 33.9% of the depressed group. Among those *O*s who were below the median there were 22, or 34%, of the cheerful group, and 39, or 66.1%, of the depressed group. Among those *O*s who were in the last quartile, there were 8, or 12.5%, of the cheerful group, and 23, or 39%, of the depressed group.

The following results were obtained by Method II. Among those who were in the first quartile, that is, had the highest number of pleasant ideas suggested, there were 22, or 34%, of the cheerful group, and 9, or 15.2%, of the depressed group. Among those who were above the median, there were 41, or 64%, of the cheerful group, and 20, or 33.9%, of the depressed group. Among those who were below the median there were 22, or 34%, of the cheerful group, and 39, or 66.1%, of the depressed group. Among those who were in the lowest quartile there were 6, or 9.7%, of the cheerful group, and 25, or 42.2%, of the depressed group.

It thus appears that both methods have value for the determination of cheerful or depressed temperaments. By Method I, it is about four times as likely that a person in the first quartile will be cheerful rather than depressed, nearly twice as likely that a person above the median will be cheerful rather than depressed, nearly twice as likely that a person below the median will be depressed rather than cheerful, and about three times as likely that a person in the lowest quartile will be depressed rather than cheerful. By Method II, there is about twice as much probability that a person in the first quartile will be cheerful rather than depressed, twice as much that a person above the median will be cheerful rather than depressed, twice as much that a person below the median will be depressed rather than cheerful, and four times as much that a person in the lowest quartile will be depressed rather than cheerful. A very slight superiority of Method I appears from the following treatment of the results. If the methods were not at all diagnostic, it is evident that 25% of each group ought to be in the first quartile, 50% above the median, 50% below the median, and 25% in the last quartile. The differences between these percentages and those actually obtained indicate the degree of correlation between the scores and the degrees of cheerfulness or depression; that is, the diagnostic value of the methods. Thus, Method I gave for the first quartile a difference of 14%, for above the median a difference of 14%, for below the median a difference of 16%, and for the last quartile a difference of 12.5%, in favor of its diagnostic value, and the sum of these differences is 56.5. Method II gave for the first quartile a difference of 9%, for above the median a difference of 14%, for below the median a difference of 16%, and for the last quartile a difference of 15.3%, in favor of its diagnostic value, and the sum of these differences is 54.3. In the depressed group, Method I gave for the first quartile a difference of 15%, for above the median a difference of 16.1%, for below the median a difference of 16.1%, and for the last quartile a difference of 14%, in favor of its diagnostic value. The sum of these differences is 61.2. Method II gave for the first quartile a difference of 9.8%, for above the median a difference of 16.1%, for below the median a difference of 16.1%, and for the last quartile a difference of 16.8%. The sum of these differences is 58.8. As the signs of all the differences are in accord with what would be expected if the methods have diagnostic value; that is, they are plus above the medians and minus below the medians for the cheerful group and vice versa for the depressed group, the diagnostic value of Method I may fairly be represented by 56.5 plus 61.2, or 117.7, and that of Method II by 54.3 plus 58.8, or 113.8.

The rank difference correlation between Method I and Method II is + 37.8, P.E. 0.05.

Considering that a certain amount of error attaches to the estimates on which our cheerful and depressed groups were selected, both these methods of diagnosis seem worthy of consideration. It is probable, also, that our groups of college girls represented no very extreme divergences in the matter of cheerfulness and depression, and that older *O*s would furnish more striking results.

LV. A FURTHER STUDY OF REVIVED EMOTIONS AS RELATED TO EMOTIONAL AND CALM TEMPERAMENTS

By M. F. WASHBURN, J. ROWLEY, and G. WINTER

In this JOURNAL (36, 1925, 454-460), there were reported some attempts made in this laboratory to study the relation between emotional and calm temperaments on the one hand, and the intensity, introspectively reported, with which the emotions of anger, joy and fear were revived in memory, the recency from which they were recalled, the number of such emotionally toned incidents that could be recalled, and the galvanometric disturbance accompanying the recall. In the present Study this investigation was repeated with other *O*s, and incidentally the correlation found between the recency of the emotionally toned experiences recalled by a given *O* and the number of such incidents she could recollect; the hypothesis being that a person much subject to emotional disturbance would recall, as a rule, more recent incidents and a greater number of them.

The emotionality of our *O*s was determined by the same method as in the earlier study: an *O* was required to judge herself, and three of her intimate friends were asked to judge her, as either emotional, counting four points, emotional rather than calm, counting three points, calm rather than emotional, counting two points, or calm, counting one point. Her emotionality score was the sum of these credits, and thus ranged from 16, for an *O* who was judged emotional by herself and all three of her friends, to 4, for an *O* who was judged calm by herself and all three of her friends.

Two groups of *O*s were used in our experiments. Group I consisted of fifty students of psychology, twenty-five being classed as emotional, with scores of from 12 to 16, and twenty-five as calm, with scores of from 4 to 9. Group II consisted of forty-four freshmen who had not studied psychology, half of these were likewise classed as emotional and half as calm.

Two experiments were performed. In the first experiment, the *O* was comfortably seated by the galvanometer, and cylindrical electrodes were securely tied to the palms of her hands and connected with the galvanometer. She was then asked to recall an occasion on which she had been very angry, to let the recalled emotion develop as fully as it would by mentally reliving the incident, and to give a signal when the recall was complete. The experimenter noted the extreme deflection of the bright disk. After the recall had been completed, the *O* was asked whether the revived emotion had been as intense as the original one, in which case it was credited with 4 points of intensity; somewhat less intense, which counted as 3 points; very much less intense, counting as 2 points; or whether she had merely recalled the fact that she had once felt the emotion, in which case one point only was credited. She was also asked how long ago the incident had occurred. A similar procedure was followed for the revival of joy and fear.

The results of this experiment were as follows: (*1*) Is there any relation between degree of emotionality in an *O* and the recency from which she recalls emotionally toned incidents? If so, is it sufficiently marked to serve as a diagnostic test separating emotional and calm groups?

The figures show that our emotional *O*s did not tend to recall emotions from more recent dates than those from which our calm *O*s recalled them. There were about as many emotional as calm *O*s who recalled from dates above the median in recency, except in the case of anger. In Group I, 56% of the emotional *O*s were above the median in recency; in Group II, 63.6% of the emotional *O*s were above the median. There is a slight tendency for the emotional *O*s to recall anger from more recent dates than do the calm *O*s, but of course nothing like a diagnostic difference.

(*2*) Is there any relation between the intensity of recalled emotions, introspectively reported, and the degree of an *O*'s emotionality? If so, is it sufficiently marked to serve as a diagnostic test?

The *O*s of Group I show some relationship between intensity of recall and degree of emotionality. When each *O*'s intensities for anger, joy, and fear are added, of the nineteen persons for whom this sum was above 6, twelve, or 63.6% were emotional. Of the thirteen for whom this sum was below 6, four, or 30.6%, only were emotional. No such difference appeared for Group II, and in any case it is too small to be significant.

(*3*) Is there any relation between degree of galvanometric deflection during recall of an emotion and degree of emotionality in the individual? If so, is it marked enough to have diagnostic significance?

Unfortunately we have galvanometric records only for the *O*s of Group I. After the experiments on Group II had been made it was discovered that the adjustment of the galvanometer had been tampered with. When each *O*'s maximum deflections during recall of anger, joy and fear had been added, it was found that of the *O*s for whom these sums were in the first quartile for the group, 81.5% were emotional; of the *O*s who were above the median, 69% were emotional; of the *O*s below the median, 60.8% were calm; of the *O*s in the lowest quartile, 66% were calm. Perhaps a better way of reckoning the galvanometric scores is to discard all small deflections,—with our adjustment of the galvanometer, all below 10 mm.,—as insignificant; they might be produced by accidental causes. There were 20 out of the 50 *O*s of Group I who had no deflections above 10 mm. Of these 17, or 85%, belonged in the calm group. There were 20 the sum of whose deflections above 10 mm. amounted to 20 or more, and of these 15, or 75%, belonged to the emotional group. It thus appears that the amount of galvanometric deflection does possess a very fair degree of diagnostic value.

(4) Is there any relation between the amount of galvanometric deflection and the intensity of the revived emotion introspectively reported?

Apparently there is some relation. For 31 of the 50 *O*s who performed the experiment with the galvanometer, the largest deflection occurred in connection with that emotion to which the *O* assigned the highest intensity. This however includes cases where two emotions were assigned the same intensity, higher than that of the third, one of the two being accompanied by the largest deflection which occurred with that *O*. There were only 10 *O*s for whom the largest deflection occurred in connection with the lowest intensity reported.

The second type of experiment performed followed the method described in this Journal (36, 1925, 458). The *O* was asked to recall as many instances as she could where she had felt great anger, and to give a signal as each one occurred to her. The number of signals was recorded. A similar procedure was followed for joy and fear.

(5) Will an emotional *O* recall more instances of anger, joy, and fear than a calm *O*? If so, is the difference sufficient to constitute a diagnostic test?

In Group I, 66% of those in the highest quartile as regards the total number of emotionally toned incidents recalled were emotional. 61.5% of those above the median were emotional. 60.8% of those below the median were calm, and 58.3% of those in the lowest quartile were calm. In Group II, 81.8% of those in the first quartile were emotional, 65% of those above the median were emotional, 55% of those below the median and 45.5% of those in the lowest quartile were calm.

It thus appears that there is a tendency for the more emotional *O*s to recall more emotionally toned incidents than do the calm *O*s, but the difference is not sufficiently marked to constitute a means of diagnosis.

(6) Is there any correlation between the recency from which an *O* recalls incidents in the first experiment and the number of incidents she recalls in the second? It might be supposed that both depend on the frequency with which emotions are experienced.

For anger, the rank difference correlation was +.29, P.E. 0.09, in the case of Group I of *O*s, and +.39, P.E. 0.08, in the case of Group II.

For joy, the correlation was −.05, P.E. 0.09, in the case of Group I, and −0.15, P.E. 0.105, in the case of Group II.

For fear, the correlation was +.24, P.E. 0.09, in the case of Group I, and −.03, P.E. 0.10, in the case of Group II.

It thus appears that no correlation exists, except possibly in the case of anger.

On the whole, the method of revived emotions, except when the galvanometer is used, is not a success in separating an emotional from a calm group.

Downey Test Scores Made by Emotional and Calm Groups

37 of our *O*s, including 20 emotional, that is, with emotionality scores of 12 or more, and 17 calm, that is, with emotionality scores of 6 or less, were given the Downey Group Will-Temperament Test.

In the first division of the tests, regarded by Professor Downey as essentially speed tests, taken as a whole, there was no striking difference between the two classes of *O*s. Analysis of the tests of this group individually showed no difference between calm and emotional *O*s in freedom from load or speed of decision. The test for speed of movement indicated that few calm *O*s made the highest score. There were 10 in all who made scores of 10, and only 2 of them belonged in the calm division; that is, 40% of the emotional class and 11.8% of the calm class made the highest score in speed. In the test for flexibility there was some indication that the calm *O*s were less flexible. Of the 8 who made the highest scores, 5 were emotional and 3 calm, which is nearly balanced; but of the 9 who made scores of 4 or below, 3 were emotional, constituting 15% of the emotional *O*s, and 6 calm, or 35% of that class of *O*s.

In the second division of the tests, which are supposed to represent aggressive traits, the results in this Study indicated superiority for the calm group. Analysis showed that the tests for motor impulsion and non-compliance were responsible for this result. Motor impulsion was notably greater in the calm group. 8 *O*s reached scores 9, 8 or 7, and all of them belonged to the calm group, constituting 47% of that group. 9 *O*s made the low scores of 1, 2, 3, or 4; 7 of them were emotional, or 35% of the emotional group, and 2 calm, or 11.7% of the calm group. The emotional group was more compliant than the calm group. 9 *O*s scored 10, 9, 8, 7, or 6 in the non-compliance test; 6 of them were calm, or 35% of the calm, 3 emotional, or 15% of that group. 12 *O*s made the low scores of 1, 2, or 3; 9 of these were emotional, constituting 45% of the emotional group, and 3 calm, constituting 17.6% of the calm group. There was no difference between the *O*s as regards self-confidence or finality of judgment.

In the third division of the tests, which are supposed to indicate motor stability, an apparent superiority of the emotional group proved on examination to be due to the factors of interest in detail and motor coördination. There was no difference between the groups in motor inhibition (which it may be noted was very low in all our *O*s: no one made a score above 2), or volitional perseveration. There were 10 *O*s who made scores of 10, 9, or 8 in interest in detail; of these 8 were emotional, forming 40% of the emotional group, and 2 calm, forming 11.7% of the calm group. There were 10 *O*s who made the low scores of 2, 3, 4, or 5; 6 of these were calm, constituting 35% of the calm group, and 4 emotional, constituting 20% of the emotional group. In motor coördination, 12 *O*s made the high scores of 10 or 9; 8 of these, 40% of the emotional group, were emotional, and 4, 23.5% of the calm group, were calm. 14 *O*s made the low scores of 1, 2, 3, 4, or 5; 10 of them, 58.8% of the calm group, were calm, and 4, 20% of the emotional group, were emotional.

In summary: our calm *O*s showed on the whole greater motor impulsion, less suggestibility or compliance, less interest in detail and poorer motor coördination than did our emotional *O*s. There was some tendency for them to be slower in movement and to show less flexibility.[1]

It seems possible that the superiority of the emotional *O*s in attention to detail may have been due to superior ability on their part. The median academic standing of our emotional group was, measured in terms of the Vassar system of credit-ratios, 2.38; that of our calm group, 2.14. The median credit-ratios of the *O*s standing high in attention to detail was 2.78; that of the *O*s who stood low in attention to detail was 2.27. Shall we say that this indicates difference in general ability, or that superiority in the special character of attention to detail produces high academic records as well as high records in the Downey test directed to this character? The same consideration applies to coördination of movements: the median credit-ratio of the group making high scores in this test was 2.82; that of the group making low scores, 2.4.

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[1] Two of these traits, slowness and lack of flexibilty, are considered as introvert traits, but lack of interest in detail and lack of suggestibility are held to be extrovert traits.

MINOR STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY

PRIOR ENTRY IN THE AUDITORY-TACTUAL COMPLICATION[1]

By SYBIL A. STONE

"The stimulus for which we are predisposed requires less time than a like stimulus, for which we are unprepared, to produce its full conscious effect."[2] This simple law of prior entry has, however, depended for its experimental demonstration primarily upon the complication experiment in which the observer notes the incidence of a sound (or touch) upon the perception of a continuously moving pointer. The experiment involves two difficulties. In the first place, as Dunlap has pointed out,[3] the visual perception of a moving object introduces eye-movements and at best transforms the continuous movement into a perception of successively discrete positions. The test of the law would be better made by eliminating movement; and in the present experiment this end is accomplished by the use of an auditory-tactual complication. In the second place, the rhythmic movement of the pointer provides in its repetition a temporal preparation of the observer which enables him cumulatively to 'accommodate his attention' toward the event of coincidence, so that prior entry as dependent upon attentive predisposition is obscured by its concurrent dependence upon the factors that further temporal accommodation. In the present experiment, therefore, rhythmic repetition has been eliminated, and the observer passes a judgment of successivity or simultaneity upon a pair of stimuli presented but once after a warning signal.

The tactual stimulus, in this experiment, was given to the right forefinger of the *O*. The *O* rested his forearm and hand, palm down, on a wooden platform, and pressed the tip of the index finger over the upper end of a small hole, slightly countersunk. Below the platform was a solenoid with an iron plunger bearing a brass rod with a blunt conical tip. When a current passed through the solenoid, the plunger was raised so that the brass tip came up momentarily through the hole and into contact with the forefinger with a pressure of about 4 grm. The action of the plunger, both on making contact and on return, was noiseless.

The auditory stimulus was a similar solenoid with a hard rubber cap fastened very firmly across the top, so that the plunger in rising produced a sharp click when it came into contact with the cap. The return was noiseless. An earlier attempt to get a simple, sharp, uniform click from a telegraph sounder had been unsuccessful.

For the actuation of these stimuli there was an electric phonograph motor, with governor, bearing a brass drum, 300 mm. in circumference. The drum was made in two parts, so that the upper half could be twisted with respect to the lower half. Each half of the drum was covered with tracing-cloth, held in position by shellac, and a short slot was cut in each piece of tracing-cloth. Two electric spring contacts pressed against the two halves of the drum, completing electric circuits when the slots in the cloth came under them. The two circuits thus established were used to actuate the two stimuli. By twisting the one half of the drum with respect to the other either stimulus could be made to precede the other by any amount, or the two stimuli could be given simultaneously. A scale on the drum indicated the interval between the two stimulations.

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[1] Communicated by Edwin G. Boring.

[2] E. B. Titchener, *Psychology of Feeling and Attention*, 1908, 251. See in general, *ibid.*, 251-259 and notes; W. Wundt, *Physiologische Psychologie*, 6 Aufl., 1911, III, 58-79; W. James, *Principles of Psychology*, 1890, I, 409-416; and the classical references here cited to von Tisch, Angell and Pierce, Geiger, and Stevens.

[3] K. Dunlap, *Psychol. Rev.*, 17, 1910, 157-191.

The drum was adjusted to rotate once on 1.5 secs. and its speed was checked daily. Since it was 300 mm. in circumference, 2 mm. on the drum corresponded to 10 σ in time-interval.

Throughout the experiment the stimulus-intervals determined in preliminary trials were adhered to. These intervals were such that the touch followed the sound by −60, −30, 0, +30, and +60 σ. Here the negative values mean that the touch actually preceded the sound. Simultaneity is indicated by 0.

It will be noted that the maximal interval, 60 σ, occupied only 12 mm. on the drum. The experimenter closed both circuits with a single key before stimulation while the remaining portion of the drum was passing under the contacts, and opened the circuits by releasing this key as soon as the stimulation was over. Thus only a single pair of stimuli was presented to the observer.

The *Os* were Miss H. Peak (*P*), Mr. J. R. Butler (*B*), graduate students of psychology, and Miss M. S. Russell (*R*), an undergraduate concentrating in psychology. They were given the following instruction:

"You will be given the sound of a click and a touch upon the forefinger. You are to report whether the 'touch' or 'sound' comes first or whether they come together in time. In this experiment you are to attend to the touch [sound]. Expect the touch [sound]. Attend just as completely as possible to the touch [sound], after you get the warning signal and until you get the two presentations. Then make your judgment as to which comes first. If you have been inattentive, or for any reason think you have not followed the instructions, or if you are in doubt of the judgment for any reason, do not make a judgment but report the fact to the experimenter."

The five intervals were presented in accordance with the procedure of the method of constant stimuli, and series with the attentive predisposition for touch were alternated with series with the attentive predisposition for sound. Fifty presentations of every interval with each predisposition and with every observer were given. The resultant limens are, therefore, based upon 250 judgments each. The results are shown in the Table.

The actual frequencies that constitute the psychometric functions are shown in rows 1 to 5. These frequencies indicate the preliminary nature of the present experiment, for had time permitted, it would have been better to have changed the stimuli so as to avoid frequencies of 0 and of 100, values which are weighed by 0 and therefore play no part in determining the limens. As the data stands, it will be noticed that of the 12 psychometric functions all five frequencies are significant in six cases. Three functions, however, depend on only four frequencies; two depend on three; and one, on only two. In the last case the limen has to be extrapolated; in all the others it is interpolated.

The resultant limens are shown in sigma in rows 6 and 7, and the corresponding moduli, *h*, pertaining to σ, in rows 8 and 9. Row 10 gives the interval of uncertainty of the order of 16 to 60 σ. The large value for *P* depends on the extrapolated limen and must not be taken too seriously.

The points of subjective equality, row 11, are the foci of the present study. The meaning of the figures, in the case of *B*, for example, is that, when *B* is predisposed for 'sound', the touch has objectively to precede the sound by 17 sigma in order that the two may be perceived as coincident; whereas, when *B* is predisposed for touch, then sound has to precede the touch by 23 sigma for the two to be perceived as coincident. Prior entry would thus seem to be of the order of 45 sigma and less.[4]

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[4] W. B. Pillsbury cites 20 to 60 σ as the order of prior entry in the visual-auditory complication, *Attention*, 1908, 8.

TABLE: PRIOR ENTRY IN AUDITORY-TACTUAL COMPLICATION

The frequencies of rows 1-5 are percentages. All other figures (rows 6-13) are times expressed in σ. These times are the intervals from the sound to the touch; hence a positive value means that the sound precedes the touch, and a negative value that the touch precedes the sound. For further explanation, see text.

Observer:		*B*				*P*				*M*				Table row
Predisposition:		Sound		Touch		Sound		Touch		Sound		Touch		
Function:		Sound	Touch	Sound	Touch	Sound	Touch	Sound	Touch	Sound	Touch	Sound	Touch	
Frequency, %,	−60	6	92	0	100	22	22	6	66	32	54	6	88	1
against	−30	14	52	0	92	36	8	12	42	48	26	30	54	2
stimulus-	0	36	8	2	44	48	0	20	26	66	16	36	52	3
intervals,	+30	94	0	24	20	90	0	52	8	74	10	62	36	4
σ	+60	100	0	96	2	92	0	94	0	86	8	64	32	5
Limens	Sound	0.49		37.67		13.19		21.04		−25.69		22.09		6
	Touch	−29.44		−2.33		−96.58		−38.48		−65.62		5.60		7
Precision	Sound	.0226		.0466		.0138		.0169		.0089		.0099		8
(*h*)	Touch	.0331		.0252		.0149		.0135		.0091		.0080		9
Interval of uncertainty		29.93		40.00		109.77		59.52		39.92		16.48		10
Point of subj. equality		−17.30		23.63		−43.79		−5.39		−45.87		14.67		11
Average point of subj. equality		3.16				−24.59				−15.60				12
Diff. betw. pts. subj. equality		40.93				38.40				60.55				13

We are not, however, certain that objective equality is exactly represented by *O*. There may be different latent times in the two stimuli or in the two excitatory processes. For *P* both points of subjective equality are negative. Had this been true of the other *O*s, we might have concluded that the sound preceded the touch at 0 and that excitatory equality is best represented by a negative number. The individual differences of the *O*s, however, render any such conclusion impossible; nevertheless it is to be noted that the general tendency of the data is to imply an objective equality represented by a negative value. Row 12 gives the averages of the points of subjective equality. If the data were symmetrical, these averages would be 0. They are negative for *P* and *R* and only just barely positive for *B*. The general average is $-12.3\,\sigma$.

It is not necessary, however, to consider objective equality. We can take the differences between the two points of subjective equality, row 13, as indicating the sum of the facilitation by predisposition to sound and the facilitation by predisposition to touch. These values are 40, 38 and 60 σ, and imply average times of prior entry of 20, 19, and 30 σ.

The data upon which this experiment is based are not numerous enough, nor in some cases drawn from sufficiently strategic stimuli, for us to lay weight upon the actual values that issue from the liminal computations. Nevertheless the raw psychometric functions, rows 1 to 5, show clearly the striking effect of the predisposition in favoring the priority of the impression toward which the predisposing attention is directed; and here the 0's and the 100's, which drop out of the liminal computations become as significant as the other frequencies.

As to how the perception of one stimulus is facilitated or the perception of another delayed, as to the physiological nature of such delay, we are still completely uninformed. It would not seem, however, that the discrepancy can be localized in the report. Simultaneous stimuli might be reported as successive if, because of the predetermination, the introspective 'note-taking' process were successive. Here, however, we seem to have successive stimuli experienced as simultaneous because the later is facilitated, or the earlier delayed, or both; and we have experiences of both orders of succession forming consistent distributions on each side of simultaneity.[5]

Conclusion. In a complication of auditory and tactual stimuli the latent time of the sensation for which the *O* is attentively predisposed appears to be less than the latent time of the sensation for which the *O* is not predisposed. The difference in the latent times in this experiment was of the order of 50 σ. The delay of the one impression in relation to the other may be such as to make two potentially successive impressions simultaneous, or as to make two potentially simultaneous impressions successive, or as to increase the interval of successivity. It seems probable that this difference in time is established prior to the occurrence of those introspective processes which are the immediately first phase of the judgment of the impressions; *i.e.*, prior entry is not an artifact of report but a factor in the temporal relations of immediate sensory experience.

VISUAL LOCALIZATION OF THE VERTICAL[1]

By ELIZABETH NEAL

Koffka has suggested a relativistic theory of spatial position.[2] "A definite single phenomenal position exists only within a fixed spatial level. If the conditions for the formation and conservation of such a level are absent, localization is no longer possible." Ordinarily the spatial level (*Raumlage*) is determined by "visual points of anchorage." The visual field contains many points of known position which constitute a frame to which other phenomenal objects can be referred. All one needs is an appreciation of relative position to become, under ordinary circumstances, adequate to supposedly absolute positions in space. Adequacy breaks down only when the points of anchorage become indeterminate, as in the illusions of absolute movement that occur when two railroad trains are moving relatively to one another, or when points of anchorage are eliminated as in the complete dark. However, Koffka notes that a point of light in the complete dark may maintain its position for a while because the "spatial level" from the light persists; nevertheless, if complete darkness be continued, it presently wanders about, and, presumably, ultimately should lose absolute position entirely.

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[5] Thus the tendency of this study is to refute the general conclusion of Dunlap, who wrote: "It is safe to say that in the normal subject sensations whose peripheral processes are simultaneously excited are always perceived as simultaneous, unless the duration of one is much less than that of the other." *Op. cit.*, 190.

[1] Commuicated by Edwin G. Boring. [2] K. Koffka, *Psych. Bull.*, 19, 1922, 570-578.

With this relativistic theory there are certain obvious difficulties. The "spatial level," divorced from the experiencial anchorage, is hardly more meaningful than other vague concepts like "attention" and "attitude" against which Koffka inveighs.[3] But, granting that the "spatial level" means nothing more than that the effect of the points of anchorage is conserved after the points have disappeared from phenomenal experience, we are still faced by the difficulty that gross differences of spatial position would seem to remain absolute. It is hard to believe that a man, no matter how long he had been in the complete dark, would not be able to tell immediately whether an illuminated letter was right side up or upside down, even though the letter were exposed tachistoscopically without eye-movement.

The proper test of these considerations, however, lies in experiment and the present study therefore seeks to determine how accurately a faint line of light lying in the complete dark can be adjusted vertically, and whether there is any change in accuracy after the observer has continued a long time in the dark and the "spatial level" established in the light has become, presumably, instable.

The experiment requires a line of light that can be rotated by the *O* in a vertical plane so that the *O*, working under the method of average error, can adjust the line to the supposed vertical. For this purpose a metal cylinder, 25 cm. long by 20 cm. diam., was mounted on a bearing so that it would rotate about its horizontal axis. Within the cylinder was a 15-watt frosted Mazda light to which the current was supplied through slip rings. In the end of the cylinder away from the bearing a slot was cut, 100 mm. by 1 mm., centered upon the axis. It was necessary to cover this slot with two pieces of milk glass, each 4 mm. thick, and one piece of white paper. This screen gave what seemed at first entrance into the dark a distinct but faintly illuminated line, slightly yellowish. Under dark adaptation it became more intense, and less drastic screening let through enough light for objects in the room to become faintly visible before the close of an hour's dark-adaptation. A cord was fixed to a large pulley close to the bearing, and the two ends of the cord were brought about other pulleys to *O*, who sat ten feet from the apparatus, facing it squarely with the axis of the bearing on a level with his eyes. By manipulating the cords *O* could adjust the line to the vertical, and *E* could then read off the setting from an angular scale of degrees attached to the back of the cylinder.

The room was completely dark. After an hour's adaptation it was not possible for *E* or any of the *O*s to distinguish any objects at all. *E*, however, had to have light to read the settings, and this reading she accomplished by having *O* shut his eyes while, with the aid of a pocket flash-light, she read the scale and set the cylinder for the next trial.

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[3] *Op. cit.*, 535 f.

In nearly all the experiments *O*'s head was kept rigid by the use of a biting-board consisting of sealing-wax impressions of the *O*'s teeth on metal plates fastened to a frame screwed to a table. In making these impressions the *O*s (with the exception of *R*) bit down through the sealing-wax to the metal of the plate, so that the metal showed at the bottom of the impressions. In this sense the axis of the head may be said to have been perpendicular to the plane of the metal plate. After the experiment was over, it was discovered that the plane of the plate was about half a degree off from horizontal, so that the head was twisted from the vertical of the earth half a degree in the direction that is represented by a counter-clockwise rotation of the line. *R* did not bite evenly and her head was very slightly inclined to the other side of the vertical.

A series in this experiment consists of 60 adjustments of the line to the absolute vertical. In such a series the line was presented to *O* rotated in one direction or the other from the vertical. There were ten presentations at each of the following initial positions: 10°, 20°, and 60° counterclockwise displacement, and 10°, 30°, and 50° clockwise displacement. The *O*s were Miss H. Peak (*P*) and Mr. J. R. Butler (*B*), graduate students of psychology, and Miss S. A. Stone (*S*) and Miss M. S. Russell (*R*), undergraduates concentrating in psychology.

The results are shown in the Table. The principal results are for *B*, *P*, and *S* with the biting-board.

In these trials three series (180 adjustments) were made in each of four experimental hours. The *O*s, closing their eyes when *E* used the flashlight, remained in complete dark continuously during the three series. These results are shown in the Table as averages for the three successive series, so that the persistence of the initial orientation can be determined by comparing successive periods. The three series required nearly an hour for their completion.

The main portion of the Table is concerned with *B*, *P*, and *S* using the biting-board. Here the constant errors from the true vertical are all small (averages for series are all between 0.67° and 3.58°) and are all in the counterclockwise direction. There are no striking individual differences: the individual averages are 1.68°, 1.99°, and 2.47°. In general, then, the constant error of localization for these *O*s under these conditions was about two degrees (2.05°) in the counterclockwise direction.

It is not clear why the errors should be consistently counterclockwise. It will be remembered that the biting-boards of these *O*s were twisted about one-half a degree counterclockwise, so that the error from a perpendicular to the biting plane is only between one and two degrees for all *O*s. This slight displacement of the head from the vertical of the earth does not, however, seem sufficient to explain the constant counterclockwise error. Nevertheless it should be noted that these same *O*s without the biting-boards gave smaller constant errors with the average practically vertical with respect to the earth (slightly clockwise, −0.14°); and that *R*, whose head was inclined very slightly clockwise by the biting-board gave an average error slightly clockwise (−0.45°).

The size of the constant error seems to have no significant systematic relation to the length of time that the *O* has been in the dark. It is true that in the general averages there is a slight increase in the constant error from period 1 to period 2 and from period 2 to period 3 (1.81° to 2.12° to 2.21°). Only one of the three *O*s, however, shows a consistent increase of this sort. Moreover, the two differences (0.31° and 0.09°) are very small and respectively only 0.8 and 0.2 as large as their mean variations. The 'probabilities that these differences are not due to chance' are only about 48% and 14% respectively.

If the "spatial level", persisting in the dark from the light, becomes less stable as the *O* continues in the dark, we should expect the change to show in the variable error of localization. There might be no significant

TABLE: VISUAL LOCALIZATION OF THE VERTICAL

Av. errors of localization of the vertical in degrees. Counterclockwise errors are shown as positive; clockwise errors as negative. The vertical with respect to the earth is represented by 0. The variable errors of the last five rows of figures are average mean variations and not mean variations of the respective averages.

With Biting-Board

Session	Period	Constant errors of localizations (60 adjustments per period)				Variable errors of localization (M. V.'s) (correspond to const. errors.)				Const. errors	Var. errors
		B	*P*	*S*	Av. of 3 *O*s	*B*	*P*	*S*	Av. of 3 *O*s	*R*	*R*
1	1	1.80	0.67	2.03		1.16	0.85	0.91		0.03	2.59
	2	2.20	1.12	1.88		0.97	0.84	0.88		0.07	2.96
	3	1.90	1.85	1.34		1.16	0.99	0.99		−1.18	2.04
2	1	3.00	1.87	1.84		1.10	0.66	0.79		−0.73	2.72
	2	3.53	2.60	1.22		1.36	0.65	1.01		0.48	2.65
	3	2.60	1.87	1.97		0.85	0.73	0.77		1.65	2.37
3	1	1.95	1.31	2.33		1.13	0.95	0.71		−1.91	1.27
	2	2.99	0.98	2.49		1.04	0.92	0.83		−1.06	1.38
	3	3.58	1.71	2.22		1.48	0.72	0.85		−0.92	1.66
4	1	1.10	2.02	1.82		1.01	0.63	0.90		−0.64	1.51
	2	2.31	1.55	2.52		1.19	0.66	0.94		−1.00	1.45
	3	2.76	2.62	2.24		1.24	0.94	0.92		−0.44	1.34
Avs. of 4 Sessions	1	1.96	1.47	2.01	1.81	1.10	0.77	0.83	0.91	−0.79	2.02
	2	2.67	1.56	2.03	2.12	1.14	0.77	0.92	0.94	−0.38	2.11
	3	2.69	2.01	1.94	2.21	1.18	0.85	0.88	0.97	−0.22	1.85
Gen. Av.		2.47	1.68	1.99	2.05	1.14	0.80	0.88	0.94	−0.45	2.00

Without Biting-Board

Cf., these results with avs. for period 1 above	Constant errors of localization (120 adjustments)				Variable errors of localization (M. V.'s)			
	B	*P*	*S*	Av. of 3 *O*s	*B*	*P*	*S*	Av. of 3 *O*s
Gen. Av.	0.93	0.17	−1.52	−0.14	1.30	0.93	1.04	1.09

change in the constant error, but there should be an increase in the variable error. The variable errors for *B*, *P*, and *S*, however, show no more significant relation to the period than do the constant errors. The general averages indicate a very slight consistent increase (0.90° to 0.94° to 0.97°). Again, it is true that only one of the three *O*s taken separately shows the same consistent increase as do the averages. The two successive differences are only about half as large as their mean variations, and the 'probability that they are not due to chance' is, if the usual statistical procedure is considered valid with so few cases, only 30%. The reader will get the same impression from the Table by inspection of the cases which yield these averages. We can not assume, therefore, that there is any demonstrated increase in the variable error of localization of the vertical as *O* continues in the dark.

At the right of the table the results for *R* have been separated from the rest for the reason that she failed to obey instructions to shut her eyes when the experimenter used the flash-light. If between adjustments she reëstablished the "spatial level," one might expect perhaps a better performance. The constant error, it is true, practically disappears: the very small clockwise error is not significant as against its mean variation. The variable error, however, lies within the range for the other *O*s. The diminution of the constant error as against the other *O*s is not necessarily significant because this *O*'s head was tilted clockwise, whereas the other *O*s' heads were tilted slightly counterclockwise. It is therefore not even clear whether the difference is due to the difference in orientation of the head or to the difference with respect to the closing of the eyes. Hence little is to be learned from the results of *R*.

Unfortunately time did not permit the taking of series in dim light for comparison with those in the dark. It would be interesting to know whether the visibility of vertical and horizontal objects, establishing a spatial frame, would give any improvement at all over results in the complete dark.

The last line of the table gives some results without the biting-board. Variability remains as great as it is with the biting-board, but the constant error is reduced. These results, taken together with the results of *R* and with the fact of the consistent counterclockwise error of *B*, *P*, and *S* with the biting-boards titled half a degree counterclockwise, make it seem as if the orientation of the head, as Müller has stated,[4] affected the amount and direction of the constant error.

Conclusion. Because of the small size of the errors for the visual localization of the vertical in the dark and of the failure to demonstrate any increase of the variable error as the *O* continues in the dark, it would seem that this localization must depend almost entirely upon the absolute vertical and the position of the body, and not to any appreciable extent upon points of anchorage in the visual field or the conservation of such a frame of reference after its perception has been abolished by the dark.

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[4] Koffka, *op. cit.*, 572 ff; G. E. Müller, *Zeits. f. Sinnesphysiol.*, 49, 1916, 109-244.

BOOK REVIEWS

Psychology; A Study of Mental Activity. By HARVEY A. CARR. New York, Longmans, Green and Co., 1925, pp. iv, 432.

The sub-title gives the key to the book; psychology is primarily a study of mental activity. Illustrations of mental activity are "perception, memory, imagination, reasoning, feeling, judgment and will" (1). The reader is nowhere told what makes an activity 'mental,' possibly because the psychophysical question is a matter for the philosophers (5-7). We learn, however, that one's own mental operations may be subjectively apprehended and the mental operations of other individuals may be objectively apprehended (7). "Mental acts can also be studied from the standpoint of anatomy and physiology. The structure of any organ and its functional possibilities are intimately related" (10). From the above and similar statements it is not clear whether mental activity is merely a form of movement of living organisms, however observed, or whether it is something else. In the body of the book further light is thrown upon the nature of mental activity. "Mental acts involve an expenditure of sensory, neural and muscular energy, and obviously this energy must be supplied by the vegetative processes" (260). "Mental acts also directly influence the vital processes by means of their neural connections" (266). "[An] organism is a reactive unit, and the mental and vital activities are so intimately related that they are continually influencing each other in various ways" (277). Thus mental activity is in some manner related to physiological processes, but it is not identical with them. The naïveté of the author upon the psychophysical problems is shown in the following quotation from a discussion of auditory localization: "The student must not confuse the physical and the psychological principles involved. A person hears a sound as loud because of its nearness, but recognizes its near location because of its loudness. The first statement expresses a law of physics, and the second a principle of psychology" (144).

The term 'mind', we are told, "is used when we wish to characterize an individual from the standpoint of his intellectual characteristics and potentialities" (4). "The mind is also continuously evaluating the various aspects of experience. The mind not only labels things as good, bad and indifferent, but it also arranges the good things of life in a crude scale of relative worth" (3). In discussing volition the mind is regarded as a purposing, directing, supervising agent. "Voluntary activity is that which is performed in accordance with a person's wants, desires and wishes; it is purposeful and intentional action" (309). "Will may be defined as the intellectual control and supervision of activity in relation to some approved or undesirable end" (311). "Any type of activity may be subject to volitional supervision" (312). "Reasoning may now be defined as *volitional* or purposive thinking" (312). "The term 'volitional control' of activity may thus be defined as an initiating, inhibiting, directing, energizing, or selective influence that is exerted in the interest of attaining some end or of avoiding those things which we dislike" (314). We are told in a section upon Will Power that "A person of strong will power is one whose mind dominates his actions" (320). Mind is dynamic. Motives, for example, are "the directive forces that determine what we do" (73), and "an idea, like perception, is a dynamic as well as a cognitive process" (183). "[Such] an idea may possess a sufficient dynamic strength to disrupt the act in question, and arouse a response adapted to the situation which that idea represents" (183).

The wholly naïve and uncritical standpoint of the writer is shown in various ways. As a single example consider the loose use of the term stimulus. One reads "A sensory stimulus may be defined as any extraneous condition that actually does affect a sense organ" (70); then further on that "hunger, thirst, and internal pains are very powerful stimuli that largely determine the nature of the organism's reactions" (71-72). Again one is informed that "sometimes the same stimulus serves as both the motive and the goal of a response" (73). Earlier in the book reference was made to "warm stimuli" (53), and to Hering's assumption about "light stimuli that enter the eye" (55). "In perception, the sensory or imaginal stimulus is attentively examined from the standpoint of its significance for conduct, while in thought it is *what* this stimulus means that is regarded as the potential object toward which to react" (175). Owing to lack of rigorous criticism and definition the term 'stimulus' is used now in the sense of phenomenal object, now in the conceptual sense of physical energy, and now in some other manner.

Following the introductory chapter, which gives the author's standpoint, a second chapter presents some elementary facts of neural anatomy for "the connection between mental acts and the nervous system is a matter of common knowledge at present" (16). The cerebro-spinal nerves are "primarily involved in mental reactions" (17). The neurological material, however, is not utilized in the latter part of the book nor is its relation to the major problems of psychology adequately discussed. A chapter upon sensory and motor equipment includes a discussion of sense organs and of sensibility. More than twenty-two pages are devoted to a discussion of sensory equipment and a single page at the close of the chapter is given to motor equipment, which latter makes no reference to glands and smooth muscles and treats the other effector organs very inadequately. Among other things we are informed that there are eight kinds of sensitivity including static, cutaneous, kinaesthetic, and organic (44-45).

"Perception may be defined as the cognition of a present object in relation to some act of adjustment" (110). Later the teleological implications of this definition are made explicit—"objects are perceived as potential goals or objectives toward which to react" (132). Carr identifies the context theory of meaning with the view that images are necessary to meaning (124-125) forgetting that, according to Titchener, images may drop out and meaning may be given by brain habit.

The chapter on The Organic Background of Mental Life is one of the better parts of the book. "An organism exhibits two groups of activities—those that are concerned more or less directly with the adaptation of the organism to its environing circumstances, and the vital or vegetative or physiological processes that are primarily concerned with the maintenance of the structural identity of the organism" (260). The chapter concerns itself chiefly with the latter. Vital processes furnish some of the motives of life—hunger, thirst, sex and the need for a constant temperature. The discussion of emotions mentions anger, fear, disgust, joy and surprise. The single paragraph upon joy is a weak substitute for discussion of sex emotions; the book under review is almost wholly sexless. James' theory of emotion is, of course, discussed. Carr believes that emotions should be classified in terms of behavior situations rather than in terms of organic sensations. His treatment here (277 f.) is original. "According to our conception, there are just as many kinds and varieties of emotions as there are distinctive behavior situations, and one emotion is just as unique and primary and fundamental as another" (281). In a chapter upon the affective and evaluative aspects of experience Carr defends a view of affection which he calls the judgmental conception. Over against certain other views of affection "the judgmental conception assumes that the sensory object and the organic reactions that it elicits are the only observable contents that can be detected in the search for an affective element" (299). This point of view appears to avoid the question of affective processes in the term 'observable contents'. Possibly the kind of observing which yields sensory objects and organic reactions is only one type of observing. To rule out at the start the possibility of non-sensory affective report is to avoid the question at issue.

A critical discussion of the criteria of native characteristics is a refreshing substitute for the usual instincts. The author has examined a number of text-book definitions of instinct and finds an unsatisfactory state of affairs (380). The difficulties with the concept of instinct are pointed out and the statement is made that instinct is not a useful concept (384).

The final chapter deals with the measurement of ability. The chapter is a sane, balanced, conservative discussion of tests; but it does not enter into the actual procedures very much.

One general criticism of the book is that it expresses a common-place philosophy of mind. The treatment of ideas is throughout epistemological. The chapter on the self may be characterized as a practical philosophizing about a number of things. In the first chapter we are told that "facts of common observation constitute perhaps the major portion of the factual data upon which the present conceptions of psychology are based" (11). The psychological materials are traditional for the most part. Thus in the chapters dealing with practice and memorization, retention and recall (which are among the better parts of the book) one is not surprised to meet once again Bryan and Harter's curves for learning telegraphy and Book's curve for learning to typewrite (220-221).

From the standpoint of the professional psychologist Carr has presented a psychology which is a naïve expression of the every-day philosophy of the individual and his mind, tempered here and there by common-place facts from the psychological laboraotry. This simple, uncritical functionalism is a good beginning for serious study, but the necessity of rigorous criticism remains, and until this task is taken up seriously the type of psychology presented by Carr should hardly be dignified by the name 'science.'

From the standpoint of the beginning student the picture is different. The book is understandable and readable and in most respects very satisfactory. The illustrations in Chapters II, III and VII are well selected and clearly printed. The book aims "to present a clear, matter-of fact and concise statement of general principles with just enough illustrative material to enable the student to comprehend those principles." From the pedagogical point of view the book is adequate and the very fact that it is uncritical, naïve and common-place commends it as a text for beginning students. At the close of each chapter questions and exercises are added most of which connect the topic in hand with the student's knowledge and practical interests and are thus valuable in introducing the subject. There are a good many cross-references within the pages; and the listing of other English texts at the close of the chapter also adds to the usefulness of the book. The great mass of factual material is sound and well presented.

University of Illinois PAUL THOMAS YOUNG

Principles of Psychotherapy. By PIERRE JANET. Trans. by H. M. and E. R. Guthrie. New York, The MacMillan Co., 1924. pp. v, 314.

We have in this little volume a very readable and instructive digest of some of the earlier works by the same author on psychotherapy. The book is divided into three parts: the first is historical, dealing with the origins and evolution of various kinds of mental treatment; the second part takes up the principles and laws of the psychological phenomena confronting the psychiatrist; the third deals with conditions under which methods of treatment can be successfully applied. After reading the first part one feels that the early animal magnetists and hypnotists are still worth study for they were forerunners of later methods of treatment which rest upon a more secure scientific foundation. Janet points out the close relationship between himself and Freud, claiming that the latter derived his fundamental concepts from him. The various therapies deriving from the early non-scientific movements are known today under such names as isolation cures, re-education, New Thought, Christian Science, morale therapy, and under more scientific cloaks. Nor does Janet overlook the psychophysiological causes and treatment of the various neuroses.

In the second part on principles Janet points out that the psychological concepts in current diagnoses are very hazy. Excellent medical and psychological observations may suffer from false interpretations or ignorance of the real causal factors involved in the etiology and cure of neuropathic conditions. A more adequate psychology would undoubtedly help the psychiatrist as well as workers in other practical and theoretical fields. The misuses of suggestion are pointed out and at the same time the legitimate sphere of hypnosis is indicated. Probably the psychology of behaviorism has not been without influence in some of the formulations in this book: the neuropath is regarded as a being whose maladjustments are largely matters of behavior. The neuropathic organization is a bankrupt concern whose assets must be carefully mobilized. Useless expenditures of energy causing fatigue and depression are to be avoided. The output of energy must be carefully guarded. On the other hand, psychic income must be increased by various forms of stimulation.

The applications of psychotherapy dealt with in the third part of this book will be of most interest to the person interested in what so-called psychological methods can actually accomplish. Certainly a saner and fairer estimate could hardly be made than appears here. The efficacy of psychotherapeutic treatments depends upon the diagnosis. The use of one set of concepts for all cases is as foolish and disastrous as would be the random prescription by one physician of arsenic to all patients and by another of mercury to all of his patients. The distinction between functional and structural diseases Janet no longer finds useful. Nor is the concept of the subconscious to be promiscuously employed. The psychiatrist must approach his problem with the intent to discover what is there as a matter of fact and not as a detective or moralist with preconceived notions as to what he should find. In any case it is impossible at present to say whether a physician should order a psychic outlay or a rest cure: each case must be decided upon its own merits. There is still too much random diagnosis and advice.

Throughout the third part of the book the interesting concept of psychic tension is used. A more exact definition of it would be very helpful. The reviewer suggests that one might envisage the normal psychic tension which enables one to expend energy without evil effects as due to a kind of buffer substance in the nervous system which acts in somewhat similar fashion to those salts in the blood which prevent it from becoming too acid or alkaline. The normal psychic tension may be regarded as a state of equilibrium which, when upset, is restored automatically and autonomously in a self-sufficient organism. When the organism can no longer make normal expenditures and restore its equilibrium without outside help the aid of the psychotherapist must be sought. He may help set into operation what normally are internally regulated mechanisms.

This brings us to the problem of psychotherapy proper: does psychotherapy refer merely to mental mechanisms or can the effects produced by physiological means be classified under psychotherapeutic methods? Janet believes that the science of psychotherapy, while dealing predominantly with psychological facts and their laws, can apply to any treatment which ultimately influences what may be called psychological data. The treatments used by psychotherapists are gradually emerging from the moralistic, religious, and philosophical backgrounds in which they have been submerged. The insane person can no longer be regarded as an exceptional, unique being to be isolated from the rest of society, incurable. Mental disorders should be treated like all others and no greater fatality should be ascribed to them than to the so-called physical disorders. The progress of psychotherapy requires a more adequate psychology dealing with the real needs and behavior of the human organism.

The reviewer has not compared the translation with the original French version of the book, but judging by the way in which the English reads, there is no doubt as to its excellence and correctness. It is to be hoped that both beginning students and the laymen who come to psychology and psychologists asking questions concerning the status of hypnosis, Christian Science, the nature of mental diseases and their cures will avail themselves of this simple presentation of the value and character of modern psychotherapies.

University of Illinois HARRY HELSON

The Nature of Intelligence. By L. L. THURSTONE. New York, Harcourt, Brace, and Co., 1924, pp. xvi, 167.

Like other books on the nature of intelligence, this one tends to broaden into a system of psychology. Whereas Spearman's attempt to define intelligence is dominated by logic and results in an epistemology, Thurstone's treatment of the subject takes its inspiration from biology and approaches a system of psychobiology. After all, the subject of intelligence belongs to a field which draws its facts from psychology, behaviorism and biology, and which might well take the name of psychobiology or of 'biopsychology.' Intelligence is a matter of the living organism with a functioning mind.

Thurstone takes from the point of view of psychiatry and psychoanalysis the notion that action, whether normal or abnormal, begins with the desire or want of the individual, and not with the environment. The individual is not merely a stimulus-response mechanism. The traditional order is thus reversed; it is not 'stimulus—mind—response,' but 'human want—stimulus—response—satisfaction.' The human drives or purposes are always prior to the stimuli which release the action and condition the details in which satisfaction is gained.

Every mental state is unfinished action or action which is being formed. A psychological act begins with a desire (which is vague and ill-defined), proceeds through conception (at first by way of concepts of the higher order—also ill-defined), then through ideation, and then (as stimuli are found and sensations are contributed to the total) to perception, which is definite enough to pass into specific action. The movement is therefore from the general to the specific, from the universal to the particular. In the genetic development of mind, the point at which the psychological act becomes focal (conscious), has been successively pushed back from the end of the act toward the beginning. In the evolution of mind, therefore, we may find various distinct stages, perceptual intelligence, ideational intelligence, and in the highest stages, conceptual intelligence. The tendency has been to push the point at which the act may become focal back from the particularizing processes, to the universalizing processes of higher order. In the highest type of mind, the higher universals are cognized and expressed in overt action. "The development of intelligence," says Thurstone, "is measured by the incompleteness of the motive at which it can become focal."

But there is another factor which cuts across intelligence, if it does not actually oppose it. This is the ideomotor tendency or the pressure to complete the psychological act overtly before it has become focal. The force of this factor depends upon the strength of the motive, or the urgency, or degree of dissatisfaction. Intelligence becomes here an inhibiting force "superimposed upon the ideomotor tendency." Inhibition of a motive at an early stage allows for mental trial and error rather than overt trial and error.

We have in this book a bold and ramifying set of hypotheses for the present state of our knowledge. The ideas set forth are exceedingly suggestive and stimulating, yet, unless one is cautious, they lead one into those speculations and vague generalities of rationalized human behavior for which the Freudians have been severely criticised, and the author does not entirely escape. He also occasionally depends upon argument by reiteration of his thesis where facts are lacking and makes some rather sweeping statements to prove his point, as, for example, "most of the stimuli that we encounter in our everyday life are actually looked for." There is also a tendency to define the same term, *e.g.* 'ideation,' 'perception,' and even 'intelligence' in more than one way wherever it seems useful to do so.

But when all these things are said, one cannot help but admire the attempt to set up a serviceable working basis for investigations of intelligence, theoretical or practical. All who are engaged in the invention of mental tests should read this book. Those who are interested in educational psychology will find besides the theoretical discussion of intelligence, brief but interesting treatment of such topics as 'trial and error,' 'autistic thinking,' and 'genius.' For those who are looking for fundamental points of view, the most significant thing is the introduction of motive into the stimulus-response organism to which the subject matter of educational psychology has become unfortunately reduced. Instead of studying merely the bonds between stimulus and response, we would, from this point of view, seek for the normal human motives and the way in which stimuli serve these motives in reaching satisfaction.

Cornell University J. P. GUILFORD

Problems of Citizenship. By HAYES BAKER-CROTHERS and RUTH ALLISON HUDNUT. New York, Henry Holt and Company, 1924, pp. xi, 514.

For some years the thought has been gaining ground that the student's introduction to the study of social science in college should be through a general course organized around important problems requiring the utilization of materials from history, government, sociology, economics and the other special social sciences. In several colleges such courses are now being offered. "Problems of Citizenship" is the result of several years experience with an introductory course in the social sciences in Dartmouth College. The authors have aimed to present a limited number of important problems of immediate concern and interest to the student, with which he has a degree of familiarity, about which he discovers different points of view, and the controversial nature of which will stimulate class discussion and offer incentive to independent, constructive thinking.

In accordance with their aim the authors have organized the content of their book around eight major problems: (1) The Newspaper Problem; (2) The Immigration Problem; (3) The Negro Problem; (4) The Woman Problem; (5) The Industrial Problem; (6) The Problem of Civil Liberty; (7) The Problem of International Relations; and (8) The Problem of War and Peace. In their discussion of the specific problems under each major heading the writers have presented a number of points of view and have not advocated any particular one. In this difficult undertaking they have succeeded admirably.

In general it would appear that the authors had shown rare discrimination and wisdom in their choice of problems for an introductory course in the social sciences. They are all questions of interest and great importance. One wonders, however, by what slip they could have failed to include the problem of public education. This is a national problem of vital and immediate concern to everyone whether child or adult. The country is embarked upon a program of education beyond any heretofore attempted. The problem of education has intimate bearing upon every other problem of social and economic importance and its successful solution depends upon the sympathetic understanding of every citizen. Its understanding is essential to good citizenship; yet, it has received but scant and incidental attention.

The book is well organized and indexed. It contains a valuable bibliography to guide the student in his further reading and study. In a pioneer attempt to organize in textbook form the content of a general course in the social sciences the authors have been remarkably successful and have undoubtedly produced a volume that meets a real need. One weakness of the book seems to be too much content which has necessitated the use of type too small to be read with comfort. With a list of rather specific guiding problems in connection with each major topic and suggested readings from sources indicated in the bibliography, the length of discussions might well have been reduced. One of the results of such a course should be, probably, the development of habits of reading, gathering and evaluating materials and ideas from varied sources.

Cornell University E. N. FERRISS

The Basis of Social Theory. By ALBERT G. A. BALZ, with the collaboration of William S. A. Patt. New York, Alfred A. Knopf, 1924, pp. xxx, 252.

This volume according to the author, "seeks to outline an approach to Social Psychology and to examine a number of conceptions that seem fundamental to the subject." Professor Balz acknowledges his indebtedness to Professor Dewey both in the preface and elsewhere and a perusal of the various chapters reveals a harmony with Dewey's "Human Nature and Conduct." This does not mean, however, that he who has read Dewey has nothing to learn from Balz.

A brief consideration of the concept of Progress is first introduced in which it is conceded that the idea has been given "practically universal acceptance" in "Western life." This all-but-universal acceptance implies an uncritical attitude, a mere faith which may prove to be a handicap. No attempt is made to define the concept but the contention is made that, "whether Progress be 'real' or not, at all events the *belief* that Progress has been made and that there is a real possibility of more to be made is a genuine and undeniable fact." This belief is held to be a potential power whereby Progress can be made. It is a faith of confidence in contradistinction to oriental fatalism. "The ills that beset life, whether catastrophic or not, are regarded as preventable." "Passive acceptance is regarded as inhuman, degrading, 'unprogressive.' Mind is stimulated, not chastened, in the face of calamity." Due account is taken of the backwardness of the social sciences as compared with the natural sciences. The complexity of the social science subject-matter and the difficulties encountered in experimentation account for this state of affairs. This condition is due to the "absence of an adequate foundation" and this foundation is to be found in man himself or in other words "knowledge of human nature is the indispensable basis for every social science." This volume is obviously designed to assist in meeting this need.

Chapter I deals with "The Field of Social Psychology." No one will disagree with the point of view that there is need for sharper definition in this field, if not in the whole field of the social sciences, but one can hardly expect any such universal acceptance of the identification of "social psychology with psychology." The chapter presents a worthy critique of the "individual" and the "social" with special attention to the latter.

Chapter II is offered under the caption of "Human Nature and Social Forces." Here the original nature of man and the innate tendencies which serve as the "social forces," are considered. Then follow in order chapters on Inherited Tendencies and Action, The Function of Instinctive Tendencies, The Function of Capacities and The Problem of Control. The point of view of the "Conclusion" seems to deviate farther from the scientific attitude revealed in the earlier part of the book, than one might anticipate. For example, in the concluding paragraph we are told "the transformation of human life as a biological fact to that life as humanly social is the achievement in the final analysis of mind." Although citations are carefully made a bibliography would not detract from the serviceableness of the volume. On the whole it must be said that the author has succeeded in his purpose and by his contribution has furnished an added stimulus to psychology to bestir itself further in rendering aid in the solution of problems in the realm of the social sciences.

Cornell University CLYDE B. MOORE

Matter, Life, Mind and God. By R. F. ALFRED HOERNLÉ. New York, Harcourt, Brace and Co., 1922, pp. xi, 210.

Five lectures designed to bring to intelligent readers ("not professors and experts") an understanding regarding the ideas of leading thinkers of the day regarding the four subjects indicated by the title. A positive attempt is made to find a synoptic view of the manifest diversities of opinion. Lecture I is a discussion of the diversity between the quantitative view of exact science and moral, religious and aesthetic values. He exhibits the tendency of science to become a moral code and an arbiter of progress. But science claims not only to give practical mastery but attempts to yield a world view. Whitehead's thesis that nature is closed to "moral and aesthetic values" is regarded as a narrower view of nature than that of daily experience. This contrast provides a problem for 'synopsis.' The author finds reconciliation in the fact that the quantitative view is necessary to scientific method, but that philosophically, even Whitehead acknowledges that *value* is the proper key to a synthesis of existence. J. A. Thompson's view that feeling is essentially synoptic—that it "sees things whole"—is indorsed.

Chapter II is an instructive summary regarding the present dilemma concerning the nature of matter. The problem of the existential status of sensa and the views of Whitehead are presented in non-technical language, despite the intricacy of the subjects. In Chapter III the issue of mechanism in biology is surveyed, and Thomson, Loeb, Henderson, Bergson, Driesch and others are allowed to have their say. He rejects mechanism as failing to obtain in the sphere of quality, which is in a sense a higher order, since it depends on specific combinations of the lower elements of molecules and electrons. He is favorably inclined to Henderson's account of the meaning of teleology, because it is an account which does not create a bifurcation between the sphere of value and the mechanical world. Henderson argues that life is made possible by systems of factors in nature. These (selected environments) cannot be accounted for by mere chance; they are a 'preparation' for life. In harmony with Hoernlé's idealistic tendencies, this view embodies both life and physical nature in a form of purposiveness which transcends both of them. To the reviewer, he does not adequately treat the possibility that the 'fitness of the environment' is really accidental; nor does he meet Bergson's argument that the 'purposiveness' of mechanical nature is but another baptism for that mechanism itself. Mechanism reduces the laws of chance, but chance by definition excludes the connivance of *choice*.

Chapter IV is a review of contemporary psychology in the interests of a synoptic view of mind. Hoernlé excludes the present chaos of views upon this subject as due to the newness, the wide ramifications, and the inherent difficulties of psychology. He finds promise of agreement regarding the nature of mind in the theory of evolution—shared by nearly all psychologists; and in the concept of behavior (in the broad sense)—whose chief virtue is that it enables us to escape the dilemma which arises when we describe activity exclusively in mentalistic or in physiological terms. Behaviorism, however, only ignores the problem—it does not solve it. Hoernlé also maintains that we should drop the notion of introspection as always involving a 'looking within,' for we normally report observations concerning ourselves without such an assumption. Introspection, thus regarded, is on the same level as observation of any other kind. Chapter V is on the meaning of God.

Wells College C. O. Weber

Introduction to Philosophy. G. T. W. Patrick. New York, Houghton Mifflin Co., 1924, pp. viii, 463.

This book was written to serve as an introductory text in philosophy for college and university students, and as a guide book for general readers. The author sets forth no system of philosophy, and includes personal views on crucial issues, which, however, often appear as footnotes. Despite the Freudian sophistication which appears in the author's book on *The Psychology of Relaxation*, he here avers that a book may be personal and still impartial. At any rate, the author is impartial in the essential sense that he gives a hearing to all views. In truth, the number of problems and views surveyed are if anything too numerous for an introduction to philosophy. The author postpones the whole subject of epistemology to the closing chapters for the sake of avoiding confusion in the minds of beginners. One notes a failure to prepare the chapters on ontology and cosmology in a way that leads to the problem of knowledge as bearing on them. The author confesses inclinations towards realism, pluralism, idealism, and optimism. He also acknowledges a frank eclecticism, saying, "it is time to call attention to the agreements rather than the disagreements in philosophy."

There is an obscurity in the first chapter regarding the definition of philosophy. On one page he avers that philosophy gives us knowledge of meanings and values, which is very welcome after science has surfeited us with facts, "now, philosophy has just this for its task—to try to answer these insistent and persistent questionings of the human mind, as to the use, meaning, purpose and value of life" (p. 1). Yet, on another page (p. 16), he makes a twofold division of philosophy; one division is concerned with meanings, purpose and value, while the other is concerned with the analysis of scientific and common-sense concepts.

The chapters of the book follow logically from his analysis of the problems of philosophy. In the first chapter, enitled Cosmological Inquiries, he considers the cosmos, space and time; the nature and origin of life; the philosophy of evolution; the question "Is the world purposive?"; the problem of God; and the problem of evil, *i.e.* pessimism. In the second chapter he discusses the ontological questions of monism, dualism and pluralism. In the third chapter he treats of the philosophy of mind, including sections on the search for the soul, the relation of soul and body, and the freedom of the will. In the fourth chapter he writes on the theory of knowledge, and in the fifth on the higher values of life—morals and aesthetics.

Wells College C. O. Weber

Psychological Tests in Business. By A. W. Kornhauser and F. A. Kingsbury. Chicago, University of Chicago Press, 1924, pp. ix, 194.

This is a moderate statement of the extent to which tests have proven useful in business and a clear exposition of the general principles governing the construction and use of tests, the authors pointing out the limitations of tests as well as their possibilities. While the book was written for students of personnel practice and business executives it should be valuable as supplementary reading for students interested in vocational education and vocational guidance. Chapter I, the Nature of Psychological Tests, Chapter V, the Place of Tests in the Personnel Program, and Chapter VI, the Outlook for Tests in Business, are likely to be most easily read by the general reader. The reviewer believes that Chapters II, III, IV, which treat of test construction and the studies made of the actual use of tests, will carry their message best to those who have some knowledge of specific tests, who have done some reading on individual differences in recent psychological literature, and who are familiar with common statistical terms and methods.

A psychological test is defined as a sample of one's behavior used to indicate his abilities or other tendencies and hence to predict his probable future behavior. In constructing tests human traits must be studied in relation to the requirements of the particular job. The authors caution the reader that we are in the early experimental stage of this task. They tell us that fairly good results have been obtained in the use of tests of mental alertness and other traits in the selection of clerical help, and in the use of proficiency tests in selecting stenographers. The tests, however, are to be regarded as only one of several methods to be employed in selecting workers and it is dangerous to generalize from the results obtained under particular conditions. Tests for the selection of workers for other jobs are still in an early experimental stage. As to the extent to which tests are being used in business the authors say: "In business, probably, only a fraction of 1 percent of American industrial and commercial establishments have ever seriously tried to use standardized tests, and in very few of these have they passed beyond the experimental stage."

The authors quote the average scores for various occupations which were obtained through the U. S. Army group mental testing (National Academy of Science Memoirs, Vol. XV, p. 160), pointing out some of the limitations of these data. Even with these critical comments the data are likely to mislead the layman. It seems to the reviewer that it would be better not to publish these data at all in a book intended for laymen. They are too much in line with preconceived notions of "superiority," and the reader is likely to accept them at their face value as "occupational intelligence limits." However, we must note again that the authors point out some of the limitations of the data. Others have been less careful.

The reviewer believes that the book represents a sane and well-informed appraisal of the use of tests in business. It probably will not be read by the people that need it most—the people whose hopes run high concerning tests and who through tests would work a kind of magic in the prediction of human behavior.

Cornell University THOMAS L. BAYNE, JR.

An Introduction to Reflective Thinking. By COLUMBIA ASSOCIATES IN PHILOSOPHY. Boston, Houghton Mifflin Company, 1923, pp. vii, 351.

The nine co-authors of this book purpose "to emphasize the part which thought plays in the formation of beliefs, and to stimulate . . . readers to a more lively realization of the road to a more congenial world which lies open to those who think." To illustrate the stages in an act of thought, they take examples of effective scientific thinking from outstanding achievements in medicine, astronomy, biology, mathematics and physics. Methods of treatment of disease in ancient Egypt are contrasted with those in a modern hospital. A lucid exposition of the Ptolemaic and Copernican theories of planetary motion is developed; and the arguments for the hypotheses of special creation and of evolution are presented. The Pythagorean theorem is cited as an example of reasoning in mathematics, and the kinetic theory of matter, in physics. A chapter on historical inquiry, critical and traditional, includes arguments for and against criticism of the Pentateuch.

The later chapters deal with the use of reflective thought in problems of social relations—more specifically, the choice of a career, the making and enforcing laws, the establishment of ethical standards.

The connecting thread, indicated by the title, unifies the sciences and emphasizes the contrast between "reflective thinking" employed in science and that used in the field of values. The pragmatic point of view is furthered by questions and exercises for class use. A brief bibliography concludes each chapter. For psychology, readers are referred to William James and to works on personnel research and educational psychology. The influence of John Dewey's *How We Think* is acknowledged in the introduction.

Of interest to psychologists is the criticism in the chapter on Nature in Explanation: "The thing explained remains as 'real' as it ever was, and scientists who feel otherwise are merely thinking confusedly. Of this error psychology, . . . our newest science, furnishes us perhaps with our most glaring examples. No mental phenomenon is so real as that we call 'will.' Yet the psychologist is prone to analyse that complex thing into its constituent elements, and then calmly to announce that, of course, there really isn't any such thing as will-power; there are only a great number of habits and tendencies."

The book should prove useful as a text for elementary courses in philosophy, or "orientation" courses, for it gathers together material whose sources are widely scattered. The subjects chosen for discussion are well fitted to their purpose, and the liberal treatment allows the reader practice in reflection.

Cornell University FLORENCE B. WOOLSEY

Nature and Development of Learning Capacity. By W. H. PYLE. Educational Psychology Monographs, No. 25. Baltimore, Warwick and York, 1925, pp. 119.

Taken as a whole the book is an excellent presentation of a careful, extensive and very thorough series of comparative investigations upon the development of the learning capacity of school boys and school girls in the United States. The greater part of the book is taken up with reports upon the results of seven tests (card-sorting, marble-sorting, manthanometer, substitution, ideational memory, mirror drawing and general mental test) given to white, negro, Indian and Chinese school children over a period of ten years. Each of the tests, except the mirror-drawing and manthanometer tests, were given to a thousand or more children.

The results indicate that, in the forms of learning called forth by the tests, girls progress faster than the boys, up to, or at least nearly to, puberty. After this time boys progress relatively faster. Rural children have a learning capacity of about 80% of that of city children of the same age and sex. In comparison with white children, Chinese students had a learning capacity of about 80%, Indians 60%, negroes 45%.

The last chapter is devoted to a discussion of the differences between a 'good learner' and a 'poor learner,' together with an unimpressive theory of what is termed 'brain action.' There is little or no connection between this chapter and the experimental results obtained in the rest of the book. The addition of the chapter, however, is all that justifies the inclusion of the term 'Nature' in the title of the book.

The author shows great skill in compressing the formidable mass of his data from tests into such concise form, but unfortunately the treatment of the theory lying back of the tests, as well as the interpretation of the results, is meagre. The reader may find much valuable empirical evidence concerning the development of learning capacity, but he will meet with little concerning the nature of learning.

Harvard University HARRY R. DESILVA

NOTES

SCIENTIFIC INDUCTION AND STATISTICS

There is no act that the scientist is called upon to undertake that is more important or more precarious than the formation of the inductive generalizations that are his scientific conclusions. Generalization is important because there is no science without it. We are told so often that science is descriptive that we forget that it must ultimately transcend description of particulars and conclude to the general. Technology sometimes deals only with particulars; diagnosis, an art, is directed upon particulars; but science seeks to induce generalizations. Scientific induction is, however, precarious because there is not for it the same carefully standardized technique that there is for description. Consequently, nearly every induction starts life as an hypothesis, and becomes a law only if it survives the perpetual process of verification long enough to arrive at maturity.

Some years ago I ventured to express an opinion as to the relative adequacy of statistical method against the problems of description and of generalization.[1] I was discussing at that time the question of the significance of differences, and I suggested that one might need to distinguish between "mathematical significance" and "scientific significance." I suppose the most frequent elementary problem of statistics is the establishment of a difference in tendency between two arrays of particulars. In such a case one analyzes each array into a central tendency and a measure of precision, notes the difference between the two central tendencies, and then determines the "significance" of this difference by considering it in its relation to its measure of precision. The simplest measure of "significance" is the ratio of the difference to its probable error, but this ratio can be converted, if one assumes universal normality of distribution, into a "probability that the difference is not due to chance."

Now my point was that these measures of "significance" are actually only descriptive accounts of the difference in relation to its dispersion and to the number of cases involved, and that, as description, they are prior to the inductive process and that their determination is not the inductive process. When we can say that A is greater than B the process of this phase of description is ended. When, however, we have many A's and many B's and some B's are greater than some A's, we have to resort to statistics to state the tendency for A to be greater than B. Here, I think, we are still mainly at the descriptive level. We come certainly to induction when we wish to assert that all A's tend to be greater than all B's, that is to say, any group of A's will on the average be greater than any group of B's. We have then to decide whether we shall regard our particular A's as a fair sample of all A's, and it seems plain to me that we can not possibly make a conclusion about the relation of the particular group to the general group on internal evidence from the particular group alone.

It is an illusion that this trick is turned in statistics by dividing the probable error of the single observation by the square root of the number of cases in order to give the probable error of the mean. Such a probable error of a mean can not be a prediction of the way in which means of new groups of A's will vary, because the central tendencies of new groups depend, not on anything inherent in the first group, but on the changed conditions that make the groups new and not identical with the old.

It seems to me quite obvious that one may demonstrate a relatively large difference descriptively and still may hesitate to take the inductive step to a generalization because one does not feel sure that his A's do not involve some particular bias of selection as compared with what is meant by

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[1] E. G. Boring, Mathematical vs. scientific significance, *Psychol. Bull.*, 16, 1919, 335-338.

A's in general. Thus I wrote: "The competent scientist does the best he can in obtaining unselected samples, makes his observations, computes a difference and its 'significance,' and then—absurd as it may seem—very often discards his mathematical result, because in his judgment the mathematically 'significant' difference is nevertheless not large compared with what he believes is the discrepancy between his samples and the larger groups which they represent." In criticizing this position Kelley observed: "The procedure described is such as I believe a competent scientist never resorts to. If he will not trust such mathematical findings as are contrary to his wishes, that fact in no sense provides scientific warrant for his discarding them. 'Absurd' hardly characterizes this procedure. Is it not rather a matter of the honesty of the investigator? The intent of this quotation seems to me to give warrant for keeping and using data if they support an established conviction and otherwise discarding them."[2]

I was not arguing, of course, that an experimenter consult his "wishes" in accepting or discarding data, but rather that he consult his judgment. Nor was I proposing that he be negligent about his statistics. He must get his complete analytical descriptions first. These descriptions are his facts, and a fact is a fact. They are not to be disregarded intellectually, but then neither are they all to be printed as if they were psychological conclusions. The more important facts are those that lead to the more assured generalizations and the reading of our journals would be more dreary than it is, if every psychologist printed every fact that he accumulated in his research without regard to its capacity for being subsumed under the generalizations that make up psychology.

I have not before replied to Kelley because my original articles[3] that he criticizes seemed to me to be clear and a sufficient answer to the interested reader. If they were not clear, I did not think I could make another article clearer. Recently, however, I have chanced upon some conclusions that seem to me to afford an excellent opportunity to show that both Kelley and I are right. What is needed is concrete illustration.

Let us take first the question as to whether married negro criminals are less intelligent than unmarried negro criminals, whereas married white criminals are more intelligent than unmarried white criminals, a matter that has recently been dealt with by Murchison and Gilbert.[4]

These authors present data upon criminals in the Maryland State Penitentiary. They have the records in the Alpha examination of 139 unmarried negroes, 99 married negroes, 143 (?) unmarried whites, and 93 (?) married whites.[5] For these four groups they give the percentage distribution by letter-grades in the Alpha-test.[6] Now then, can we say that lack of intelligence in negro criminals and intelligence in white criminals are "marital concomitants?"

Four percentile distributions are given in evidence of this conclusion. Presumably they convince the authors. They do not convince me. In fact my attention was drawn to them because I wondered why two pairs of such similar distributions were printed side by side if not for the sake of demonstrating a difference. Plainly this is a case where Kelley's advice must be taken seriously: we must not consult our "wishes" nor our "intuition," but resort to statistics for the best analytical description.

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[2] T. L. Kelley, Principles and technique of mental measurement, this JOURNAL, 34, 1923, 408-432, esp. 409-411.

[3] Kelley's criticism is also directed against a related article of mine, this JOURNAL, 31, 1920, 1-33.

[4] C. Murchison and R. Gilbert, Some marital concomitants of negro men criminals, *Ped. Sem.*, 32, 1925, 652-656.

[5] The authors do not give the numbers of the whites, but say that they were "approximately the same" as the negroes. My figures for the whites are the totals of the sets of integers that accord with the given percentages and also yield totals "approximately the same" as the totals for the negroes. I may be wrong, for I am puzzled by being thrice told that the ratio of married to unmarried negroes is 60 to 40, whereas the ratio of 139 to 99 is almost exactly 70 to 50.

[6] The meaning of the letter-grades in terms of Alpha score was changed during the test by the Army, but their definition here can be found in Murchison, *J. Crim. Law and Crimnol.*, 15, 1924, 264.

The thing to do is to compute the average scores and their probable errors and then to compute the constants that bear upon the "significance" of the difference. The results are as follows:

	Single negroes	*Married negroes*	*Single whites*	*Married whites*
Average score	34.22	28.39	69.38	80.78
P.E. of av.	5.06	1.14	2.28	3.59
Difference: D	5.83		11.40	
P. E. of diff.: $P.E._D$	5.19		4.25	
Ratio: $D/P.E._D$	1.12		2.68	
Probability that diff. is not due to chance	0.55		0.93	

In the two crucial cases it will be seen that the differences are 1.12 and 2.68 times their probable errors. In the other four paired cases, where a negro group is compared with a white, these ratios range from 6 to 16. Here then we have an abbreviated description of the facts with respect to the differences involved. The differences between whites and negroes are very much more significant than the differences between married and single persons of the same color. Does the conclusion follow? For the group, yes: there is a difference for both whites and negroes, although the difference is not relatively large.

Now, however, comes the problem of induction. Are single negroes more intelligent than married in general, and is the opposite relation true for whites? The larger the difference the more probable does the conclusion seem: the larger its probable error the less probable does it seem. The difference actually is about equal to its probable error. The difference for the whites is opposite and considerably less than three times its probable error. I feel sure Kelley would assert that no reliable conclusion can be reached from these data, because the differences are too small or the numbers of cases are too small. Here I agree with Kelley and I think practically any persons accustomed to deal with figures would also agree. One can not, however, set arbitrary rules for the ratio. Sometimes it is said that the difference must be three or four times its probable error to be "significant;" sometimes the necessary ratio is set at six, because the probability "that the difference is not due to chance" (on the assumption of normal distributions) is 1.0000 in four-place tables (*i.e.* "mathematical certainty").

There is no need to go farther then with this case. The authors concluded: "On the average, married negro criminals seem somewhat less intelligent than the unmarried, these tendencies being opposite to those of the white criminals in the same prison." The adequate statement would be that criminals were thus and thus in that particular prison at that time, and that they do not, when statistical insight is had into the case, even seem to be thus and thus in general. They may be, of course, for we do not have the data.

Let us take another case, the question whether length of incarceration affects intelligence as the Alpha test tests it.[7] Here we have data similar to the first case except that the numbers of individuals are given. The problem finally reduces to a comparison of negroes who had been in the Maryland State Penitentiary two years or less with the negroes who had been incarcerated for more than two years. Since the statistical constants are not computed, I give them here:

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[7] Murchison and P. Pooler, Length of incarceration and mental test scores of negro men criminals, *Ped. Sem.*, 32, 1925, 657 f.

Incarceration	*Two years or less*		*More than two years*
Average score	28.76		35.92
P.E. of av.	3.02		2.11
Difference: D		7.16	
P.E. of diff.: P.E._D		3.68	
Ratio: $D/\text{P.E.}_D$		1.94	
Probability that diff. is not due to chance		0.81	

Now the authors in question are not arguing that incarceration in general increases the intelligence. They keep quite properly to their particulars and say that the one group makes a better showing than the other, though I think they transcend their data in saying that the showing is "decidedly better." From this they argue that, at any rate, incarceration does not cause "deterioration in the ability to make scores in the Alpha test."

I do not know whether or not Kelley would call this difference "significant." The difference is almost twice its probable error. If one makes certain assumptions, there would seem to be nine chances out of ten that the difference would come in the same direction another time. I should think Kelley would reject such a difference as too inconclusive to be of scientific value, but that some psychologists might consider it seriously, even though they believed in a 10% chance of some deterioration with incarceration.

Here, however, I have my opportunity to explain what I meant when I said that the statistical constants give values of probabilities that are "too high" with respect to the general case. Are there really nine chances in ten that intelligence increases slightly with incarceration?

Suppose that, in Maryland just before the time of the tests, popular education about intelligence and the growth of the belief that lack of intelligence leads to crime, had so affected justice that there was a tendency to acquit the more intelligent criminals. Or suppose that the police had temporarily become less effective so that the more intelligent criminals tended to elude arrest. Suppose that any change at all occurred where intelligence could be used, even if ever so slightly, to avoid final incarceration. Suppose even (while we are supposing) that the intelligent criminals came to see that honesty is the best policy while the stupid criminals could not. In any such case the intelligence of convicted criminals would fall off in successive years, and we should find that the men who had been the longer incarcerated were the more intelligent.

I am not making these suppositions seriously, for I know nothing about these matters. I am trying to show the nature of the difficulties with which one is beset in inducing a generality. I am not in the least convinced by the data in question about the general fact. It still seems to me almost just as probable that performance in the Alpha test is diminished by incarceration as that it is not. There is where my real difference of opinion with Kelley arises. This case is much too particular. It applies to Maryland at a particular time with a few men. The induction seems valid only so long as one lacks the ingenuity to think of some possible uncontrolled factor that would make it invalid. Of course, if one is ingenious enough to think of some other explanation than the apparent one, it may be possible to investigate the validity of the other explanation. My point is merely that I do not think that the effectiveness of statistics for proving the generality of a difference should increase in proportion to one's ignorance of the special conditions that both determine and limit a particular sample!

Thus we come back to the scientist's dilemma that I pointed out in 1919 and that Kelley so severely criticized. Is the psychologist merely to take his statistical results at their face value or is he to evaluate them "intuitively?" I still believe that he is to evaluate them in formulating his final inductions. I think that these conclusions which I have been criticizing are not statistically valid, but that they are even less valid scientifically. By this statement it will be apparent that I mean that the evaluative departure from the statistical finding should always go in the direction of conservatism. I have never implied that we should be more positive than statistics warrant, but merely that we should often be less positive. The trouble is that, in making a general induction, our knowledge of all the possible causes of variation is never even nearly complete. The safe plan then is to combat ignorance of conditions with caution.

Harvard University EDWIN G. BORING

DR. MARSTON ON DECEPTION TYPES

In attempting to revive his theory of positive and negative types of liars, as indicated by reaction times, Dr. Marston has raised a number of interesting questions. It will be remembered that in the original experiment upon which this theory is based, Dr. Marston set before his subjects the simple task of performing a series of like arithmetical exercises either according or opposite to the instructions.[1] At this simple task some subjects could disobey more rapidly than they could obey, others took a longer time to disobey, and a considerable number varied in their reactions. These results were interpreted as meaning that people could be divided into good liars (who lie more readily than they tell the truth), bad liars (who react most rapidly when telling the truth), and a middle range of liars (whose performance vacillates). Miss Goldstein attempted to verify this conclusion.[2] A repetition of Dr. Marston's experiment yielded similar quantitative values together with introspective evidence that reaction times were lengthened when, and only when, the subject was conscious of deception. A variation of procedure, such that the subject performed a series of different rather than of like arithmetical exercises, resulted in almost universally longer reaction times when disobeying.

Dr. Marston has tried to show in a recent review of Miss Goldstein's work that her results uphold rather than cast doubt upon his theory of deception types.[3] In doing so he raises the central question: Just what is 'consciousness of deception'? This is surely an important phase of the discussion; but we might add to it, after reading Dr. Marston's argument, a further question: Is there any psychological entity which can be called the 'consciousness of deception'? If the term 'consciousness of deception' is distasteful, as it appears to be, we may substitute 'deceptive attitude' without affecting the argument. For it may still be asked, whether there is any true deceptive attitude, that is, any emotional, motor or intellectual attitude characteristic of the attempt to deceive. If there is none, we may as well stop talking about deception from the psychological point of view. It becomes merely an extra-psychological descriptive category.

Miss Goldstein, however, found certain emotional and motor characteristics which were typical of conscious deception and which were correlated with increased reaction times. In the simpler experiment these situations occurred only occasionally, while almost every reaction showed them under the more complicated conditions of her second experiment. The emotional set tells nothing of objective truth or falsity, but it is the only psychological characteristic of deception that has been found up to the present time. Truely enough, we may divide subjects into those who do and those who do not exhibit this emotional set when disobeying instructions as to the performance of a series of like additions or subtractions. It is a far cry, however, to say that these same subjects are respectively bad or good liars.

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[1] W. M. Marston, Reaction Time Symptoms of Deception, *J. Exper. Psychol.*, 3, 1920, 72-87.

[2] E. R. Goldstein, Reaction Times and The Consciousness of Deception, this JOURNAL, 34, 1923, 562-581.

[3] W. M. Marston, Negative Type Reaction-Time Symptoms of Deception, *Psychol. Rev.*, 32, 1925, 241-247.

These considerations lead to another vital point in the argument—the question of the adequacy of experimental conditions. Which of the two sets of experiments is more nearly typical of deception? Miss Goldstein holds that the use of a series of similar arithmetical operations so mechanizes the task of the subject that all consciousness of deception, emotional or otherwise, is lost. She therefore made the subject determine for every exercise what operation he would perform, in each case disobeying the instructions by not performing the indicated operation. Dr. Marston raises the objection that this complication merely added extra mental and physical work, thereby making longer reaction times inevitable. To this point we shall return. To answer the question of adequacy of experimental conditions, we must set up a standard by which adequacy may be estimated. Since the existing data upon reactions are, save for the two studies quoted here, based upon association reactions, we may well compare with them. In an association reaction, the subject responds to a series of different words by reactions which must, when the instructions to give the first associate are disobeyed, be separately chosen for every response. A series of similar reactions surely cannot approximate this condition, while a series of different reactions may well do so. The whole purpose of Dr. Marston's original experiment was to eliminate the qualitative factors resulting from the differences in words and to isolate the quantitative phase. Either method used by Miss Goldstein accomplishes this end. But the simpler method (Dr. Marston's) goes too far. It mechanizes the performance and then measures the reaction time. The results show not the ability of the subject to lie, but their ability to learn a new experimental procedure. Miss Goldstein's second method, on the other hand, prohibits mechanization while it still retains the feature of isolation of the quantitative measurements.

We may now return to the objection raised by Dr. Marston, that the complication introduced by Miss Goldstein merely added mental work to the task and that the increased reaction times in deception were due only to this additional work. That more work was required to disobey than to obey the instructions is obvious. Probably it accounts for the greater part of the increased reaction times, just as Dr. Marston claims. But is not the increased mental work a factor in deception? Does not the liar, or even the subject in an association reaction, have to do more work to give a false response than to give a correct one? The answer to these questions is too obvious to require further discussion.

One further point should be noted. In his original discussion, Dr. Marston held that the experiences of a witness in court should tend to keep the individual within his dominant type, negative or positive. In his later argument he takes the opposite view and thereby concedes the main point of Miss Goldstein's thesis. For he now holds that, when the negative type deceiver is cornered by the experimenter and perceives that his plan of deception is inadequate, he passes over into the typical emotional and motor setting that causes lengthened reaction times. In other words, the stress of responding, unless the deception is eminently successful, makes all persons positive in type, that is, bad liars. We then want to know whether an association reaction test is sufficient to drive the subject far enough into the corner to change his type. Also, what would be the effect of a cross-examination in court? These questions cannot be definitely answered without experimentation. The demonstrational experiment Dr. Marston quotes is only an isolated instance obtained under peculiar conditions and is therefore insufficient to prove anything. The weight of experimental evidence to date is that deception lengthens association reaction times.

We may summarize our argument in the following points: (1) Deception exists as a psychological category only in so far as it involves a typical emotional and motor attitude or 'consciousness' which accompanies increased reaction times. (2) Reactions which permit of mechanization are inadequate to the investigaton of deception, since typical deception involves a series of heterogeneous responses. (3) Additional mental work appears to be a component of the deceptive attitude. (4) The distinction between positive and negative types of reaction in deception is valueless, since any complication of conditions which leads to conflicting impulses makes all persons react more slowly when deceiving.

Chicago, Illinois GILBERT J. RICH

A CONTRAST TO KASPAR HAUSER

Kaspar Hauser, the abducted Prince of Baden, was, as accurately as can be judged from existing evidence, probably a royal personage. Whether he was born with good intelligence it is impossible to state, but he was confined in a dungeon until sixteen years of age, and on his release and appearance at the gates of Nuremberg it was found impossible to educate him or to make him useful to himself or to others. The case here presented is that of a modern girl, known to be defective from birth, and wholly neglected as to education after the age of seven or eight, who yet at nineteen years of age proved capable of considerable mental improvement. At seven years of age, having previously gone to school and been reported by her teacher as mentally deficient, she was sent to a state institution for the feebleminded, which was at the time under very bad management. She experienced for twelve years nothing but drudgery and severe punishments, no attempt being made to educate her. At nineteen, the circumstances of her family improved and she was taken home. An intelligence test given her shortly after showed a mental age of nine years, but as she was unfamiliar with coins and could neither read nor write, the tests had to be modified to suit her case. She was incapable of concentration, and so easily fatigued that she could not finish the test at one sitting.

A few weeks after being brought home she had learned to handle coins and could run errands for her mother, bringing back the correct change. Employment was procured for her at sorting salt bags, with a wage of nine, later thirteen dollars. This position she kept for nearly five years, until the firm went out of business; she then procured employment at putting pepper in boxes, for one of the chain grocery concerns, being paid thirteen dollars a week. Later she was advanced to the work of attaching labels, and finally to the packing of candy. She has therefore never been a burden to her people. She is neat and honest, greatly attached to her family, and has improved so markedly that though at first decidedly 'institutionalized,' she now in appearance passes for normal. She has gone to a night school for several winters, where she has learned a little reading and writing and simple figuring, although the school had of course no individual instruction. She is now twenty-five, and an intelligence test, given without modification in 1924, showed her to have a mental age of ten. She has learned to dance and to play simple games, such as Lotto; she can sew, crochet, and embroider, writes a legible hand and copies well from a copy book. It is not improbable that training by experts between the years of nineteen and twenty-one might have resulted in a gain of one or two more mental years. As it is, her gain at so late an age renders her case interesting.

New York Aquarium IDA M. MELLEN

THE ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The thirty-fourth annual meeting of the American Psychological Association was held at Cornell University December 28–30, 1925. The meeting was an unusually large one. About 225 registered, and 203 were at the banquet. It was gratifying to observe that even though the meeting was held in the east, a number of members came from the far west, and the other parts of the country were well represented. All the members were comfortably housed in one dormitory and ate together in the same building. This arrangement added greatly to the pleasure of the meeting.

It is not necessary to describe the papers which were presented, as the abstracts will appear shortly in the *Psychological Bulletin*. I shall merely mention a few of the features which seemed to stand out and which gave color to the meeting. The programme was arranged very similarly to the one of the previous year. Fortunately, long sessions with a large number of papers appear to be a thing of the past. The plan of having only about five papers at each session and of running sessions which overlap seems to have decided advantages. It enables the committee to group together a few papers of closely related subjects and gives the members the chance to hear only the papers in which they are most interested, with plenty of time between for corridor discussions. One is also more easily tempted to enter into a discussion of a problem when there is not a formidable number of papers still to come.

There were two sessions and twelve papers on general psychology. The subjects which created most interest were behaviorism, introspective methods, and the theory of *Gestalt*. The value of the discussions on this last subject was greatly increased by the presence of Dr. Köhler, who took the occasion to give a brief defense of his views. The papers in the second session consisted mainly of theoretical discussions of experimental data and methods. There was a session of seven papers devoted entirely to experimental psychology. Five of these papers were on vision or eye movements. Another session was devoted to experimental and comparative psychology. There were two papers on learning and four on experimentation on animals. It was surprising that there were so few experimental papers on the higher processes.

The session for informal reports of graduate students was well worth while. It is true that the list of fifteen papers was too long for anyone to sit through without considerable fatigue, but some of the papers were among the most interesting of the meeting. In one address the moving picture was used to present the method and some of the results of an experiment on conditioning fear in children, and proved to be a most effective means of exposition.

The session for applied psychology was concerned chiefly with vocational guidance and research in athletics. The program of the clinical psychologists contained five papers dealing with personality, mental instability, children's dreams, travelling clinics, and infant behavior. There were only five papers on the subject of abnormal psychology, two of which were upon emotions.

On the last afternoon there was a session for mental measurement consisting of five papers which dealt with various aspects of intelligence tests, and a session for educational psychology consisting of five papers which were concerned principally with class room problems.

The conference of experimental psychologists consisted of two sessions this year. About eighty attended the first session and fifty the second session. Everyone seemed to think them a very valuable addition to the programme. There were also three round table conferences on the "Measurement of Character and Personality Traits," "Psychological Consultations for College Students," and "Clinical Psychology" respectively. They were all well attended and enthusiastically endorsed by the members of the groups. There were indications that there will be a gradual decrease in the number of papers and an increase in such informal conferences.

At the banquet President Farrand of Cornell made a very graceful speech of welcome and Dr. Madison Bentley, President of the Association, read a paper entitled "The Major Categories of Psychology," which consisted of a scholarly exposition of the present tendencies in psychology, and was both critical and constructive. It was a happy combination of wisdom and humor, and gave much pleasure to the large audience.

Princeton University H. S. Langfeld

The Sixth International Congress of Philosophy

First notice of the provisional programme for the sixth international congress of philosophy, which will be held at Harvard University on September 13–17, 1926, has just been received. The Congress is divided into four divisions: (A) Metaphysics; (B) Logic, Epistemology and Philosophy of Science; (C) Ethics, Social Philosophy and Aesthetics; and (D) History of Philosophy. Every division is divided into four sections in which special phases of the divisional topics will be considered.

The attention of those who wish to offer papers is called especially to Section 4 in Divisions A, B and C, designated as 'Open Sections.' Topics proposed for discussion in these Sections are: (A) relation of biology and metaphysics, realism and idealism, value and existence, concept of personality; (B) synthetic judgments *a priori*, memory—its significance for epistemology, logic of probability and theory of induction; (C) philosophy of history, political philosophy, philosophy of education.

The titles and brief descriptions of voluntary papers should be submitted at an early date to the chairman of the programme committee, Professor R. B. Perry, 447 Widener Library, Cambridge, Mass. The time limit on the papers is twenty minutes. All the papers must be presented at the Congress by their authors in person.

Questions concerning the Congress on matters other than the programme should be addressed to the corresponding secretary, Professor J. J. Coss, Columbia University, New York City.

Genetic Psychology Monographs

The first number of the *Genetic Psychology Monographs*, dated January, 1926, has been received. It contains an article by Rachel Stutsman on "Performance Tests for Children of Pre-School Age." This new monograph series is published bi-monthly at Clark University and is supervised by the editorial board of the *Pedagogical Seminary*. Every issue will contain the complete report of a single investigation in the fields of child behavior and differential and genetic psychology.

K. M. D.

Industrial Psychology

January, 1926, also marks the appearance of the first number of *Industrial Psychology*, a monthly magazine. It is edited by Donald A. Laird, Colgate University, with the coöperation of an international board of editors, consisting of Percy S. Brown, Corona Typewriter Co.; William Forster, Academy of Labor, Prague, Czecho-Slovakia; Dougles Fryer, New York University; Keppele Hall, New York; Harry D. Kitson, Columbia University; J. M. Lahy, École Pratique des Hauts-Études, Paris; Fred Moss, George Washington University; C. S. Myers, National Institute of

Industrial Psychology, London; Lorine Pruette, New York; A. J. Snow, Yellow Cab Co.; and Erich Stern, Giessen, Germany. The board is strong and representative; the new venture is in good hands.

The first number contains a prefatory note, "The Forward Look," a cartoon; the following articles: G. L. Gardiner, Handling Men through their Self-Interest; C. S. Myers, The International Institute of Industrial Psychology (London); H. W. Hepner, Better Judgments of Men; D. Fryer, Industrial Dissatisfaction; A. J. Snow, Tests for Chauffeurs; S. C. Dodd, A Correlation Machine; A. Clark, Control of Office Output; and departments entitled "Uncle Job sez," Notes and Items, Recent Advances, and New Books to Read.

The object of the new magazine has to be inferred; for the prefatory note—to which one turns for such information—is made up entirely of a series of unrelated short stories, jokes, and items of news interest. It is obvious from the nature of the articles, however, that the object of the journal is to popularise the findings of psycho-technology—to make available for men of industry, in a readable and interesting way, the results of applied psychology. A very worthy object; for the reports in the scientific magazines are admittedly too technical for the business man, who turns—if he is interested in psychology at all—to popular magazines which are not only non-technical but are non-psychological as well. As a *liaison* between psychology and industry this magazine fills a long felt want.

K. M. D.

The Japanese Institute for Science of Labor

The Institute for Science of Labor at Kurasiki, Japan, has recently published a report covering the four-year period from July 1921 to January 1925. It is published in English, and it thus enables Occidental readers to learn about the history, organization, scope and work of the Institute.

The Institute was organized to study the 'biological, psychological, economic, sociological and moral problems' incident to the industrialization of Japan. The work of the Institute is divided among 6 departments: industrial physiology; industrial psychology; biometry; nutrition; social hygiene; and industrial diseases. An outline of the work in process and a summary of the completed work is given in the report.

The problems that have thus far been attacked are those of fatigue and health. Studies have been made of the effect of night work; the effect of temperature, humidity and ventilation; individual differences in efficiency; and the selection of employees. A bibliography of twenty-odd Japanese references, both theoretical and experimental, shows the output of the Institute during the past four years.

K. M. D.

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BINAURAL VS. MONAURAL SENSIBILITY OF THE HUMAN EAR TO SMALL DIFFERENCES IN FREQUENCY[1]

By Roscoe Conkling Young, University of Chicago

Introduction

The most interesting of the human sense organs are those used in the perception of light and sound, and many investigations have been made on these organs. In audition it is known that the perception of a sound is dependent on pitch, intensity, and the duration of the stimulus. Much of the investigation carried on in this field has been done on pathological ears from the viewpoint of diagnosis, while not so much has been done on the normal ear.

The most important types of investigation made on the normal ear have been on the lowest audible pitch, the highest audible pitch, the minimum intensity required to stimulate audition, the barely perceptible change in intensity as a function of intensity and pitch, and the barely perceptible change in pitch as a function of intensity and pitch. In this last field numerous investigations have been made on the single ear for pitches within the range most used in speech, and for persons of normal hearing. But apparently no exact measurements have been made on a large number of observers of normal hearing at the pitches most used in ordinary speech to determine to what extent the right and left ears of the same person differ in pitch sensibility, or to what extent the single ear differs from the two ears used simultaneously.

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[1] From the Ryerson Physical Laboratory, University of Chicago. The author wishes to thank Professor H. G. Gale, under whose direction the investigation was undertaken, for offering many valuable suggestions. He also is indebted to Dr. A. J. Dempster, Dr. F. M. Kannenstine and Dr. V. O. Knudsen.

Knudsen[2] while investigating the sensibility of the single ear to small differences of intensity and frequency obtained evidence which indicated that the right and left ears are nearly the same in pitch sensibility, while the two ears when used simultaneously are better than is either ear when used separately. Knudsen's work was carried out only for a single pitch of 1,000 d.v. (double vibrations, or cycles); and even for this pitch he did not feel that his investigation was sufficiently extensive in scope to warrant a positive conclusion.

In view of this, it was considered desirable to make a careful investigation on the ears of several persons of normal hearing, to see whether the right and left ears have the same resolving power, and to determine whether there really is a difference between one ear and two ears in resolving pitch. It also seemed desirable to carry the investigation of pitch resolution to pitches higher than those used in ordinary speech, which has usually been considered. No investigation has been made in this region, even on the single ear, so far as the writer is aware. (Knudsen[3] has investigated 15 single ears from 12 different persons up to 3,200 d.v.). Information derived from these proposed investigations ought to throw important light on the pitch-differentiating mechanism of the ear.

PROBLEMS

The specific problems investigated in this paper are:—

(1) The pitch sensibility, measured inversely by $\Delta N/N$, at varying levels of loudness, first with each ear separately, then with the two ears functioning together, at a constant pitch of 1,600 d.v.

(2) The pitch sensibility, measured inversely by $\Delta N/N$, at a constant level of loudness, first with each ear separately, then with the two ears used simultaneously, for pitches within the range from 64 d.v. to 4,096 d.v.

(3) The pitch sensibility, measured inversely by $\Delta N/N$, at a constant level of loudness, first with each ear separately, then with the two ears used simultaneously, for pitches between 4,000 d.v. and 8,000 d.v.

(4) Data and curves showing the results of the principal investigators with a statement and discussion of the factors affecting the result.

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[2] V. O. Knudsen, The Sensibility of the Ear to Small Differences of Intensity and Frequency. *Phys. Rev.*, 2nd Series, 19, 1922, 261 ff.

[3] *Loc. cit.*

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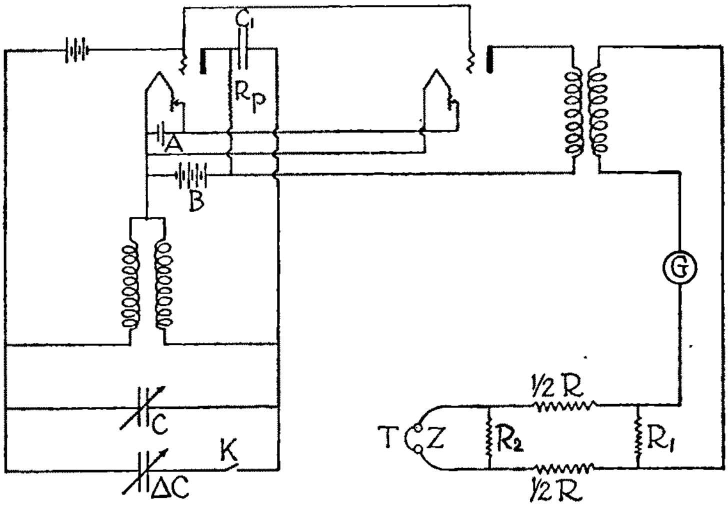
Apparatus and Procedure

Apparatus. Fig. 1 shows a diagram of the apparatus which was used for making the investigation. The tones used were produced by a telephone receiver actuated by electrical energy from a vacuum tube oscillator. This method of producing sound by means of transforming electrical energy from a vacuum tube oscillator as a source of alternating currents offers possibilities of pitch control, intensity control, and quality control which are not approached by any other method. The oscillator is capable of producing any pitch within the limits of audibility. Tests made with manometric flames and resonators show that the tones are approximately pure when the plate resistance is kept sufficiently high, provided the currents in the telephone receiver are not too large. Overtones, however, can be introduced by making the plate resistance of the oscillator sufficiently low. The data in this investigation were obtained with approximately pure tones, though the quality of the note does not appreciably affect sensibility, as has been shown by Knudsen.[4]



Circuit Diagram

Fig. 1.

The constant note within a given range was produced by varying the capacity C which was furnished by a three-dial condenser reading to 1 micro-farad capacity. The ranges of frequency were produced by using different inductance coils. The lower of the two pitches presented successively to the ear was produced by putting a capacity ΔC in parallel with C. This variable capacity ΔC was furnished by an air condenser and a three-dial mica condenser in parallel. By varying ΔC the note of lower frequency can be made to differ from the constant note by any amount desired. This additional capacity ΔC was thrown in and out periodically by a motor-controlled mercury switch, K, which was also arranged so that it could be controlled by hand. By varying ΔC the two notes can be brought gradually as close together as desired, or they can be separated gradually as far apart as desired. By varying the speed of the motor the two notes which are presented to the ear can be sounded in rapid succession, or in slow succession. Better repetitions were obtained when the change from one note to the other was about once a second. The duration of each note can be controlled by the amount of mercury in the cup of the mercury switch. These data were obtained when the notes were sounded for the same length of time.

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[4] *Loc. cit.*

The network of resistances R, R_1, and R_2 permits an accurate adjustment of the intensity to any desired level. R is distributed equally on each side of the line, and it is kept constant at about 5,000 ohms during a test. One side of the line is grounded.

The receivers used were a pair of double phones, varying from a pure resistance of about 3,000 ohms to an impedance of about 90,000 ohms at a frequency of 4,096 d.v. The receivers were screened against electrostatic effects and grounded. Preliminary tests indicated that it would be more desirable to connect the two phones in series and in phase, and all of the data recorded in this investigation were taken with the phones so connected. The intensity in the two phones was equal, as nearly as could be detected. It is essential that the intensity in the two phones be equal so that there is no tendency for the louder tone to mask the weaker one.

When one ear was being tested, the current was cut out of the phone which was placed over the other ear. The impedance used in calculating the intensities was determined for the double phones and for the single phones under the exact conditions of use. When making the tests, the phones were fitted comfortably to the ears of the observer, and care was used to keep the pressure as nearly as possible the same in every case.

The galvanometer, G, measured the total current, I, which flows into the network of resistances. The data for Fig. 2 were taken with a Duddell thermo-galvanometer; the data for Figs. 3, 4 and 5 were taken with a low resistance thermo-couple and a low range milli-ammeter.

Altogether four units of inductance were used in the tuning circuit of the oscillator. For frequencies below 200 d.v. an iron core inductance was used. For frequencies above 200 d.v. wooden core inductances were used. These inductances were closely coupled to give stability of oscillation and output. The output of the circuit shown in the diagram was fairly stable from day to day. The oscillator was calibrated by comparison with standard tuning forks at a constant temperature, by the method of beats. This calibration could have been made from the constants of the circuit, but the former method was considered more accurate as well as more convenient. For frequencies above 200 d.v. the calibration did not vary appreciably over the period of more than a year during which the data were being taken. For the frequencies below 200 d.v., where the iron core inductance was used, wider variations occurred, so that frequent calibrations had to be made for this range.

Procedure. The loudness of a sound, according to Miller[5], is a comparative statement of the strength of the sensation received through the ear, and hence it is impossible to state simply the factors which determine loudness. There is no unit of loudness for measuring sound, so we are dependent on the subjective comparison of our sensations. The ears of different *O*s are of different sensitiveness to sound, and the ears of the same *O* vary in sensitiveness at different pitches. In studying the physical characteristics of sounds, we are, therefore, forced to consider intensity, not as loudness perceived by the ear, but as determined by the energy of vibration, which varies as the square of the amplitude when the frequency remains constant.

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[5] D. C. Miller, *The Science of Musical Sounds*, 1916, 53 ff.

From theoretical considerations Rayleigh says that the acoustical energy given out by a telephone receiver is probably directly proportional to the mechanical energy of the vibrating diaphragm, and therefore to the electrical energy which actuates it. He suggests, however, that this relation needs experimental verification. An experimental test of this relation was made by Knudsen,[6] who found that a linear relation did exist between the acoustical energy put out by the receiver and the electrical energy which actuates it. Since the energy which actuates the receiver is proportional to the acoustical energy output, the intensity, or loudness, of sound produced by a telephone receiver varies as the square of the current in it, provided the impedance remains constant, which it may be expected to do at a given frequency. Hence the current which flows through the telephone receiver is an accurate measure of the loudness, or intensity, of the sound.

From the network of resistances shown in Fig. 1, the total impedance of the network is at once seen to be given by

$$R_T = R_1 [\, R + R_2Z/(R_2 + Z)\,]/[\, R + R_1 + R_2Z/(R_2 + Z)\,]. \qquad [1]$$

The current, I_R, passing through the receiver is given by

$$I_R = R_1R_2I/[R_2 + Z\,][\, R + R_1 + R_2Z/(R_2 + Z)\,], \qquad [2]$$

where I is the total current flowing through the network, and is measured by the galvanometer, G. The resistance, R, is divided between the two sides of the line, is made rather large, and is kept constant during a test. The current flowing through the receiver is adjusted by varying R_1 and R_2. The threshold current, I_o, is the current which actuates the receiver diaphragm when the note is barely perceptible. In determining this the operator varied R_1 and R_2 until *O* indicated that the note was barely audible. A short-circuiting key is placed across the receivers so that *O* can close it and cut the current out of the receivers, and then open it and let the current flow through again. In this way *O* can be more certain that he has reached his threshold of audition.

When the threshold has been reached, R_1 and R_2 are adjusted to give the required test current, and then K is operated by the motor to give the two pitches. A value is given to ΔC sufficiently large for *O* to hear the two notes distinctly; then the operator gradually decreases ΔC until the two notes merge into one note. This value of ΔC is noted and recorded. Next, ΔC is given a value sufficiently small for *O* to hear only one note, and then it is slowly increased until the observer indicates that he hears two notes. This value of ΔC is noted and recorded. The mean of three independent observations in each direction is taken for the final value of ΔC. If *O* has difficulty in repeating his judgment consistently, the operator sets ΔC at various places near the indicated value and asks the observer to give his judgment as to whether he hears one or two notes, in order that the rhythm of the change from one note to the other may not be so marked. As far as possible *O* is kept from knowing what the operator is doing, in order not to prejudice his judgments. Fatigue effects are minimized by having *O* rest frequently.

Seashore[7] says that one can not hope to get as reliable a test on a large number of individuals selected by chance as upon a single individual. He further says that those who have keen ears for pitch discrimination reveal the physiological limit at once. It is believed that the data here recorded represent the physiological limit because of the care used in selecting the *O*s for the tests, and because of the consistency in repetition which they showed. Many of the *O*s were graduate scientific students, trained in making careful observations. Either because they had poor ears or because they were unable to concentrate sufficiently well, it was found that some of those selected could not make consistent repetitions. No data were recorded for these.

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[6] *Loc. cit.*

[7] C. E. Seashore, The Measurements of Pitch Discrimination, *Psychol. Mon.* No 53, 1910.

The investigation was carried out in rooms comparatively free from disturbing noises. The data for Fig. 2 were taken in a room having double doors and no windows. The data for Fig. 3 were taken partly in an ordinary laboratory at times when the building was comparatively free from noises, and partly in a specially constructed sound-proof booth. The data for Fig. 4 were taken in the specially constructed sound-proof booth.

By the number of beats between a standard tuning fork and the oscillator, frequencies were determined for the oscillator when ΔC was placed in parallel with C. These frequencies were determined for various values of ΔC at each value of the constant frequency at which tests were made. From a knowledge of the constant test frequency, the frequencies of $C + \Delta C$, and ΔC, curves were prepared from which the pitch sensibility could be read for a given value of ΔC.

RESULTS

(1) *Pitch Sensibility as a Function of Intensity at 1,600 d.v.* Fig. 2 shows how the pitch sensibility of the two ears used together compares with that of the single ear for a constant pitch at different levels of loudness. Pitch sensibility, throughout this investigation, is measured inversely by the ratio of the barely perceptible change in pitch, ΔN, to the pitch, N. The curves represent the average values for 10 *O*s of normal hearing. Normal hearing signifies that *O* had never had any serious ear trouble and was not aware of any impairment of hearing. The curve marked "Right Ear" represents the average values for the 10 right ears; the curve marked "Left Ear" represents the average values for the 10 left ears; while the curve marked "Both Ears" represents the average values for the 10 *O*s when the two ears were used simultaneously. The ordinates represent $\Delta N/N$, measuring inversely pitch sensibility as defined above. The abscissae, I_R/I_0, represent the ratio of the test current in the telephone receiver to the threshold current. Values are shown for this ratio varying from 4 to 4,096. Intensities where this ratio was less than 4 were too weak for reliable repetitions, and values of this ratio above 4,096 were so loud as to produce rapid fatigue. At these high intensities the ear was so easily fatigued that it had to be rested often. The pitch used for these observations was 1,600 d.v. Tests made at other frequencies indicate that the curves there are of the same general shape.

The threshold current in the telephone receiver was determined in the manner indicated above, and the ratio of the test current in the telephone receiver to the threshold current was increased by multiples of 4 from 4 to 4,096, by means of calculations made from equation [2]. The threshold current was determined separately for each ear and for the two ears used together, and the higher intensity values were based on these values.

The curves show that pitch sensibility is essentially the same for the right ear as for the left ear, but that it is greater for the two ears when used together. This difference between the single ear and the two ears is greatest for small intensities and least for high intensities. For medium intensities the difference is practically constant.

From the curves it is seen that the average single ear at 1,600 d.v. can detect a change in pitch of 0.63%, or 10.08 d.v., when the test current is 4 times the threshold current, while the two ears functioning together can detect a change in pitch of 0.33%, or 5.28 d.v., at the same pitch and current ratio. Likewise the average single ear at 1,600 d.v. can detect a change in pitch of 0.25%, or 4 d.v., when the test current is 4,096 times the threshold current, while the two ears working together can detect a change in pitch of 0.2%, or 3.2 d.v., at the same pitch and current ratio. For intermediate current ratios the differences between the single ear and the two ears lie between these two extreme values.

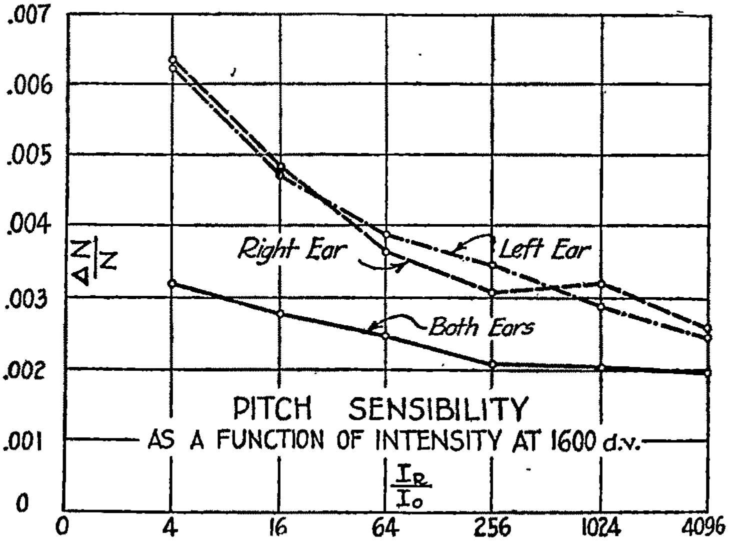

FIG. 2.

The results show that at a current 4 times the threshold, two ears resolve pitch 48% better than the single ear, at a current 16 times that of the threshold 41% better, at a current

64 times that of the threshold 34% better, at a current 256 times that of the threshold 36% better, at a current 1,024 times that of the threshold 33% better, and at a current 4,096 times that of the threshold 13% better.

It should be noted that the *O*s for the tests were not selected on the basis of musical training, though some of them had had musical training. They were required to detect merely a change in pitch and not the direction of the change. In this, those who had had no musical training did quite as well as those who had had musical training.

The curves of the individual *O*s are of the same general shape as the average curves which are shown, but some of the individuals showed a sensibility less than the average at certain intensities, and greater than the average at other intensities. The two ears, however, were always better for resolving pitch than one ear, provided there was no marked impairment of hearing in one ear, as sometimes occurs at some particular frequency.

(2) *Pitch Sensibility as a Function of Frequency at Frequencies Ranging from 64 d.v. to 4,096 d.v.* Fig. 3 shows how ΔN/N changes with frequency from 64 d.v. to 4,096 d.v. at a constant level of loudness. The curves represent the average values for 40 *O*s of normal hearing. The test amplitude of vibration used is kept constant for each pitch, being 100 times the threshold amplitude. The curve marked "Right Ear" represents the average values for the 40 right ears; the curve marked "Left Ear" represents the average values of the 40 left ears; while the curve marked "Both Ears" represents the average values for the 40 *O*s when the two ears are used simultaneously. The right ears, left ears, and the two ears are, of course, those of the same 40 people. The ordinates, ΔN/N, represent inversely, as before, the pitch sensibility. The abscissae represent the pitches at which the tests were made. The test amplitude chosen was sufficiently high to permit accurate decisions, but not high enough to produce serious fatigue. The range of pitch covered is that which is most important in ordinary speech.

The curves show that the two ears are approximately alike in pitch discrimination at a constant level of loudness when used separately. The fact that the pitch sensibility of the left ear is slightly better here than that of the right ear may be regarded as accidental, or it may be due to the fact that the left ear has had more training, since it is the ear used in telephone conversation. This apparently better sensibility of the left ear is noticed only in the lower pitches and lower intensities, which are the most important in telephone conversation. In Knudsen's investigation, which was referred to in the first part of the paper, the same apparently better sensibility of the left ear was observed. For higher intensities and frequencies the right ear seems to have the better sensibility quite as frequently as the left ear. The two ears when used together are appreciably better at all frequencies and intensities. The difference between the single ear and the two ears is practically constant from 64 d.v. to 4,096 d.v. This difference between the single ear and the two ears is shown for 40 *O*s of normal hearing in the table below, expressed in vibration differences barely audible.

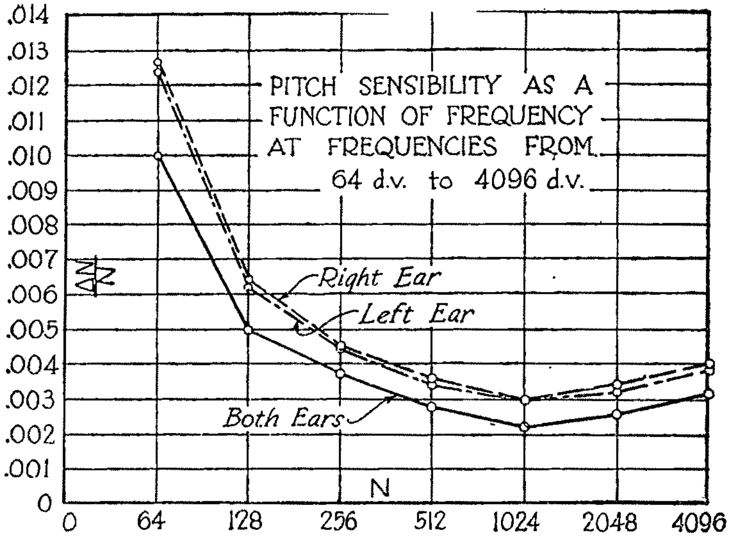

FIG. 3.

TABLE I

N	ΔN_S (Single Ear)	ΔN_B (Both Ears)	$\Delta N_S - \Delta N_B$ (Difference)
64	0.80	0.62	0.18
128	0.81	0.64	0.17
256	1.14	0.97	0.17
512	1.81	1.43	0.38
1024	3.01	2.23	0.78
2048	6.70	5.32	1.38
4096	15.97	12.88	3.09

The table shows the difference between the single ear and the two ears in terms of vibration differences. Column 1 shows the pitches at which the tests were made. Column 2 shows the vibration difference between the two notes when the single ear just resolves them. This means, for instance, that at a frequency of 64 d.v. the average single ear can distinguish two pitches when they differ by 0.80 d.v. At 4,096 d.v. the average single ear can distinguish two pitches when they differ by 15.97 d.v. For intermediate pitches the barely perceptible differences in frequency lie between these two extreme values. Column 3 shows the vibration differences between the two notes when the two ears can just resolve them. At a frequency of 64 d.v. the two ears of the average person can distinguish two pitches when they differ by 0.62 d.v. At 4,096 the two ears of the average person can distinguish two pitches when they differ by 12.88 d.v. For intermediate frequencies the vibration differences between two notes which can just be resolved by the two ears lie between these two extreme values. Column 4 shows the differences, expressed in vibrations, between the vibration differences resolved by the single ear and those resolved by the two ears. This difference is seen to grow larger as the frequency of the note increases.

The results show that at a frequency of 64 d.v., two ears resolve pitch 23% better than one ear, at a frequency of 128 d.v. 21% better, at a frequency of 256 d.v. 15% better, at a frequency of 512 d.v. 21% better, at a frequency of 1,024 d.v. 26% better, at a frequency of 2,048 d.v. 21% better, and at a frequency of 4,096 d.v. 19% better.

The curves for individuals were of the same general shape as the mean curves, though the curves of certain observers showed greater sensibility than the average at certain frequencies and less sensibility at other frequencies. It was always found, nevertheless, that the observer could distinguish appreciably smaller differences with the two ears than with the single ear, unless the hearing was markedly impaired for one of the ears at some particular frequency, which may happen in the case of persons of apparently normal hearing. This impairment shows up in the form of an abnormally high threshold for the impaired ear. For such persons at these particular frequencies the two ears are no better in resolving pitch than the better of the two ears. It should be noted, however, that an abnormally high threshold for a particular frequency does not mean that the sensibility is higher when the test amplitude is made 100 times that of the threshold. The absolute intensity in this case is much higher, and consequently the impaired ear distinguishes change in pitch as well as the unimpaired ear.

In a different type of investigation Wegel[8] has found that when one ear becomes less sensitive, even though the loss is small, the use of the binaural sense disappears, and after a time is not missed. He also found that for the binaural sense to be utilized the two ears must be very nearly alike. In certain cases of deafness which he has investigated, where one ear is much more impaired than the other, the hearer does not use the binaural sense. In such cases he has fitted to the individual a compensating deaf set so that he hears the sound equally well with both ears. With this compensation the individual manifests a marked pleasure, provided he experiences the binaural sense. Wegel states also that in some cases it takes practice to restore the binaural sense, just as it does to restore the sense of perspective when glasses are fitted to eyes, one of which is defective.

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[8] R. L. Wegel, Binaural Aids for the Deaf, *Proc. Nat. Acad. Sci.*, 8, 1922, 155 ff.

The same experience seems to apply in the resolution of pitch. When the threshold amplitude of one ear is several times that of the other for some frequency, the two ears resolve pitch no better than the better of the two ears when the test amplitude for the two ears is set at 100 times the threshold amplitude of the two ears, and when the test amplitude of the better ear is set at 100 times its threshold amplitude. Experiments show that this holds equally well for other test amplitudes. Evidently in this case only the better ear is functioning, even though both ears are in use. The intensity in the impaired ear is not sufficiently great to enable it to aid effectively in pitch discrimination. In the ears tested, this tone deafness in one ear occurred only in a few cases, and then only at some particular frequency. There seems to be no evidence to show to what extent this particular type of tone deafness prevails. Seashore[9] says that tone deafness probably prevails to the same extent as color blindness, though there are no satisfactory statistics on either.

It would prove interesting to fit a compensating set to persons having this tone deafness in one ear at a particular frequency, so that they could hear the pitch with equal intensity in both ears, and then test them for binaural pitch resolution.

The 40 persons tested above ranged in age from 18 to 40, the average age being 25. Of the 40, 9 were women and 31 men. The individual curves showed no detectible variation for sex. The average curve for the 8 oldest *O*s, whose ages averaged 36, showed that they did not resolve pitch quite as well as the younger *O*s. This was noticeable both when the single ear was used and when both ears were used together. The variation of the older *O*s from the younger was so slight, however, that it might well have been due to characteristics of the individuals. So the evidence on this point is not regarded as conclusive.

All of those whose curves were included in the average curves of Fig. 3 were able to repeat their decisions within limits of 10% variation. The test is quite a tax on one's powers of concentration, and it was found that some observers either did not have ears sufficiently good, or else could not concentrate sufficiently well to give reliable repetitions. For these persons no data were recorded.

Twelve of the 40 *O*s had had some musical training varying in amount from 3 months to 6 years. The *O*s were required to detect merely a change in pitch and not the direction of the change, and in this task those who had no musical training did as well as those who had musical training, though the latter could detect the direction of the change somewhat better than the former, as has been found by previous investigators.

(3) *Pitch Sensibility as a Function of Frequency at Frequencies Ranging from 4,000 d.v. to 8,000 d.v.* Fig. 4 shows how pitch sensibility at a constant level of loudness varies with pitch for frequencies ranging from 4,000 d.v. to 8,000 d.v. The amplitude used is 100 times the threshold, as before. The curves

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[9] *Loc. cit.*

are the average values for ten persons of normal hearing. Pitches are plotted as abscissae, and ΔN/N's as ordinates. The curve representing the average values for the right ears is marked "Right Ear;" that for the left ears, "Left Ear," and that for the two ears used together, "Both Ears." The individual curves are of the same general shape as the average curves.

The curves show that the pitch sensibility, varying inversely as ΔN/N, diminishes somewhat rapidly for pitches ranging from 4,000 d.v. to 8,000 d.v. This is true for each ear used separately, and for the two ears used together. The pitch sensibility, however, is still approximately the same for each ear when used separately, but it is appreciably higher for the two ears. For the single ear ΔN/N is 0.00365 at 4,000 d.v., and it increases to 0.01 at 8,000 d.v. For the intermediate pitches it lies between these two extreme values. For both ears it increases from 0.00275 at 4,000 d.v. to 0.00840 at 8,000 d.v., while for intermediate pitches it lies between these two extreme values.

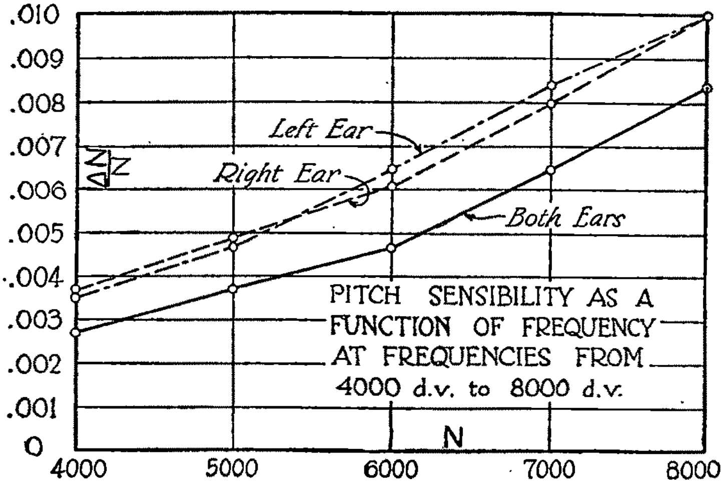

FIG. 4.

For these higher pitches, the inductances and capacitances used are comparatively small, so that the inductances and capacitances in the line become appreciable. In view of this the line was made as short as possible. The motor-controlled mercury key was cut out, and the key was operated by hand. Precaution was taken to reduce inductance and capacitance effects in the line. Any of these effects remaining were taken into account in the calibrations.

At these high frequencies it was thought that there might have been some appreciable change in loudness between the two notes as they were successively presented to the ear, so that what the observer heard was a combination change in frequency and loudness. So far as could be detected by the ear or by the galvanometer which measured the current flowing into the network, there was no change in intensity. Knudsen's[10] results show that there would have to be a change in current greater than 6% for the change in intensity to be detected at this frequency and level of loudness. Since a change in current of less than 1% could have been detected, it must be concluded that change in intensity is not a matter of importance here.

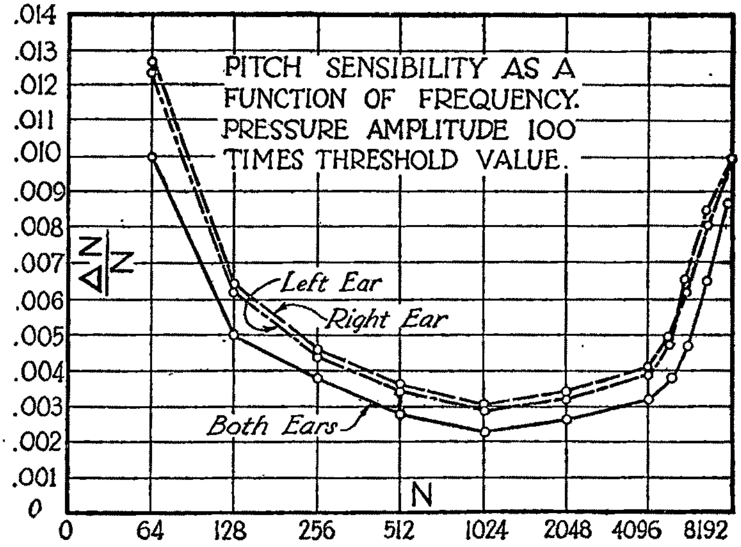

FIG. 5.

Of the 10 persons tested, there were 9 men and one woman. All but one were graduate students in science, trained in making careful observations. The age of the *O*s ranged from 23 to 40, the average age being 31. None of the *O*s had had musical training. The *O*s were required, as before, merely to detect change in pitch, and not to tell the direction of the change. All of the *O*s whose curves are included in the average curves were able to repeat their decisions consistently, though the variation in repeating decisions was somewhat greater for the higher pitches.

(4) *Pitch Sensibility as a Function of Frequency at a Pressure 100 Times the Threshold Value.* Fig. 5 is a combination of Fig. 3 and Fig. 4. This combination gives a picture of how pitch sensibility varies from 64 d.v. to 8,000 d.v.

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[10] *Loc. cit.*

RESULTS OF PRINCIPAL INVESTIGATORS

An examination of the literature on pitch sensibility shows that there is considerable disagreement in the results of the principal investigators. This is due partly to the fact that there was no accurate means of controlling intensity in most of the methods employed, so that what the observer heard was a combination of change in pitch and change in intensity. It is also due in part to the fact that some of the investigators used one ear while others used two ears. The regularity with which the two notes are presented to the ear and the relative time of duration of the two notes affect the results, though to a much less degree than the other factors.

Vance[11] has given a good historical survey of the problem, and has made a valuable contribution to the literature on this subject by his own investigations.

Preyer[12], using vibrating reeds, states that 1/3 d.v. in 500 d.v. and 1/2 d.v. in 1,000 d.v. can always be heard by the best observers. His *Os* could not resolve as two notes 500 d.v. and 500.2 d.v., or 1,000 d.v. and 1000.3 d.v.

Luft,[13] using tuning forks having sliding weights, gives:

N	64 d.v.	256 d.v.	2.048 d.v.
ΔN	0.15	0.23	0.36
ΔN/N	0.0023	0.0009	0.00017

Meyer,[14] using tuning forks with adjustable set screws attached, finds the following:

N	100 d.v.	400 d.v.	1,200 d.v.
ΔN	0.54	0.28	0.69
ΔN/N	0.0054	0.0007	0.00059

Stucker,[15] using tuning forks, a monochord, and a Galton whistle, finds the following as the average for 50 ears:

N	73.4 d.v.	130.5 d.v.	261 d.v.	435 d.v.
ΔN	0.7	1.0	1.3	1.4
ΔN/N	0.0094	0.0074	0.0049	0.0032
N	870 d.v.	1740 d.v.	3132 d.v.	6264 d.v.
ΔN	2.5	7.7	26.7	304
ΔN/N	0.0030	0.0044	0.0086	0.0491

Vance,[16] using tuning forks in front of resonators, obtained the following as the average of 50 ears:

N	64 d.v.	128 d.v.	256 d.v.
ΔN	3.4	1.55	1.50
ΔN/N	0.053	0.012	0.0058
N	512 d.v.	1,024 d.v.	2,048 d.v.
ΔN	1.85	3.35	5.94
ΔN/N	0.0036	0.0033	0.0029

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[11] T. F. Vance, Variation in Pitch Discrimination within the Tonal Range, *Psychol. Mon.*, 16, 1914, 115 ff.

[12] W. Preyer, *Die Unterschiedsempfindlichkeit für Tonhöhen*, 1876, 24.

[13] E. Luft, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, *Phil. Studien*, 4, 1888, 511 ff.

[14] M. Meyer, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, *Ztsch. f. Psychol. u. Physiol. d. Sinn.*, 16, 1898, 352 ff.

[15] N. Stucker, Ueber die Unterschiedsempfindlichkeit für Tonhöhen in verschiedenen Tonregionen, *Sitz. Kais. Ak. d. Wiss., Wien, Math. Kl.*, 116, 1907, 367 ff.

[16] *Loc. cit.*

Knudsen,[17] using a telephone receiver actuated by a current from a vacuum tube oscillator, obtained the following as the average for 15 single ears from 12 different people:

N	50 d.v.	100 d.v.	200 d.v.	400 d.v.
ΔN	0.51	0.65	0.90	1.36
ΔN/N	0.0102	0.0065	0.0045	0.0034
N	800 d.v.	1,600 d.v.	3,200 d.v.	
ΔN	2.40	4.80	9.60	
ΔN/N	0.0030	0.0030	0.0030	

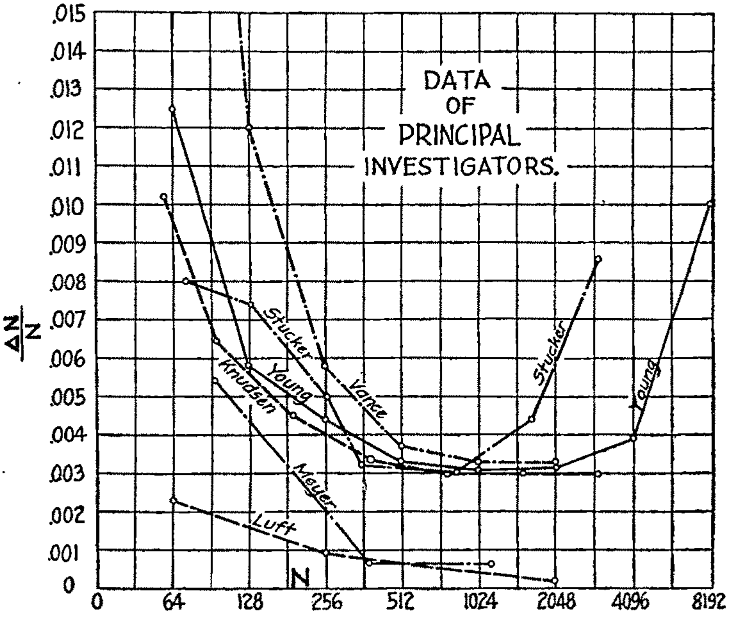

Fig. 6.

These results are more comprehensible when they are shown in graphical form. Fig. 6 shows the results of the 5 principal investigators, together with those of the writer. The pitch sensibility is plotted as ordinates and frequency as abscissae. The curves are marked with the name of the investigator who obtained them. The data for the curve of the writer are the average of 80 single ears from 40 different people between the frequencies of 64 d.v. and 4,096 d.v. Between 4,096 d.v. and 8,000 d.v. the data are for the average of 20 single ears from 10 different people. The curves representing the data given by Stucker, Vance, Knudsen, and the writer have the same general shape and agree remarkably well between the frequencies 512 d.v.

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[17] *Loc. cit.*

and 1024 d.v. There is seen to be considerable disagreement for the lower frequencies. For these lower frequencies, the curve representing the author's data agrees most nearly with Knudsen's curve, which is to be expected, since the data for Knudsen's curve and for the writer's curve were obtained by a method which offers better control of intensity, pitch, regularity of change from one note to the other, and duration of sounding the two notes, than was available at the time when the other investigations were made. These two curves agree almost exactly between 512 d.v. and 2,048 d.v. Their variation can be explained as due to characteristic differences in the individuals tested. The curve of the writer should be more representative, in that it is based on the average of a larger number of ears from a larger number of individuals.

SUMMARY

(1) At 1,600 d.v., the pitch sensibility at the same level of loudness is approximately the same for the right and the left ear, but it is appreciably greater for the two ears used together. This characteristic difference between the single ear and the two ears holds as the intensity of the note changes from a current 4 times that of the threshold to a current 4,096 times that of the threshold. The difference between one ear and two ears is somewhat greater for low intensities. For currents ranging from 64 times that of the threshold to 1,024 times that of the threshold, the difference between the single ear and both ears remains practically constant. For greater intensities the difference becomes somewhat less marked. Tests made at other frequencies show that this characteristic difference obtains there.

(2) The pitch sensibility at a constant level of loudness 100 times that of the threshold amplitude is approximately the same for the right and the left ear, but it is greater for the two ears functioning together. This characteristic difference between one ear and the two ears holds throughout the range most important in speech, from 64 d.v. to 4,096 d.v., and is practically constant throughout this range.

(3) At frequencies between 4,000 d.v. and 8,000 d.v., the pitch sensibility at a constant level of loudness, 100 times the threshold amplitude, is approximately the same for the right and the left ear, but it is greater for the two ears functioning together. The pitch sensibility in this range of pitch decreases rather rapidly both for the single ear and for the two ears, and the characteristic difference between the single ear and the two ears is somewhat greater.

(4) It should be especially noted that in the case of the single ear, and for the frequencies most used in speech, the investigation gives information on a larger number of normal ears than has been obtained before by a method in which pitch,

quality, intensity, regularity of change from one note to the other, and relative duration of the two notes could be carefully controlled. In addition, the investigation has been carried to pitches higher than those used before, and information is obtained which shows the difference between the sensibility of the single ear and that of the two ears, for pitches ranging from 64 d.v. to 8000 d.v.

There is no generally accepted theory as to how the ear differentiates pitches, but any comprehensive theory of the pitch-differentiating mechanism of the human ear must include an explanation of this characteristic difference between one and two ears.

EXPERIENCE AND VISUAL PERCEPTION[1]

By CHARLES A. DICKINSON

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While working on an experiment which employed in its technique, tapping while reading, and responding to a stimulus word while adding digits, certain mental experiences became evident, which were unable to be satisfactorily accounted for. The scrape of the kymograph and the moving of the point tracing on the smoked paper appeared, at times, to be present without meaning. Several attempts were made to place this kind of experience under rigid control with the result that the visual field was finally selected. The problem resolved itself into the present experiment on Experience and Perception.

Apparatus. The apparatus consisted of a tachistoscope of the pendulum disk type, a postcard projector and a ground glass. Two 150-watt, 110-volt, nitrogen lamps were used as illumination for the projection of the stimulus field. The ground glass was placed on a frame which was firmly clamped to a table; the frame of the tachistoscope was also firmly clamped to the table. The projectoscope was placed on a box on the table so that the stimulus field centered evenly on the ground glass, that is, so that no one-sided effect would act as a distraction to the *O*s. The disk of the tachistoscope was 13.8 cm. from the lens of the projectoscope, and the ground glass was 94 cm. from the disk. The length of exposure, measured by the tuning fork method, was 64σ.

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[1] From a dissertation presented to Clark University for the degree of Doctor of Philosophy. I wish, here, to acknowledge my obligation to the late Professor Edmund C. Sanford, of Clark University, under whose guidance the experimentation was carried on; to Professor John P. Nafe, of Clark University, for many valuable suggestions in arranging the data; and to Dr. L. B. Hoisington, of Cornell University, for many further suggestions in rearranging the material for publication. I wish also to acknowledge my obligation to Professor E. B. Titchener for the privilege of carrying on further experimentation in the Cornell laboratory, with trained observers, the results of which lay the basis for further conclusions as well as substantiating the data herein presented.

The *O*s sat at a table 1.2 m. from the ground glass. The illumination of the room was controlled by black draw-curtains, sufficient illumination being admitted to prevent the occurrence of a definite after-image on the ground glass, and to allow for the writing of the introspections. The whole right side of the apparatus, from the tachistoscope to a point beyond the position of the *O*s, was bordered with black cardboard in order to prevent the appearance of shadows on the ground glass from the illumination admitted through the windows. On the ground glass was pasted a fixation spot of black paper, at a height adequate to allow the *O*'s line of regard to be as nearly as possible in the horizontal plane.

Observers. The observers used in these experiments were Miss E. V. Howell (H), a graduate student in the department of psychology; F. A. Geldard (G), an advanced senior taking graduate subjects; R. S. Erickson (E) and K. E. Pohlman (P), seniors; F. J. Loungway (L) and H. P. Stedman (S), juniors and the author (D). D and H had had considerable observational experience; the remainder of the *O*s had previously observed in another problem for the writer.

Instructions.

Sec. 1, Series 1. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed, attend to the field in general, not to any particular section of it; then immediately following the exposure describe, orally, what you perceive. A 'ready' signal will be given, followed—with a 2 sec. interval between—by a 'now' signal, when the trigger of the tachistoscope will be released and the stimulus field exposed. Fixate visually the center dot and have no eye-movement."

Sec. 1, Series 2. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed you are to name the center letter and give the location of one other letter which will be called by *E* immediately following the exposure of the stimulus field; anything you may wish to add to this will be acceptable. A 'ready' signal will be given. . ." and so on, as in Series 1.

Sec. 1, Series 3. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed you are to name the center letter and give the location of another letter which will be called by *E* just before the 'now' signal; anything you may wish to add to this will be acceptable. A 'ready' signal will be given . . .," and so on as in Series 1.

Sec. 1, Series 4. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed you are to name the center letter and another letter, the location of which will be called by *E* immediately before the 'now' signal; anything you may wish to add will be acceptable" . . . and so on, as in Series 1.

Sec. 1, Series 5. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed you are to name the center letter and another letter which will be called, with its location, by *E* immediately before the 'now' signal; anything you may wish to add will be acceptable" . . . and so on, as in Series 1.

With this set of instructions *E* indicated on the ground glass, to each *O*, the following locations to be referred to as such at all times by both *E* and *O*; top row, left to right, 'upper left,' 'upper' and 'upper right;' center row, left to right, 'left,' 'center' and 'right;' lower row, left to right, 'lower left,' 'lower' and 'lower right.'

Sec. 2, Series 1. "A field of two playing cards will be exposed on the ground glass before you; immediately following you will reproduce, orally, what is called for. You are to give color, suite, number of spots and location of the card, whether right or left. The stimulus field will be exposed successively until you have reproduced all the items correctly and then one other exposure will be given for the purpose of verifying the field as the cards previously reproduced. Note: red diamonds and black diamonds, red hearts and black hearts, black spades and red spades and black clubs and red clubs will be included. A 'ready' signal will be given, followed, with an interval of 2 sec. between, by a 'now' signal, when the trigger of the tachistoscope will be released and the stimulus field exposed. Fixate visually the center dot and have no eye-movement."

Sec. 2, Series 2. "A field of three playing cards will be exposed on the ground glass before you; immediately following you will reproduce, orally, what is called for. You are to give color, suite, number of spots and the location of the card, whether left, center or right." And so on, as in Series 1.

Sec. 2, Series 3. "A field of four playing cards will be exposed on the ground glass before you; immediately following you will reproduce, orally, what is called for. You are to give color, suite, number of spots and location of the card, whether upper left or upper right, or lower left or lower right." And so on, as in Series 1.

Stimuli. In the first section of the experiment the stimuli were black letters printed on white paper, which was pasted on blue-ruled coördinate paper so as to form a field of nine letters in rows of three and evenly spaced. The blue lines, horizontal and perpendicular, gave the appearance of each letter being enclosed in a square. When this field was projected on the ground glass, these squares, in which the letters were centered, were 9.5 cm., the letters being 8.9 cm. high and the whole field occupying a space of 28.6 cm. square.

In the second section of the experiment a field consisting of playing cards was exposed: a 2-card series with the cards placed side by side; a 3-card series with the cards placed side by side; and a 4-card series with two cards above and two directly below. Black diamonds and hearts, red spades and clubs, as well as the usual combinations, were included. When the field was projected on the ground glass, the size of a single card was 8.9 x 12 cm.

Procedure. The first section of the experiment has five series, each carrying its individual set of instructions. Twenty-five exposures were given in each period, but in order to diminish the stereotyping of observational habits, the series were divided, five exposures from each being presented in an observational period. Approximately one hour was required for 25 exposures; the *O*s each carried 3 periods a week. They gave their reports orally, *E* writing them as they were given. When *E* was not sure just what the *O*s meant, they were asked to describe further or more in detail. D acted as *E* for all the other *O*s; the late Professor E. C. Sanford acted as *E* for D. With the exception of D, the *O*s were without knowledge of the problem.

Section 2 embodies three series. Here every series was carried through before another was begun. Each *O*, as in the first section, observed for approximately one hour 3 times a week.

In Section 1, 875 observations were taken and in section 2, 504 observations—a total of 1379 observations.

In discussing these data we should constantly bear in mind that our *O*s were not trained to describe technically, but we should also bear in mind the fact that one may be able to observe carefully and accurately even when unable to describe technically. We should remember, too, that our *O*s were instructed that a field of nine letters or a field of playing cards, embodying certain definite characters, would be presented. Instructions of this nature naturally set up an *Aufgabe* to report experience in terms of letters and playing cards, but not necessarily with the implication of logical meaning. In fact, when reporting in such terms, the *O*s sometimes explicitly intimated that logical meaning was not present; that, at the initial moment of the exposure, they did not really see letters as such but as visual forms.

EXPERIENCE AS VISUAL PATTERN

A significant fact, brought out by the introspective reports in relation to experience at this level, is that experience here is a mere 'thereness,' clear in contour and in detail of illumination but lacking in logical meaning. This 'thereness' is a totality of the whole field exposed; it is a flat clearness. We present data in support of this fact.

D. "Bare perception of the field with the letters perceived as just bare letters, then center as *H* with inner speech. There appear to be two different processes here. In the first bare perception the process is more passive, then, later, there is a definite attentional focusing on some one letter, with the falling away of all the rest. The letter is now clear in another way; seems to stand out from the background and to carry with it a good deal more implication; more definiteness, *e.g.* as a letter *H*."

D. "The same type of bare perception of the letters, then *C* as center and with inner speech; same fading of the field with the naming of *C*. *C* seems like a raised up letter; comes to prominence in this manner and then fades away. There is always location, definite, of all letters in the bare perception; the location is more definite and better retained than the letters. The background seems to be the most definite in the bare perception."

L. "I saw the whole nine and yet I did not see the actual letters immediately; then I tried to see certain letters, and when I looked at these, [attended to them] the rest disappeared."

L. "Saw nine letters, yet when I see the nine letters I don't know what they are, but when I looked at them I saw *N J B*, and then the field had disappeared. I repeated these to myself. If in the first flash I could quickly run over the whole nine to myself I could give them all."

L. "I don't think I see any more the last flash than the first, but I repeat them to myself before the last flash, and then they mean definite cards." (From playing card series.)

P. "As a matter of fact I see the whole field first; saw the composite group easily and the top and bottom disappeared. The letters I gave you were raised, forward, raised."

P. "I observe the whole field first and then memorize as fast as possible."

G. "In the first flash all the details are clear, but only in the last flash are they like a verification; just a thereness in the first flash; the last flash I'm satisfied I know them." (Report on verification in the playing series.)

D. "The process of immediate bare perception of the field appears to be a regular one, then some letters are reacted to and the others drop out."

S. "A general field of nine letters, but I only seem to recognize three or four out of the field. The ones I recognized are the ones I gave. I got the center letter in the process of looking for the letter called."

S. "I first see the field and then I go to the center and the center row appears to be much easier to get than the rest; and then I go wherever I can, usually to the lower row, so that the upper line is more or less shut out." (Report given after five exposures of the stimulus field.)

The foregoing reports point to the fact that experience at this level is a phase in development prior to the accrual of logical meaning. Here the whole field appears as a flat visual pattern with no further implication, but transformation from this level is exceedingly rapid, so that almost immediately we progress to another level.

EXPERIENCE AND GENERIC OBJECT

At this further development experience is represented at the level of generic object, and the field has here taken on a different appearance. In place of being entirely flat, parts of it have begun to stand out slightly in relief; they have begun to rise up out of the general field; they have become differentiated in the total field unity, previously undifferentiated except as visual pattern. These forms have taken on meaning, not definite and specific, but generic meaning; meaning merely as object. They have become object forms not yet specifically labeled. Again we present our evidence.

D. "Bare perception of the whole field, the edges not quite so clear as the rest of it. In this first perception all the letters seemed to come almost to the point where they could be reproduced, but then *L* was named and the rest fell away."

H. "I saw distinct squares with dark letters, which stood out clear, not exactly in relief, but the edges clean-cut, which indicates that they are certain letters."

G. "The field perceived distinctly as letters. I can not reproduce any more because I did not go through the process of repeating them to myself while trying to observe the general field."

L. "I saw more letters; the reason I could tell these to you was because I repeated them to myself first, in that order."

E. "The field as a whole was clear. The letters recalled popped out. Once I had six letters, but some disappeared with the rest of the field. I find that immediately after the exposure I repeat the letters I give you, to myself, as I locate them."

P. "I was aware of the general field, with letters as definite letters, but had not time to memorize them all. I got the general field, then the center, then the top and to either side, but not to the bottom."

D. "Bare perception of the field; more letters with letter value than reported. *X* named, then *Y* named, and then shift back again to *X* for retentional reinforcement, then further exploration of the field, but no more letters were retained."

P. "The two left receded back and to the left, and the others not secured simply receded back and out of sight. Those I reproduced I said to myself, either in series or as single letters."

S. "Saw lower right before *D*, but gave *D* first as I could not recall the name of lower right at the time." [S gave lower right as *L*, but it should have been *E*.]

S. "I get the center and then try to recognize the letter called, and if I get that also I get some in the immediate surrounding. It seems to be easier if the letter is at the left of the line. When I get a word like *M UD*, it is easier to get the letters in it and also others."

H. "I see the four cards on the field, and sometimes I recognize them as they go by, but don't see them long enough to fixate them." (From the playing card series.)

From the level of generic object there is again a quick passage to the level of specific object.

Experience and Specific Object

Here a definite label has accrued, the forms have taken on a stability which previously they lacked. They now have the characteristics of a specific letter, with the implication of the logical meaning of that letter. Visually they pop out, or rise up, or come forward markedly toward the observer. Simultaneously with this coming forward the rest of the field recedes away from the observer or fades out. The following reports are characteristic.

L. "The letters in the field that I gave were outstanding, I could tell what the other letters in the field were, but the moment I tried to recall them it was impossible."

D. "The field was perceived in bare perception; it seemed as if all the letters were letters, then there was a continuation of attention on *X*, which stood out very clearly as the center letter in the center square. There was a gradual but very quick sharpening of *X*, with a corresponding dropping away of the rest."

D. "Perception of the field as in the previous exposure, but letters not quite so plain as last time; the *M* as before, from that flatness to a very definite clearness as letter *M*; stood out from the rest of the field; center with background still present."

P. "I see all the letters and then find the letters called for if the field remains long enough. The field fades and the others come forward; the ones I don't get recede out and sideways."

H. "I got the center letter and then I looked at the general field and recognized the letter called for; that and the center letter stood out as a pair, and the rest faded out." *E.* "What do you mean by 'fading out'?" H. "As I get the letter called or the center letter, the rest fade out and the letter I get comes forward."

G. "*B* seems very, very solid, concrete, definite shades of light in back of it, a halo around the letter; it appears to be set off, set out the same as a sign-painter would set it out by putting shades of paint behind it." [*B* was the letter called.]

S. "The field in general was clear. I saw, I should say, about seven letters as distinct letters. The ones I gave seemed to swell right out toward my eyes and then recede after the exposure."

S. "As the letters were called they seemed to come forward into certain positions."

S. "All the letters were distinct as letters; those I remember seem to be pushed out toward me and then drawn right back again."

H. "I seemed to get *B* definitely, and all the rest were equal until you called *O*, and then *O* stood out and the rest of the field disappeared."

E. "Letters *W E G U* in that order, left to right; I saw other letters, but they were hazy and did not stand out." *E.* "What do you mean by 'standing out'? E. "They appeared brighter and seemed to come right out toward my eyes."

P. "I see all the letters and then find the letters called for; the field fades out and they come forward."

This process of accrual to experience, giving it a name, tabulating it as specific object, requires a shift from one part of the field to another; logical meaning accrues to one item at a time or to a group of items as a unit; it is a successive and not a simultaneous process.

Naming as Specific Object a Successive Process.

G. "Shift of attention from center to the others in the middle row and the rest of the field had not receded, then a shift to the top left and lower left, and then the others had disappeared."

P. "Saw the top fading; got center and then went to left and right; said them to myself."

P. "The process is from center to general field and then to the position called; center row and upper row and not to the lower row until after."

D. "Center first, then shift to *A*, then shift back to center to reinforce *B*, shift to right and *S*, and then to *O*.

Specific Naming and Changes in Contour and Saturation. In addition to the popping out or coming forward of letters as they are specifically named, there also appears to be a decided change in saturation and contour.

E. "As before, the letters I recalled are distinct and the surrounding letters are of a less black color; they seem to get cut right off."

E. "I can distinguish all the letters in the general field as letters; I can remember that in certain places were round letters and in others were straight line letters. The letters I recall are very clear, that is, the black is very black and the shapes are clear and clean-cut. There is a kind of red line very clear around them; they appear like what is called in shadows 'umbra' and 'penumbra'; the outstanding ones are like the dark shadows, the 'umbra.' "

The *O*s often mentioned that between the naming of two letters there was, as it were, a slipping back to the generic level, and at times to the level of visual pattern.

H. "Center to upper right, then across and *E M S* named, then to the general field, that is, I see the general field twice, after seeing the center and the location called, and then to the lower row and then to the general field again."

L. "This time I saw the field as I was trying to get from the location to the center letter."

Recognition as a Fitting of Sense Data and Existing Imagery. Under the instructions for verification, recognition appears to be the fitting of sense data and existing imagery. This was brought out in a number of ways.

L. "Did not see *W*; lower was *X*; did not see all the letters so well as in the first exposure. If not called upon to get certain letters I think I could probably get them all. It is a process of elimination, because I look at another just to see if it is something which is not called." [Reference to the previous exposure by L is reference to the exposures under the first set of instructions.]

L. "Looked everywhere until I got *R* last. I can't tell what letters I saw. *K* was somewhere; have now forgotten the center."

S. "Order of letters *O B S T D Y*; if I had not been told that *D* was there, I should never have recognized it." [In this case *O* thought that *E* had called the letter *D*, whereas *E* called *T*. *O*, however, reproduced *D*, locating it at the location of *T* and misplacing *T*. *D* was nowhere on the field exposed.]

Color has Advantage in the Naming Process. The reports on the playing card fields exposed, some of which have already been included, coincide with the reports from the letter field series; the playing card series is somewhat more complex, but in the main exhibits similar experimental characteristics. There are additional items, *e.g.* the playing card charts show that color appears to be almost immediate in perception, it is reported constantly when other factors are missed, that is, when neither suite nor number of spots can be stated at all, and it is, in most instances, reported correctly; occasionally its position on the field may be wrongly given yet the color correct. It appears at an advantage in the naming process. We find also that "imaginal overlay" enters in. Other items, too, appear, but we shall state them later in the general discussion of the material.

DISCUSSION

We have submitted introspective data which appear to demonstrate that, in visual perception, there are three main levels, not separate and distinct, but in the nature of a progression, one level developing to another. At these levels we have indicated the nature of 'experience' as visual pattern, as generic object and as specific object; we realize, however, that there is no such thing as level, separate and distinct, although there do appear to be momentary pauses or 'slackenings-up' of experience, in its temporal course, from the initial moment of the exposure of the stimulus field, through the process of the accrual of specific meaning to certain items in the field, to its termination. Many of our reports bear upon this point. Two will suffice to illustrate here.

D. "The field was perceived in bare perception; it seemed as if all the letters were letters, then there was a continuation of attention on *X*, which then stood out very clearly as the center letter in the center square. There was a gradual but very quick sharpening of *X*, with a corresponding dropping away of the rest." (From the letter series.)

L. "When the flash appeared I saw perfectly clearly four cards of different suites and different colors but however plain it may be it is a flat vision inasmuch as it appears to carry no particular meaning; then I habitually attend to the top and the bottom. I am still aware of color, but the moment I get the top as actual cards the bottom is gone, and then I am just seeing two cards at the top; but even then, when I attend to the top row, the process appears to be attending to one and then to the other. In the succesive exposures I really attend to one card at a time, which I definitely determine beforehand, and find it is much easier to take across instead of up and down. When I verify the last exposure I really repeat the cards to myself before the flash comes on, that is, the cards I have seen, and they have fairly definite visual image; if it is not the same, I am aware of the difference. If I find a difference, *e.g.* suppose I say to myself a card which is not really on the exposure, then I cannot verify it like the others, but must actually attend to it again and get it anew. I always begin with the card I am least sure of because I can easily get the other three." (From the playing card section, series 3.)

Experience at the initial level is merely a flatness, a 'thereness' of visual pattern which carries a wealth of detail of contour and illumination. No one part has begun to stand out from any other part; parts do not exist, the field is a totality and hangs momentarily as such. G states this quite definitely when he reports:

"In the first flash all the details are clear, but only in the last flash are they like a verification; just a thereness in the first flash; the last flash I'm satisfied I know them." (Playing card section.)

D also reports in this connection:

"The process appears to be an immediate bare perception of the field, then a slight reaction to certain letters with a view to establishing them or carrying them over until they can be definitely attended to, then an attentional exploration from center toward the left, up toward the top of the field and over toward the right if the letter called is not located, then back toward the center to reinforce it; some kind of kinaesthesis with certain letters in the course of this attentional exploration." (A general introspection after five exposures.)

The rise from this flat visual pattern to the level of generic object is very quick; the letters become letters and the playing cards become playing cards, but not yet individually named letters or cards. This change shows itself in a very slight coming to prominence of the letters or cards, with a corresponding change in definiteness of contour; the letters are now letters in a field, and the cards are cards in a field, with letter and card contour and not merely contour of visual pattern. Of this H reports:

"I saw distinct squares with dark letters which stood out clear, not exactly in relief, but the edges clean cut, which indicates that they are certain letters."

P states this from another angle:

"Since I saw *MUD* and *H I K* across the center row I always try to put a meaning to the rows of letters. I said the lower row as *EMPTY*, but did not actually observe them that way."

In the first case most of the field was, apparently, taken in at once; in the second case one row was taken as a unit and the letters grouped as a word. Here the letters were *N T Y*, but P reproduced them as *M T Y*. What P really did was to name the three letters as a word unit, as they were visualized, and later to name the letters as individual letters from vocimotor-auditory imagery of the word 'empty.' His imagery misled him with the result that an *M* was reproduced when it should have been an *N*. When H speaks of the edges of the letters being clean cut, thereby indicating that there are certain letters there, it is evident that differences in letter contour are noted, but the letters are not yet named as specific letters; they are letter, generic.

At the level of specific object certain characteristics appear to be much more concrete than at the previous level. The letters and the cards seem to actually come forward as they are specifically named. Different observers have reported this in different ways.

H. "I got the center letter and then I looked at the general field and recognized the letter called for; that and the center letter stood out as a pair and the rest faded out." *E.* "What do you mean by 'fading out'?" H. "As I get the letter called or the center letter, the rest fade out and the letter I get comes forward."

G. "*B* seems very, very solid, concrete, definite shades of light in back of it, a halo around the letter; it appears to be set off, set out the same as a sign-painter would set it out by putting shades of paint behind it."

S. "The field in general was very clear. I saw, I should say, about seven letters as distinct letters. The ones I gave seemed to swell right out toward my eyes and then recede after the exposure."

From these reports it is evident that, with the specific naming of letters, they appear to stand out or come forward, while the letters not named appear to sink back or recede. Sometimes these letters continue to recede backward until they have entirely disappeared, producing a two-level effect as they go, and sometimes they seem to hang in the balance before they disappear, giving to this two-level effect a certain appearance of stability. This appears to parallel the two levels postulated by Dallenbach.[2] We corroborate Dallenbach's facts almost in entirety. Our data also agree, in part, with those of Oberly.[3]

Again we find, as experience develops to this level, that there is a very noticeable change in color saturation and brightness and in the texture of experience. From E we have the following:

"I can distinguish all the letters in the general field as letters; I can remember that in certain places were round letters and in others were straight line letters. The letters I recall are very clear and clean cut. There is a kind of red line very clear around them; they appear like what is called in shadows 'umbra' and 'penumbra'; the outstanding ones are like the dark shadows, the 'umbra.' "

"As before, the letters I recalled are distinct and the surrounding letters in the field are of a less black color; they seem to get cut right off."

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[2] K. M. Dallenbach, Attributive *vs.* Cognitive Clearness, *Jour. Exp. Psychol.*, 3, 1920, 183-230.

[3] H. S. Oberly, The Range of Visual Attention, Cognition and Apprehension, this JOURNAL, 35, 1924, 332-352.

This change from a loose, soft, shadowy experience to one carrying more of a hardness of surface and a greater saturation appears to be directly correlated with the popping out, or the rising up, of the letters or playing cards as they are definitely tagged with specific, or logical, meaning. If, for example, the form *G* is being specifically labelled as *G*, with the full implication of the letter *G*, there is, visually, a coming forward of the form with a simultaneous hardening or solidifying of the texture, and an increase of brightness and saturation.

In this process of ascription of specific meaning the different items in the field are dealt with individually and successively. At times several items appear to begin to rise up simultaneously but these items are then named as a unit or one item becomes focal to the exclusion of the rest of the field. The reports are emphatic in this respect.

The rest of the letters in the field, that is, the letters not undergoing the process of naming, sink back, or fade out, or are cut right off, and the letter named remains momentarily after the rest have disappeared.

Under the second and third sets of instructions, especially, a very important and interesting fact becomes evident. The letter called sets up imagery and this imagery is in some manner compared or fitted to the letter in the field or the letter in the field is fitted to the imagery set up. Under the second set of instructions there is, as it were, a hunting in the memory after-image for the letter called; if the imagery set up by the calling of the letter fits, then there is recognition of that letter as the letter required, and the location is noted.

S. "I get the center and then get the letter called and any other letters I can, but when I get the letter called there is a tendency to stop, so the number of letters given probably depends on the getting of the letter called; but I want to get the letter called, and so slip over them quickly, and when I get it the others fade away."

L. "I repeated the cards to myself before the exposure and had visual imagery, but I was not sure of the fourth; when the field was exposed, the three I repeated to myself were recognized, but I really had to attend to the fourth once again as it went by." (Playing card series.)

L. "Recognized, a sweep of attention, but almost like the first flash, because I did not definitely repeat them to myself beforehand." (Playing card series.)

At another time L said: "I fit what is there to the visual image; if it is not the same, I am aware of the difference."

S. "I did not look at the letters to be able to recall them, except the center and the one called. I just looked at the letters to make sure that they were not the ones I wanted."

In this fitting of imagery, the field appears just to wrinkle at the place of fitting, the letters dwelt upon, in the attentional exploration of the field rise up ever so little; they do not reach even the level of generic object, but are merely matched against the existing imagery. The reports suggest that, as a rule, they are recognized with no more implication than being either the same as or different from the imagery set up by the calling of the letter. Sometimes, because of a certain peculiarity resulting from a combination of visual contour and illumination, a contrast effect appears, and, as it were, holds the observer momentarily at that position, and this momentary pause is sufficient to raise that item definitely out of the flatness of visual pattern to the level of generic object, and at times even to the level of specific object.

Imagery of one letter may persist and be reported in place of another letter in another location. Here is such a case.

D. "First of all *I* was perceived as not being *H*, with a clearness which, perhaps, was just a familiarity feel that it was not *H*, then *I* was named and it seemed actually to stand out as if raised up; shift back to *W* and very clear; shift to upper left and then to *J*. When *I* was not *H* there seemed to be a lack of balance which belongs to *H*, in contour and saturation; *I*'s contour and saturation are different. This was the first factor in recognizing that *I* was not *H*; this is of the same type as locating a letter: when you call *H* it makes a mental set for that letter which embodies imagery for *H*, which is expected to be shown on the ground glass."

E[4] had injected a series with D in which occasionally a wrong letter was called, *e.g.* in place of calling the letter *P*, which would show in the lower right corner of the field, *E* would call the letter as *R*, and D was then to give his introspection on the observation. D was therefore alert for false letters and here thought that one had been called. We may note that, in this instance, the upper left position and the letter *H* are called, that is, *H* is in the upper left. D's habitual shift is from center to left before going to upper left, therefore it is natural that the letter *I* should be seen before the letter *H*. This, apparently, is what happened: D visualized the letter *I*, and carried *I*'s imagery up to the position of *H*, which made it appear as though *E* had called a false letter. It is easily discerned from the report that D could not make image *I* fit the visual pattern of *H*, and immediately named *I*, and then shifted to the letter *W*. With the naming, the letter *I* stood out to prominence, and *H* was blotted out, but there is little doubt that if D had remained at the upper left position *I* would very soon have resolved itself into the letter *H*. D noted the difference in balance of the two letters, but evidently was unable to inhibit the naming of *I*, and when to the letter in the upper left position the name had accrued, the *Aufgabe* was fulfilled, and D passed on.

If we turn to the playing card section we find similar results. The field, first of all, is a totality of flat visual pattern, which then progresses through the level of generic object to that of specific object. H reports:

"Had a sweep of attention from left to right and just barely recognized the right hand card." Again: "A very short affair, but a complete flash of clearness, details all there, a sort of flatness to it and then the two upper cards became prominent."

L has also stated: "When the flash appeared I saw perfectly clearly four cards of different suites and different colors, but however plain it may be it is a flat vision, inasmuch as it appears to carry no particular meaning; then I habitually attend to the top and the bottom. I am still aware of color, but, the moment I get the top as actual cards, the bottom is gone, and then I am just seeing two cards at the top; but even then, when I attend to the top row, the process appears to be attending to one and then to the other."

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[4] *E* here used to designate the experimenter.

With the successive exposures of the playing card stimulus field several other factors manifest themselves. In the first place, color appears to be almost immediate in the perception; It appears to have an advantage in the naming process; coupled with location, it is stated with certainty many times where contour and number of spots are stated with uncertainty or are not stated at all. Again, we find the fact of "Imaginal Overlay."[5] For a number of exposures an *O* sometimes reports, *e.g.* a red spade as a black spade, and then, when he eventually reports it as red, says that he has seen it as red all the time.

Sometimes an item is read into the perception which does not exist on the exposure but which materially alters the meaning of the perception.

For five consecutive exposures G named a *7* of red hearts as *8* of red hearts, finally reporting that he really had seen a *7* each time, but that he had added a spot and reported an eight. He said: "I saw a spot in the middle and assumed another one at the top although I really didn't see it." This addition was due to the fact, he said, that the three spots were at the bottom of the card instead of at the top. G is a card player and when playing always holds his cards so that the seven, for instance, will be right side up, that is, the card held so that the extra spot is at the upper and not at the lower end of the card. When the extra spot is at the lower end the card is, for him, upside down; so, although visualizing the seven spots, and no more, he added one in naming the card and reproduced it as an eight.

Four of the *O*s reported what appears to be a very quick reflective process, while attempting to give specific meaning to an item in the field exposed. This was more noticeable in the playing card series than in the letter series and was on several occasions almost as immediate as the transition to generic level. It evidently had little effect on the speed of reproduction, although on one or two occasions the *O*s hesitated slightly, as a result, in beginning their report. It usually occurred in this manner: when the *O* had reproduced, *e.g.* a black heart consecutively for several exposures, or if the *O* was not quite sure whether, in the first exposure, the black heart was really a heart or a spade, instead of stating immediately what the exposure appeared to be, a reflective factor entered in and the *O* began to 'figure' what to name that particular item. In the case of this doubt about the heart in the first exposure, the process was more in the nature of a definite or direct judgment, but in the case where the suite is named a heart for a number of consecutive exposures, the same field still being presented, it differed somewhat from the nature of a direct judgment; in the final analysis a judgment was formed, and formed very quickly, but in the course of the formation of the judgment the *O*'s mental experience appeared to be, relatively speaking, considerably prolonged and of a reflective nature.

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[5] G. K. Adams, An Experimental Study of Memory Color and Related Phenomena, this JOURNAL, 34, 1923, 359-407.

When P hesitated just a trifle in making his report, *E* asked: "Why did you hesitate then?" P replied: "I have been stuck on this card four times now and this time I discovered I was figuring what it should be; very quick, but it was figuring just the same." D also reported: "I notice that there is a secondary process enters in sometimes when I am not quite sure of a card for several exposures; it is so fast that sometimes I can hardly catch it; it appears to take place while the field is still there; like an attempt to juggle the suite instead of stating what appears to be there, when I am not sure." *E*. "Are you quite sure that it is not embodied in your *Aufgabe*?' D. "Quite sure."

This may, perhaps, be considered in the nature of a judgment, as stated by Titchener[6]—an attentional alternation with a certain element of conflict entering in; and there appears to be little doubt that this kind of thing enters considerably into perception. Interest concerning it lies in the speed with which it takes place and its effect on the specific name which accrues to the item in question.

Two additional facts, which may be of importance, have been stated without adequate description; they receive here merely a casual mention. D reports: "In the first perception all the letters, except perhaps the lower part, were clearly present, those on the outer edges not so definite as those across; just letters recognized as different; just bare letters. This appeared to be almost like a double exposure; first bare perception of the field, and then *Z* and *P* definitely.

E. "In the exposure I recognize nine letters and know what letters they are, then for a fraction of a second there is nothing there, and then the letters which are reproduced are outstanding and clear and the others are not distinct."

E. "I find now that when I recall prominent letters there is a connection between each one as they come to me; seems as if I could draw a line from one to another; they pop out."

These reports seem to indicate that between the specific naming of different items in the field there is, as it were, a sag in experience back to the level of visual pattern, and perhaps still farther back than that.

The manner in which experience will organize is, in general, determined by the instructions. From the instructions in the first series of observations to the instructions in the last series, section 1, there is set for the *O* a trend in the requirement, that is, he is required in the first series to take the field as a whole for the purpose of description, but in the last series he is required to observe a definite letter in a definite location. Here the *O* is not called upon to describe, but merely to name and to reproduce one letter, and to verify or recognize another; whereas in the first set of instructions the *Aufgabe* is description, in the last series it is naming and verifying. The reports coincide closely with the requirements of the *Aufgabe*. In the last series the field as a whole comes into the reports in relatively fewer cases, but it does appear sufficiently often to enable us to say it is simply because it is ignored that it is not reported, and not because it is not seen as a field.

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[6] E. B. Titchener, *Text-Book of Psychology*, 1910, 542.

SUMMARY

We may sum this paper up by stating that, from the reports of our observers, there appear to be three levels in the progress of experience, in visual perception, from its initial moment to its termination. These levels we designate as visual pattern, generic object and specific object; we do not, however, consider them as separate and distinct but as grading the one into the other. The climax of visual perception appears to be the coming forward, or rising up, of certain items in the field exposed, a process which corresponds to the specific naming of the particular items. These items are named consecutively. While the process of specific naming is in progress, the items in the field not being named recede or fade out. When an item has been named it remains momentarily after the rest have disappeared and then recedes or fades out itself. The total perception consists in a gradual but quick rise to prominence of certain items and then a gradual but quick decay.

CONCLUSIONS

(1) Our data appear to demonstrate that, in visual perception, there are three main levels, not separate and distinct, but in the nature of a progression, one level developing to another. We designate these levels: visual pattern, generic object and specific object.

(2) Sometimes experience at the level of generic object embodies a number of items as a totality, and this totality, as such, rises to the level of specific object while the units remain at the level of generic object. Thus letters were named as a word unit.

(3) The rise of any item to the level of specific object exhibits itself visually as a coming forward or a rising up from the field in general. With this rise to prominence of one item there is a receding or fading out of other items.

(4) Recognition appears to be the fitting of sense data to existing imagery.

(5) Sense data may be obliterated by imagery which rises to the level of specific object.

(6) Color appears to have an advantage over contour and form in the naming process.

(7) Imaginal overlay may be present in perception.

(8) Secondary reflective factors, almost as immediate as experience itself, may enter into perception.

(9) Visual perception is first a gradual but rapid rise of experience to a climax, which embodies logical meaning, and then a gradual but rapid decay.

AN EXPERIMENTAL STUDY OF MENTAL AND PHYSICAL FUNCTIONS IN THE NORMAL AND HYPNOTIC STATES: ADDITIONAL RESULTS

By Paul Campbell Young, Louisiana State University

A former article[1] dealt with the differential diagnosis of hypnotic from waking consciousness, and brought out that the likenesses in the two states were even more noticeable than the differences in the two states. Specifically, it was held that the normal consciousness and the hypnotic consciousness are alike in showing great variability of performance from person to person and from time to time in the same person; in showing equal abilities in the two states in the fields of sensation, perception, present memory—learning and retention—and physical work not involving fatigue. In hypnosis, however, there is increased ability to endure pain and to recall long-past memories, and decreased ability in some stages, at least, to make continuous responses.

This article will report further experimental results: first, in regard to the capability-differentia of hypnosis from waking; and second, in regard to the nature of hypnosis *qua* hypnosis. For studying the capability-differentia of hypnosis (*i.e.* what one *can do* in hypnosis that he *cannot do* in waking) a strict comparative method is a *sine qua non*.[2] For studying the nature of hypnosis *qua* hypnosis (*i.e.* what the hypnotic *S will do* with conviction that he *will not do* in waking for lack of conviction) the comparative method, although helpful, is not essential.

I. THE PROBLEM OF PERCEPTUAL ACUITY: DISCRIMINATION OF POSTAGE STAMPS

Can the normal perceptual powers be greatly or even appreciably augmented in hypnosis? The great majority of writers on hypnotism has held that they can be, *e.g.* Moll[3] and Bramwell[4] report marvelous perceptual acuity in hypnosis. Perhaps the classical case of alleged heightened powers in hypnosis is that reported by Henri Bergson,[5] in which a boy read out of the cornea of a man in front of him the three-millimeter letters of a book which the man was holding behind the boy's head, the whole image on the cornea being not more than one mm. in height. Other similar results of increased acuity have been almost universally claimed as an outcome of experiments in which the hypnotized *S*, after having been given the suggestion of an hallucination of a picture on one of several identical-looking cards, afterwards picks out the card from the pack.

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[1] This Journal, 36, 1925, 214-232.

[2] The comparative method demands: (1) a large number of *S*s; (2) a large number of separate tasks; (3) a large number of experimental sessions; (4) many hours of experimentation; (5) tests in both the normal waking and hypnotic states; (6) identical instructions in the tests under the two states; (7) identical conditions in the two states; and (8) control *S*s.

[3] A. Moll, *Hypnotism*, trans. 4th. ed., 1909, 100 f.

[4] J. M. Bramwell, Hypnotic and post-hypnotic appreciation of time, *Brain*, 23, 1900, 161-238.

Three considerations militate against accepting as final this type of experimental result, which is so common in the literature: (a) It is often without scientific control, *i.e.* in measuring the stimulus, standardizing the instructions, or keeping careful time. (b) Even if the experiment is scientifically controlled in the hypnotic state, there has been no bona-fide trial in the waking state of the same *S*s under identical conditions, with the same expectations of accomplishment and demands for putting out genuine effort. (c) The performance of normal *S*s in waking consciousness has been grossly underestimated. Feats of perceptual acuity as marvelous as any in the literature of hypnosis have been performed by *S*s in the normal state.[6]

In order to use the comparative method on this problem of increasing perceptual powers in hypnosis, the following experiment in picking out ordinary postage stamps was devised.

Condition A. At first, the stamps were used just as they appear when pulled from a sheet of a given denomination (say, of two-cent stamps; that is, in an afternoon's experimenting all the stamps were of one kind). After eliminating blurred or lightly-printed stamps, *E* chose one, which he lightly marked with pencil on the back in order to recognize it later himself. The *S* then studied this stamp by itself for 10 seconds. When the time was up, the stamp was taken behind a screen and shuffled with 19 others. Then, when the 20 stamps were uncovered, the *S* would search for the one he had studied. With a few trials, almost any *S* could study a given stamp for only 10 seconds, and, thereupon, either in or out of hypnosis, pick it out from the others which to the uninitiated seem identical. When the *E* saw that with a 10-second study the feat could be accomplished so readily, the time was reduced to a 5-second perusal of the stamp. Five seconds was kept as the time for the later refinements of the technique. Even then, with any sort of observation worthy of the name, the clues from the remains of perforations on the edges were an absolutely infallible guide. The test was still too easy for ascertaining any alleged difference of ability in the two states; but it is suggested that this feat of itself would pass, if done only in hypnosis, for a good 'stunt' to prove increased powers in that state.

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[5] H. Bergson, De la simulation inconsciente dans l'état d'hypnotisme, *Rev. phil.*, 22, 1886, 525-531.

[6] *Cf.* Helen Keller's muscle reading and her sensitivity to other vibrations, in her *The World I live In*, 1914; R. H. Gault's case of synesthesi in a deaf and blind girl, who could distinguish colors by the sense of smell, *J. Abn. Psychol. & Soc. Psychol.*, 17, 1923, 395-401; K. Dunlap's finding that for normal persons the palmesthesic threshold for rate in the neighborhood of 440 d. v. is not more than 36 d. v., and may be as low as 16 d. v., Palmesthesic sensibility, *Amer. J. Physiol.*, 29, 1911, 109 f.

Condition B. To make the task more difficult, the edges of the stamps were cut off, leaving only the portrait surrounded by its own frame; but, even so, the edges could not be cut absolutely square all around: the test was still too easy, and equally easy in the two states.

Condition C. Finally, after other attempts to make the problem difficult enough to ascertain differences of performance in the two states of consciousness, if they exist, a piece of card-board 10x5 in. was taken; 10 round holes ½ inch in diameter and one inch apart were punched in it, and behind these holes the stamps were placed to be inspected, so that they appeared to be, as in fact they were, behind a frame. Only the actual face on the stamps with the meagerest bit of background could be seen. In giving the test, only 5 seconds were allowed for the study of one stamp as it lay on the table looking out through one of the 10 holes. All the other holes were covered. Immediately after the study a shield 12x18 inches was thrust between the *S* and the stamp materials. Then the *E* took the stamp, put it under another hole, and placed nine other exactly similar stamps (*i.e.* any with apparent flaws had been discarded, and, of course, they were all of the same denomination) under the other 9 holes. The screen was then removed and the subject allowed to pick out the stamp he had just studied. The time taken to make a choice was noted. Three trials were made at each session in both states, each time with a different stamp to be studied. Great care was taken to keep the stamps clean; if one became soiled, it was at once discarded. The whole set of 10 stamps was frequently changed. Exactly the same procedure was followed in both the hypnotic and the waking sessions, except as noted in the third paragraph below.

Results. Comparable data under *Condition C* were obtained from 6 *S*s, equally divided among the light, deep, and somnambulistic classes.[7] The results obtained in hypnosis were so nearly identical with those of the same *S* in the waking state that even chance differences seem to have been equalized. When the *E* saw that *one S* after the usual 5-seconds study of the bare features of the face of Washington, could pick out the correct stamp 3 times in 3 trials in both waking and hypnosis, he dropped the test, considering it only too evident that in the past the hypnotic performances were marvelous mainly because the normal waking ability, not having been adequately tested, was considered inconsequential, or else because of the peculiar constitution of the hypnotic subject.[8]

As far as scores go, the first *S* made failures in both states; the second made 3 successes and no failures in both states; the third was successful in 7 out of 8 trials in waking (on account of his high score and the length of time it took to give these tests no comparable data were obtained in the hypnotic state); the fourth was successful in 1 out of 3 trials in each state; the fifth failed in all 3 trials in each state; the sixth *S* failed in 2 out of 3 trials in each state.

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[7] *Cf.* my earlier article for the meaning of the terms.

[8] In the earlier article the fact was emphasized that, in this research, only normal *S*s were used.

The old conception that somehow the formation of an hallucinatory picture on the article to be picked out was responsible for the great success achieved by the hypnotic *S* was shown to be groundless by the results in this case. Given the suggestion of hallucination of a different picture on the stamp he was to study the *S* made on the average a poorer score. For example, if the task demanded that the *S* study a Washington-stamp 5 seconds and pick out that stamp later from ten other Washington-stamps, the *S*'s performance was appreciably lowered in case the *E* led the *S* to believe through hypnotic suggestion that the *S* was really studying a Franklin-stamp which he was later to pick out from among nine Washington-stamps. This fact was ascertained during the testing under *Condition B*, and also in several tests of picking out identical postal cards (as if there were any such things, even though they be U. S. Postal cards!) with and without the hallucination of a picture on the one studied in hypnosis.

Conclusions. This type of experiment is quite different from that which Bergson reports, and in which, no doubt, the boy's eyes were naturally provided with microscopic lenses, but which, probably, he used in waking as well as under the spell of the hypnotist, or possibly used only in hypnosis, because he was led to believe that they could be used microscopically only in that state. It is not denied that some very well-authenticated results show a decided advantage of the hypnotic performance over the waking performance. A case in point is Bramwell's report on the marvelous ability of some serving girls in judging the passing of time.[9] It is contended in this article, however, that in none of these tests showing great discrepancy between the performance in the two states was there as much training given in the waking state or as much consideration given to eliciting a maximum response in that state, as in hypnosis. Since we do not know what the *S*s could have done if the conditions in the waking performance (when there was one at all) had been as favorable as in the hypnotic, it is a gratuitous assumption that the hypnotic state *per se* was responsible for the unusual performance; an assumption all the more unwarranted in the light of the extraordinary performance of normal waking subjects.

Nor is it denied that there may be a type of abnormal person who can do much better in hypnosis than in waking. In individual cases, in which the subject either has not developed his powers to the full or has trouble in giving intelligent direction to his mind, there might be a fairly consistent increase of performance in hypnosis. Final proof, however, that even these cases may show a greatly increased ability in hypnosis awaits the application of a strictly comparable and detailed investigation of their performances in the two states. It can be imagined that certain lack-willed or 'flighty' people may 'find' themselves only in a state of hypnotic trance, in which another directs their consciousness, or in a state of self-induced trance, in which some dominant idea of their own unifies their consciousness.

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[9] Bramwell, *op. cit.*

II. MEMORY

In addition to the rather extensive tests of memory already reported,[10] there was a series in which two *S*s were tested on memory for articles which they had seen over and over again in the outer of the two rooms used for experimentation but to which their attention had never been called. The tests in both states were given in one session; and were gone through on two different occasions in the inner room.[11] The results were entirely negative. Efforts to 'take' the hypnotized *S* to a certain corner where he said there was something, he knew not what, brought on great trouble in the subject, shown by an expression of discomfort of the mouth and writhing of the body. When pressed to tell what he 'saw,' the subject would say "I don't know," or else, "It's dark, I can't see." Not very often could the *S* be forced to make a wild guess. Whether the fact that he knew that his statement could be checked up, made any difference is questionable, because the same refusal to give details in regard to certain early memories (when the *S* was taken back, say, to his fourth year) was manifest also.

The results of this test, which was given to two somnambulistic *S*s, bear out the results already reported,[12] *i.e.* that there is little if any difference in the performance in the two states in this respect.

Early memory. The tests of reviving early childhood memories were carried on in the following fashion: after record had been made of the results of at least three attempts, in three separate sessions in waking, to find out certain facts of early childhood (*e.g.* with *S* 20, whether as a child he had gone to kindergarten or not, and if so what kindergarten), the *S* was 'taken back' in hypnosis to the time and place in question by being told that he was now a child again. In brief the results were clearly positive in the case of one *S* (two *S*s participating in the experiment), who recovered facts which he could not touch in waking, and of which he could not recognize the truth when told afterward that he had given them in hypnosis, but which were verified in detail by an older sister, who had been with him almost constantly while he was a child. Among such facts were the following:

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[10] Former article, this JOURNAL, 36, 1925, 223 ff.

[11] For general conditions of these tests the former article or the thesis in the Widener (Harvard) Library must be consulted.

[12] Former article, this JOURNAL, 36, 1925, 223 ff.

That he did go to kindergarten in Somerville, that he lived on Dana Street, that Mrs. Chickering lived in the lower apartment; that Mrs. Cole lived next door; that his room looked out upon the back yard and had three windows. This subject in normal consciousness was as much interested in the revived memories as *E* was, because they were as new to him as they were to *E*.

Although, when taken back to early childhood, the other *S* remembered some things that he could not recall in waking, the memories regained were nothing like so much beyond the waking performance as were those of the preceding *S*. However, in the manner of saying the alphabet, of counting, and of saying the name "Connecticut," he fell into childhood habits which he could not reproduce in waking, however hard he tried to recall them. These mannerisms were verified by his mother, who, without being told what the *S* had done in this respect in hypnosis, wrote detailed descriptions of his early habits, writing directly to *E*.

Strata of the personality. The further question of strata of the personality (*i.e.* whether a subject who is 'taken back' to four years of age, is thereupon, in some real fashion, four years old again, and circumscribed by the knowledge that he had at that age) was broached, but by no means settled. Here, again, the *S*s were in the somnambulistic state.

One method was that of asking the subject supposed to be four years old, about such things as he could have known at that age, and then every once in a while thrusting in a question to which he could not have known the answer. This method gave surprising, not to say astounding, consistency in the type of response. *S* 19, for example, in carrying out the hypnotic suggestion that he was four, could count to 13, but could go no farther. Moreover, he counted in a characteristic fashion each time the test was given him: with a certain expression of his face, with an emphasis on certain numbers, and with a way of going back, sometimes several numbers, sometimes all the way to 1. Every time he got to 13 he skipped to 16, 19, 40. Both the subjects in hypnosis would simply say "I don't know" to questions for which they could not have known the answer; *e.g.* when he had been told he was four years of age, *S* 19 knew that his father worked in Bristol but could not by any means be made to say what he did there.

As an objective check on the validity of the four-year consciousness induced by suggestion, the experimenter gave *S* 19 a Stanford-Binet intelligence test. The behavior during the various parts of the test was all that could have been desired of a four-year old child; cocksureness, hesitation, curiosity, even humor of a quiet sort (in regard to the mutilated pictures and in telling the differences between an egg and a stone) were shown. The results, however, showed an I. Q. of 183 if his age be taken at 48 months (*i.e.* just 4 years of age). If, however, as is more reasonable, he be accounted at least 57 months old (4 years and nine months) the I. Q. is 154. The age of 57 months is used for the following reason: earlier in the evening, after he had been told he was four years old, the *S* said he had just been scolded for being out in the snow. Since his birthday is late in March, and since in hypnosis he never recalled any birthdays, it is likely that this evening he was, in hypnosis, back in the winter following his fourth birthday. This *S* is a very good student; but whether an I. Q. of 154 represents what he could have done at four years of age can not be determined now. This *S* was not put through a similar test in waking, with the instructions to act as a four-year old child would, to see if he could by conscious simulation approximate his score in hypnosis; but two other *S*s (one an equally good student, and the other only of fair standing) did play this game of being four years old. By counting each of their ages also as 57 months, the first showed an I. Q. of 144, the second an I. Q. of 189. Neither of these *S*s was previously acquainted with the Binet scale.

Judging from the above results, the idea of definite strata of the personality is left without conclusive proof. To have come to a definite opinion one way or the other would have taken more time than *E* had to spare to the question.

A statement of the *S* to whom the Binet test was given in hypnosis, made after being awakened, that he had just been dreaming that he was in a tomb in Egypt, speaking to a wonderful woman, telling her in grandiloquent language about himself as a child, makes the *E* believe that, at least in this case, the *S*, all unconsciously and with great verisimilitude, was playing a rôle which was in some fashion witnessed by another part of the personality than that which was directing the acting. It may be said that within the limits of the four-year old suggestion, there was great freedom of action. But that the *S* is actually circumscribed within the limit of the early memories, or that the dissociation of personality is so complete as to leave a four-year old personality intact, is hard to believe, if for no other reason than that the very voice to which he is responding has no part in those early memories, and yet somehow in hypnosis is indissolubly bound up with them.

This series of experiments seems to point to an actually increased power of remembering past events which is in striking contrast with the merely equal ability in remembering present matters. This conclusion is the same as that of most men who have done experimenting in this field, notably Binet and Feré[13] and Loewenfeld.[14] The fact that some of the memories are not true in minor (or important) details, does not invalidate them as memories, any more than one's mistake in the year in which he received a certain injury invalidates his memory of having received the scar.

III. Post-Hypnotic Amensia

On no question in regard to hypnosis has there been greater disagreement than on that of post-hypnotic amnesia. One group of writers, including Bramwell and Wingfield, considers it as the distinguishing mark of the somnambulistic state; another group, including Bernheim and Loewenfeld,[15] thinks that it may or may not follow upon somnambulism. Bernheim held that the memory for the hypnotic events could always be recovered.

In order to deal summarily with what could be dealt with at great length, *E* will only state that his experimentation confirmed in great part the opinion of Loewenfeld and Bernheim, give the experimental results, and draw certain conclusions from them.

As a measurable method of judging the amount of amnesia which hypnosis carries with it, the adjective-noun, logical association, etc. material of experiments 10, 11, 12, and 13 of the earlier article was used.[16] The procedure was devised in order to give three sets of data: (a) the amount of amnesia which spontaneously occurs from hypnosis; (b) the amount when the *S* in hypnosis is ordered to remember; (c) the amount that occurs when the *S* is ordered to forget. Scores were obtained from the light, the deep, and the somnambulistic *S*s; but since the somnambulistic were the only ones who differed noticeably in the results under the three types of instructions, only their scores are of interest here.

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[13] *Cf.* J. Grasset, *Hypnotisme et suggestion,* 1916, 283.

[14] Moll, *op. cit.,* 145 f.

[15] L. Loewenfeld, *Hypnotismus und Medizin,* 1922, 141.

[16] See former article, 224 f.

It is to be kept in mind that to request the just awakened *S* to tell all that he remembers of the seance just ended, or to ask him if he remembers this or that, is an entirely different matter from making measurable tests with materials which have been definitely learned and scored in hypnosis and which can also be definitely scored in the succeeding waking period, both by noting what is retained and what is saved by relearning. Furthermore, the validity of the tests was assured by the *S* not knowing how, when, or whether he had learned these materials.

Results. *S* 19 on awaking from hypnosis sometimes could give only the meagerest report of the hypnotic session, and sometimes could give none at all. This *S* without any suggestion as to remembering, made practically perfect scores in the post-hypnotic quiz of what he had just learned in hypnosis; made the same high scores, of course, when he was ordered to remember; but when he had been ordered to forget made scores of about 80% (and that only with the greatest of mental effort, accompanied by doubt and hesitation). *S* 22 when asked on awaking what he remembered of an hour and a half of tests given in hypnosis, would, characteristically, say that he seemed to have been talking to somebody as he lay on the couch for *two or three minutes.* Even without a suggestion, *S* 22 retained all the associations learned, but did not remember *when* he had learned them. When, however, he had been given the suggestion to forget, this *S* was, for a time, hopelessly at sea in the immediately succeeding session; but, even so, on the average gave 50% of the associations correctly. In this case he would seem to remember that he had learned the list at a session the week before—in a waking session, so he said. *S* 21, after an hypnotic session, could report sometimes on one or two incidents in the hypnotic sleep, *e.g.* on having seen "a bright card" on which he had really had the hallucination of a certain picture; or sometimes on none at all. This *S* made practically perfect scores with and without the suggestion to remember; although he always affirmed that he had no memory of having learned the associations—"they just come." Under the suggestion to forget, however, all the associations were lost, in the immediately succeeding waking session, but not altogether for later sessions of waking. *S* 20 on waking always said merely that he had dreamt he was here or there, and that he could give no account of anything that had happened, although such a statement is not strictly true, because twice out of thirteen seances he remembered 'seeing' himself doing one of the tasks. *S* 20 not only disclaimed all knowledge, on awaking, of what he had just learned in hypnosis, but also failed in hypnosis to recognize what he had just learned in waking. And yet this *S* did not show complete amnesia when given definite tests. For the first two sessions he, like *S* 21, gave merely haphazard associations, but later, when a great number of associations—some of which were learned in hypnosis and some in waking—were in his mind, the difference between what had been learned in the two states became progressively less. With this *S* no suggestion at all was as powerful as the suggestion to forget. When, however, he had been ordered to remember, he retained the associations, but not the feeling of having learned them. It seemed to him as though he had stood outside of himself and had watched himself learn them.

As one notices, the above results were results for an hypnotic session which immediately followed a waking session. There were, for each of the *S*s referred to, an average of 3.5 of these double sessions. The amount of the post-hypnotic amnesia, however, for all *S*s became progressively less with time and with the number of associations which were, so to say, jostling for emergence, so that after a few weeks (or even a few days, if several sessions were held within that time) the distinction between what had been learned in hypnosis and what had been learned in waking had almost entirely broken down.

By another refinement the effect of suggestion was made manifest. By telling *S* that, when he awakened, he would be unable to recall any of the associations until a certain signal had been given, the fact of very great differences in the ability to remember before and after the signal, were established. The result, as one would suppose, varied with the *S*; but they were noticeably different with every somnambulistic *S*.

Conclusions. (a) That post-hypnotic amnesia varies greatly with the *S* is a conclusion in keeping with the contentions of many of the writers on hypnosis.[17] (b) It is exceedingly doubtful that there is ever total post-hypnotic amnesia. The *S* may not, of course, be able voluntarily to recall the events of the seance; he may not be able even to recognize them when they are rehearsed to him; but that he has retained something of them seems clear, at least in this experiment, from his giving in waking associations learned in hypnosis, and from his bettered score in relearning in waking what had been taught him in hypnosis. (c) Consequently, in reporting a *S* as exhibiting post-hypnotic amnesia, the *E* should always specify whether the *S* cannot recall, or cannot recognize, or to what extent his retention of the hypnotic activities has been affected as shown by his post-hypnotic performance. (d) The more numerous and the more similar the materials learned in the two states, and the longer the time since the learning, the less likelihood of the *S* keeping the hypnotic memories dissociated from the waking memories. (e) The Freudian theory of forgetting seems to be borne out in the effect *E*'s suggestion to forget has in lowering the post-hypnotic performance. (f) Post-hypnotic amnesia is no more valid a criterion for genuine hypnosis than dreamlessness is of real sleep. The fact of remembering does not militate against the reality of one or the other.

IV. Induction of 'Color Blindness'

The materials for this test were the following numbers of the Holmgren set: 27, 32, 34, 36, 39, 43, 45, 48, 58, 60, 61, 62, 63, 71, 75, 76, 82, 84, 85, 87, 88, 90, 92, 93, 94, 97, 98, 100, 108, 111, 113, 117, 118, 121, 126, 129, 131, 132, 134, 136.

After *E* had given the suggestion to the hypnotized *S*s that all the *red* and *green* skeins in this collection were now yellows, and that they differed only in brightness, he asked the *S*s to pick out all the yellows and to put them in a series, with the brightest yellow at the extreme left and the darkest yellow at the extreme right. Before the task was undertaken, *E* saw to it that the suggestion had been accepted, by having the *S* pick out various 'yellows.' In the waking session, which in this experiment always occurred after the hypnotic, the same yarns were used; but this time with the instructions to pick out the *yellows*, *greens* and *reds*, and to place them in a series according to their brightness values, without any regard to their colors.

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[17] *E.g.* J. Delboeuf; see Moll, *op. cit.*, 58.

Results. *S* 19 made the following arrangement in waking; the first number representing the brightest yarn, the last representing what he considered the darkest: 76, 61, 62, 43, 117, 88, 36, 58, 131, 94, 118, 132, 85, 46, 45, 126, 98, 121, 136, 129, 90, 60. The following is his selection made in hypnosis: 76, 93, 61, 118, 90, 88, 94, 43, 45, 132, 129, 126, 36, 98, 27, 58, 62, 131, 85, 121, 60. On awaking from hypnosis, he said he remembered putting some yellow yarns in the order of their brightness.

S 20 made two selections in waking and two in hypnosis; only one each will be given. One arrangement made in *waking* was as follows: 62, 94, 61, 88, 58, 36, 76, 118, 93, 43, 117, 85, 46, 45, 126, 129, 132, 121, 60, 111. The following numbers show one selection made in hypnosis: 76, 93, 61, 62, 43, 85, 117, 88, 58, 36, 118, 94, 131, 27, 98, 121, 136, 45, 126, 132, 129, 133, 108, 90, 134. After awaking, even when shown the yarns, this subject knew nothing of what he had done.

S 21 gave the following order in *waking*; noting only one of three trials, all very much alike in results: 46, 136, 60, 131, 85, 121, 98, 58, 36, 62, 27, 132, 117, 43, 129, 61, 94, 93, 76, 88, 118, 45, 126, 90; and the following in hypnosis, 46, 136, 131, 121, 60, 58, 36, 132, 85, 94, 62, 98, 117, 88, 61, 43, 27, 129, 118, 93, 45, 76, 126, 90. On being awakened the *S* remembered having seen some greenish-yellow and reddish-yellow skeins.

S 22's choices are typified by the following order of numbers: in *waking*, 90, 129, 133, 88, 126, 45, 132, 93, 76, 118, 61, 62, 117, 43, 85, 36, 58, 94, 27, 108, 131, 86, 121, 136, 60, 46; in *hypnosis*, 90, 111, 129, 132, 45, 126, 133, 93, 76, 61, 108, 118, 94, 27, 98, 121, 58, 36, 88, 131, 62.

Whether these results are anything more than a curiosity is very questionable. They are given in order that those interested in color theories may interpret them to their heart's content. Whether the subject did or did not see the reds and greens as yellows, is a problem which strikes at the very nature of the hypnotic consciousness, but into which, for lack of a refined technique, *E* could not go.

V. Spontaneous Catalepsy

E's results agree with those of Moll, who concludes his discussion of the question as follows: "I have hardly ever clearly seen a typical *flexibilitas cerea* in hypnosis, except when the training of the subject had been directed to that point."[18] If *E* begins with passes, then proceeds to bend the *S*'s arm, keeping the elbow on the couch and holding the hand in the air ever so short a time, *S* gets the suggestion that the thing to do is to leave the hand where it has been placed. From this simple posture, to one in which the leg, when raised, keeps the position without falling back to the couch, is only a matter of further, unexpressed, but still very easily felt, suggestion. By avoiding manipulating the limbs of the *S*s, the present *E* found that none of the somnambulistic *S*s ever showed any tendency to spontaneous catalepsy. In the very deepest stages, an arm when raised fell back to the couch with a thud. A greatly increased susceptibility to plasticity of the limbs does occur, but the popular conception that the arm or leg will mechanically stay in any position it is placed, without suggestion, seems to have arisen from the method of inducing hypnotism by stroking and manipulating the limbs. Really, the limb seems to stay in a certain position because it was *put* there. If there is never any moulding of the *S*'s limbs, an arm when raised, just as innatural sleep, falls like so much lead.

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[18] Moll, *op. cit.*, 80.

VI. Rapport

The phenomenon of isolated rapport, *i.e.* the state in which the hypnotized *S* attends only to the experimenter, was, for the most part, verified in the case of somnambulistic *S*s, but not for any others.

VII. Future Problems

Some problems, aside from the question as to the difference in capacity in the two states, which should be investigated are the following:

(1) The question of strata of the personality.
(2) The effect of waking purpose on hypnotic performance.
(3) The amount of initiative compatible with hypnotic sleep.
(4) The actuality of hypnotic hallucinations.[19]
(5) Humor in hypnosis.
(6) Artificially induced color-blindness.
(7) Whether a person is susceptible to more training, *e.g.* in discrimination, in hypnosis than in waking.

VIII. General Conclusions

In addition to the observations at the end of the previous article,[20] *E* would stress the following points.

(1) The results described in this article, as far as they affect both waking and hypnosis, seem to confirm conclusions drawn in the earlier article; namely, that on the whole there is no noticeable difference between the normal and hypnotic states in the ability of normal persons in the fields of sensation, perception, fine discrimination, present memory (learning and retention), or physical work which does not involve fatigue. This research, however, has shown, as many experiments heretofore had proved, that (at least in some persons) memory for long past events is much better in hypnosis than in waking.

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[19] See B. Sidis, Are there hypnotic hallucinations?, *Psychol. Rev.*, 13, 1906, 239.

[20] Former article, 230 ff.

(2) The results, as far as they affect only hypnosis, agree with the prevalent conception that rapport spontaneously appears in the deeper stages of hypnosis. They agree also with the idea that memory is greatly affected in the post-hypnotic period. The results, however, are distinctly negative in regard to spontaneous catalepsy and complete post-hypnotic amnesia as differentiae of any grade of hypnosis. Definite proof, in measurable form, has been found for widely different degrees of post-hypnotic amnesia, even in somnambulistic *S*s. The suggestion is offered that hereafter an *E* who reports a *S* as amnesic for the hypnotic incidents, should specify what form and what degree this post-hypnotic amnesia takes.

(3) The hypnosis met with in these experiments seems to be self-hypnosis. *S* 19 in normal life exhibits powers of concentration bordering on deep abstraction; *S* 20 often goes into a reverie amounting sometimes to an ecstasy, while, as priest, he is counting his beads; *S*s 13 and 14 can put themselves into natural sleep almost at will; *S* 21 has several times hypnotized himself; *S* 22 invariably goes to sleep while riding home on the train, and wakes up just when the train gets to the stop before his own. All that *E* added to these men was a better chance to concentrate than usual. It seems that prestige is an effective means of bringing about a concentration of the attention, *i.e.* if an important person is saying something, it must be worth listening to.

(4) For the most part, hypnosis seems to consist in taking, with great conviction, an attitude of mind. Even though all mental functions in the two states should be proved to be equal, still the differences in what the subject will and will not do, *i.e.* his *ability* to take an attitude, are enough to differentiate the two states. This ability to play a part, so to speak, and yet be quite unconscious that he is playing it, may effect increased memory for long past events in much the same way that, with some persons, throwing of the body into a certain posture may enable them to recall incidents in connection with a certain man who habitually exhibited that posture. This taking of an attitude (consequently *refusing* to feel the pain or even to take cognizance of it) seems to be a description of the state of mind when analgesia or resistance to fatigue is induced in the hypnotic subject.

The basic difference, then, between the normal and the hypnotic states seems to be a difference, not in strictly mental or even physical abilities, but in the attitudes which can be assumed with verisimilitude. These hypnotic attitudes are emotional, impulsive, and voluntary rather than intellectual. "This would leave the term 'hypnosis' with a fairly definite meaning: a state in which a person *will do*, in a *bona fide* manner, possessed of conviction, what he *will not do* in waking life for lack of such conviction."[21]

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[21] Former article, 232.

A METHOD FOR THE EXPERIMENTAL PRODUCTION OF EMOTIONS

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A description is here given of a method for the production of emotions in trained and sufficiently willing *O*s which was successfully employed during a series of experiments on the blunting of emotions.

Colours (or nonsense syllables) are presented in rapid succession and in different series and combinations to an *O* who has to perform given reaction-tasks in connection with them. The *O*'s success or failure in the performance of a particular task is (unknown to the *O*) determined by *E* who causes this to take place under different conditions and in various cases to be described later. The success or failure under a given condition becomes the causal antecedent for a particular emotion to arise.

The colours (or nonsense syllables) are presented to the *O*s by means of a Ranschburg memory-apparatus with two reaction-keys, one for *E* and one for *O*, inserted between it and the cells. The discs, which are placed in the memory apparatus and whose divisions are coloured according to a definite scheme, are caused to revolve, a section at a time every $\frac{1}{2}$-sec. to $\frac{3}{4}$-sec., *e.g.* by means of a Bowditch-Balzar contact-clock as supplied by Zimmermann. Punishment can be applied when necessary by means of shocks from a small inductorium. *O* is separated from *E* by means of a partition and the apparatus should be placed as far as possible on *E*'s side.

The arrangement of colours on each disc differs from that on the others. The first type of experiment is carried out with a disc on which the colours are so arranged that *O* can fulfil his instructions. The divisions are *e.g.* coloured as follows.[1] Two of them, separated by a more or less equal number of divisions, are pale green each being spotted with three black dots. With the view to forming easily avoidable traps other divisions at irregular intervals are coloured somewhat similarly; *viz.*, brown with three black dots, pale green with a dash and two black dots, pale green with "3 dots" written on it, white with three green dots, black with three green dots, pale green with the figure 3 succeeded by three black dots and another 3, dark green with three black dots, pale green with a dash, three black dots and another dash, yellow with three black dots, and pale green with three dashes. The remaining divisions of the disc are painted arbitrarily. Before each experiment *O* is instructed to react by pressing the button of his reaction-key as soon as pale green spotted with three black dots appears.[2] He is warned that he will be punished for an incorrect reaction. As soon as *O* reacts *E* announces his judgment of the reaction as 'right' or 'wrong.' The object aimed at with the experiment with this disc is to cause *O* to react correctly under conditions which create in him the impression that his correct reaction is a praiseworthy achievement. This impression is created by means of possible inducements for faulty reactions which are introduced into the arrangement of colours; these 'traps,' given above, are however so constructed that *O* can readily recognise them as such and avoid them without being aware that this avoidance is what is expected. Introspection reveals that satisfaction normally accompanies the correct reactions although blunting sets in after a few repetitions.

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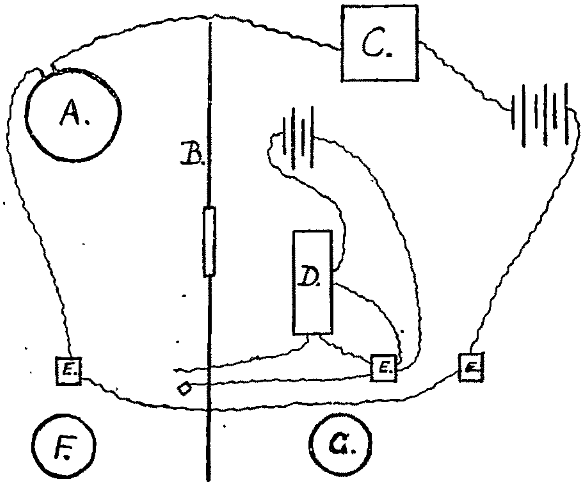
[1] Only a horizontal strip across the middle of each division should be coloured, in order to prevent the *O*s from seeing the following colour prematurely.



The following are examples of reports obtained in connection with experiments carried out as described above.[3]

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[2] The *O* is told to hold down his key. This enables *E* to break the circuit and so prevent a further movement of the disc since this will divert the attention of the *O* and since it is desirable that *O* shall see the result of his reaction plainly before him.

[3] These reports have been translated as accurately as possible from Afrikaans in which language they were given.

Satisfaction. (1) A.[4] I was prepared to react when I saw pale green with three dots. I think that this was so because my attention was well concentrated on the task and nothing distracted it. I reacted as soon as the colour appeared and with a fair amount of satisfaction realised that I was right. A slight uncertainty prevailed for just a moment but I was soon convinced that my reaction could not be wrong. *E*'s judgment increased my satisfaction and also pleased me as I now knew that the unpleasant punishment could not be applied this time.

(2) B. My attention was riveted on the rotating disc—more so than on the task to press the button of the reaction-key. I thought (I do not know exactly when) that the latter action depends upon perception of the former. A very high state of tension was present during the rotation of the disc. It was unpleasant and contained, it seems to me, something of fear or anxious expectation. While the disc was revolving I was chiefly aware of the fact that wrong reactions and punishment were possible, as well as of a desire to evade the latter. I do not, however, remember any clear desire for correctness of reaction as such. The relief which arose when I saw that I was reacting correctly was not very strong; I undoubtedly plainly felt satisfaction when I became conscious of the disc standing at the correct combination of colours. This emotion remained for some time. I feel sure that it did not disappear directly since my attention was distracted by a sudden movement of the disc[5] and yet I immediately afterwards noted that the consciousness of success accompanied by satisfaction was still present. The satisfaction did not follow my reaction at once but only arose when I realised that I had been successful, *i.e.* when it was as if I praised myself. I do not remember any thoughts relating to the *E* in connection with my satisfaction. The satisfaction was practically the same kind of emotion (although not so intense) which one feels when one has just completed an important piece of work well.

(3) F. I watched the disc fixedly and wondered whether I would react correctly or not. Several apparent reaction-colours passed; but I did not feel much tempted to react. The disc seemed to move rather rapidly. I reacted correctly and knew that this was so. I had a pleasant feeling of satisfaction—no delight—for although owing to former correct reactions I did not look upon my reaction as something exceptional, yet it was nice to react quickly and surely. The satisfaction was of the same kind that one feels when one has made a good stroke in a fast set of tennis. I do not remember thinking about the punishment at all.

(4) G. My attention was firmly fixed on the disc as I awaited the appearance of the reaction-colour. I reacted with decision when it appeared and when I saw the disc stand at the correct position a strong satisfaction with my action made itself felt. I thought that the *E* could not do otherwise than judge my reaction favourably. I fully realised that I had completely fulfilled his instructions. The idea that I was free from any possibility of punishment was only vaguely present when the *E* said 'right' and the satisfaction increased.

(5) H. I desired to react correctly since it is unpleasant to be unable to fulfil one's task, and made a special effort to do so. My attention became well fixed on the disc towards the end of the experiment when I expected the reaction-position. I remembered the punishment and thought that by reacting correctly I would prevent the *E* from applying it. Then I saw pale green with three dots and hurriedly pressed my button. I was so glad that my reaction was correct that I nearly laughed. I was well

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[4] The letter preceding each example serves to denote the *O* whose report follows in case comparison with other reports by the same *O* in connection with the further types of experiment should be desired. The letters also indicate how the reports cited were given by various *O*s.

[5] *E*'s carelessness.

satisfied for I knew that my reaction was a good one and the fulfilment of my wish. I also remembered that I would now receive no punishment and thought that I deserved to escape it. All this made me feel pleased and satisfied. Even now I feel quite cheerful.

The colours on the disc for a second type of experiment are so arranged that the *O*s will be induced to react at a position of the disc which is not the correct reaction-position. Seven divisions of this disc are each coloured in three equal sections of blue, red and green, respectively, and an eighth division blue, vermilion and green. These eight divisions are separated from one another by irregular numbers of spaces. Each of these eight combinations is immediately preceded by a blue division which in turn follows upon a yellow one.[6] The remaining divisions of the disc are painted other colours, with the exception of a few cases of yellow, not followed by blue, which serve to keep the *O*'s attention fixed on the disc. The *O* is instructed to react whenever the blue-red-green combination appears, and is told that this takes place after the appearance of a blue division preceded by a yellow. He is asked to endeavour to react correctly in every instance and is warned that a reaction when blue-vermilion-green appears is quite wrong and will be followed by punishment. The difference in the discs is shown him beforehand. *O*s are usually entrapped the first time if the disc is so placed in the memory apparatus that they must react at the seven correct reaction-positions before the wrong combination appears. The conditions can however be kept favourable for entrapping the *O*s in repetitions of the experiment with this disc. *E* attains this end by varying the position of the disc after each appearance of the trap so that *O* never knows how often the true combination will be exposed before the trap again appears. Now and then, for instance, by adjusting the disc before each experiment, the trap is allowed to appear several times in succession. The rapidity of the motion of the disc must also be adapted to the *O*s in order to obtain the best results but although the disc should move rapidly enough to entrap the *O*s the movement may not be so rapid as to cause the task to appear impossible to them. The *O*s are punished with a painful shock whenever they are entrapped. Usually, before blunting takes place, keen disappointment, and sometimes regret, is felt in connection with incorrect reactions at the trap, and exaltation or elation in the few cases when an *O* is not caught in the trap. Satisfaction is felt at the usual correct reactions at the true reaction-positions and is especially pronounced after the *O* has been entrapped several times in succession. Even delight is sometimes felt when the *O* is brought under the impression that he is reacting correctly at the ordinary reaction-positions under specially difficult circumstances, *e.g.* after the *E* has altered the position of the disc. Shame is sometimes felt in connection with this type of disc when an *O* is caught in the trap and when the realization of punishment *for a silly mistake* predominates over the realization of an incorrect reaction. A variation from the ordinary routine of the experiment can be introduced by informing the *O* that he will receive no punishment for incorrect reactions at the trap since such failures are in themselves sufficient punishment. According to the *Os'* reports the disappointment is very often just as keen as in the other cases.

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[6] The combinations are not allowed to appear without warning-colours although a sudden appearance of the trap will apparently favour incorrect reaction. The absence of warning-colours is however disadvantageous in as far as undesired mistakes such as late or no reactions will then occur more easily at the correct combinations. Besides this the presence of warning-colours has the advantage of causing the *O*s to be sufficiently prepared for reaction since these colours are for the *O*s par excellence the warning-colours in connection with the true combinations even though they also precede the trap.

Disappointment. (1) A. I remember that at the beginning of the experiment I wished to avoid an incorrect reaction. I was interested in the experiment and attempted to react correctly. I sat looking attentively at the disc in a state of readiness for reaction. The warning-colours made me still more attentive. While I was pressing the button of my reaction-key, or immediately afterwards, I remember looking at the disc to make sure whether I was right or wrong. I was really very much disappointed when I saw that I had been caught in the trap, which I had really wished to avoid. The disappointment arose when I became aware of the fact that I had reacted on the blue-vermilion-green and that this was wrong. The disappointment was afterwards accompanied by a tendency to an embarrassed kind of laughter.

(2) C. This time I again began with a fair amount of self-confidence, although I was quite nervous. I felt that I had a certain reputation to maintain and was afraid that I should lose it. I intended to look out for the yellow as I wished to make sure that I would see the yellow strip. A certain colour appeared which confused me as it resembled yellow. Expectation of the warning-color blue immediately filled my mind which then wandered for I began to think of the effect of the blue colour on me. I felt very uncomfortable when I noticed my distraction. Just then I saw blue disappear in the slit. As the combination appeared I thought for a moment that it was the wrong one, but I felt as if I could not decide soon enough with any certainty and reacted. I had only just reacted when I saw that I had made a mistake. My disappointment was very great especially when I realised that I should have acted according to my first impression. This disappointment became still more accentuated when the *E* said, "Trap—wrong." The punishment served to make me realise more emphatically that I had done wrong.

(3) D. I was in a very high state of tension and did not remember the warning-colours properly. I did not think of the wrong combination at all, but was quite determined to react correctly. I forgot about the yellow altogether and no colour had any effect upon me until I suddenly saw blue. Its appearance startled me and somewhat confused me; but was nevertheless followed by an expectation of the right combination. I also noticed the blue and green portions when the combination appeared but felt quite convinced that it was the correct one. I reacted with decision and a fair

amount of satisfaction. My disappointment was altogether painful when the *E* said "Trap" unexpectedly and when I saw that I had indeed been entrapped. This disappointment was accompanied by a little shame at my having been so sure of the correctness of my reaction.

(4) E. I recognised blue as warning-colour and sat ready to react at the following position. I was partially in doubt what to do when the following combination appeared and reacted more or less automatically. Then I saw that I was wrong. An emotion of disappointment and dissatisfaction with myself arose and I thought that punishment would undoubtedly follow. The punishment was painful and after its application disappointment with my incorrect reaction increased. My whole condition was very unpleasant; I do not know whether the painful sensation or the emotion was the chief constituent—at any rate both were pronounced and in the foreground of consciousness.

Regret. (1) F. I sat awaiting the reaction-position very determined to look well and react correctly. Once or twice yellow drew my attention but was not followed by blue and the combination. When yellow again appeared after these 'bluffs' I expected the correct colours to follow. The fact that there was a combination of colours finally attracted my notice more than the colours themselves. Owing to this I reacted. I immediately saw that I had reacted incorrectly and the disappointment and regret were so strong that I even heaved a sigh involuntarily as I released the button of the reaction-key.

(2) G. I was in a high state of tension especially after a few colours had passed without those appearing on which I had to react. I again determined to react and was quite ready for reaction and every time slightly pressed the button. When the wrong combination appeared the expectation was so strong that I reacted unintentionally. Just as I reacted I saw that I was doing wrong and tried to jerk back my hand in time but it was too late. I thought that I should have escaped this mistake altogether and that I had only myself to blame for it. I felt a fairly pronounced regret.

Exaltation. (1) C. I began this experiment in a higher state of tension than on previous occasions, since I expected the wrong combination but was afraid that I would not recognise it. I did not wish to react at the wrong combination. I was very plainly conscious of the punishment-factor in the experiment and was afraid of it. The consciousness of possible punishment made me nervous and this nervousness increased while the *E* delayed the movement of the disc. I became calm and collected when the disc began to move and watched the colours with a fair amount of self-confidence. When yellow appeared I was immediately on my guard and my mind was completely filled with thoughts in connection with the wrong combination. Expectation of the wrong combination increased when I saw the blue strip. When the combination itself appeared I became confused and nervous for a moment. Then I recognised the combination as the wrong one and with extreme self-satisfaction and a decided feeling of elation or exaltation I sat still looking at it. Together with the elation was the thought that I had not been entrapped this time. I felt disappointed when the *E* did not say "right" as is usually the case and as I expected. I expected praise and commendation most decidedly and in fact also desired it.

(2) E. After yellow and blue the combination appeared. Although quite prepared for reaction, something hindered me and seemed to prevent me from reacting. (I do not know what it was.) Then I recognised the vermilion and although the doubt as to whether I should react or not, which had been present at first, now disappeared, something of fear persisted until I realised with perfect certainty that I had done right to allow the trap to pass. Then I felt quite a strong satisfaction and a real feeling

of exaltation which made me generally pleased. When *E* stopped the disc I again doubted for a moment whether I would be punished or not but this doubt was very transitory.

(3) G. In connection with the combination blue-vermilion-green there was a very strong feeling of satisfaction with the fact that I had not been entrapped this time. An inclination to laughter and a feeling of triumph were also present. These emotions arose in connection with the thought that *E* did not succeed in catching me in the trap. At the commencement of the experiment I became aware of a desire not to react incorrectly. I was aware of strong interest in the experiment and in my task, and I concentrated my attention on the colours to be ready for reaction. The situation was a pleasant one. I cannot remember that thoughts in connection with the trap were present during the rotation of the disc. Heightened attention followed the recognition of the warning-colours and it seemed to me as if my fingers contracted slightly when blue-vermilion-green appeared. The satisfaction and elation did not last long and I sat wondering what would happen next.

Satisfaction. C. I felt hopeful although I had reacted wrongly on the former two occasions. I thought that I ought to remember the succession of the warning-colours and combination properly, and decided to concentrate on recognising the yellow strip. I decided to react correctly and was in a high state of tension. When yellow appeared I expected blue. I felt somewhat relieved when blue actually did follow and thought that I was expecting the colours in the correct sequence. The combination then appeared and I recognised it immediately. I reacted correctly when I saw the ordinary true combination. There was a very strong satisfaction when I reacted correctly and although I was sure of my reaction it became stronger still when *E* confirmed my judgment. I then felt that I had been able to depend upon myself to react correctly and in connection with this there was a slight emotion of pride which increased somewhat when I thought that *E* must be satisfied with me.

Delight. F. The tension was very great this time for owing to the alteration of the position of the disc by the *E* I did not know when to expect the reaction-colours.[7] They might even appear immediately. I was surprised when several colours passed by without the warning-colours appearing for I thought that the *E* had placed them very near the slit. I nearly reacted at times simply owing to the tendency to press which I could feel in my fingers. When the combination appeared I felt uncertain and hesitated and so nearly reacted too late. The delight was very strong indeed when I saw that I was right and when the *E* said so too. I was well aware of the fact that I had nearly made a mistake and also that the *E* by moving the disc had increased the difficulties so that my reaction under the circumstances was really an achievement.

In a third type of experiment no reaction is required of the *O*s; but they are instructed to pay careful attention to the disc which is allowed to revolve comparatively slowly. The colours on the disc are arranged as follows; yellow, red and purple succeed one another in this order at seven places; at intervals yellow, or yellow succeeded by red, is inserted in order to secure the *O*'s constant attention on the disc; and the remaining divisions are painted other colours. The *O*s are told that yellow, red and purple will succeed one another at intervals and that a severe shock (of the painfulness of which the *O*s are given a foretaste) will be applied whenever purple appears in the slit of the memory apparatus. Expectation of the shock is produced by the colour in question being preceded by warning-colours which the *O*s know as such. The appearance of the warning-colours makes the conditions favourable for fear to arise and this actually normally took place during experimentation. The *E* stops the disc on purple before applying the shock. Relief can be produced by a similar experiment if a mild shock is given after several severe ones—the *O* must however then be asked for introspection a few seconds after the shock in order that the relief may first assert itself plainly.

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[7] The increased difficulty was only imaginary for in reality the *O*s never knew with any certainty when to expect these colours.

Fear. (1) A. I sat looking at the colours with a certain amount of tension and had the expectation that a painful shock would be applied. Nevertheless I smiled although this smile contained something of fear. It was a pained kind of tendency to laughter and embraced an unpleasant feeling of impotence. I also wished that the colours would appear quickly so that the shock could be given and become something of the past. Suspense was present throughout the experiment. I intended to bear the pain with resignation and not to allow it to upset me. When yellow appeared I thought that red would follow but once or twice it did not. At last red appeared after yellow and immediately there was a heightened fearful expectation of the shock. I disliked thinking of the latter. Then purple appeared and the thought that now punishment would follow directly came clearly to my mind. It was as if I prepared myself for something very unpleasant and the fear increased until the shock was actually applied.

(2) B. I again thought of the task. When the disc began to revolve I immediately experienced an emotion of fear in connection with the expectation of what was about to happen. This feeling increased especially when yellow and then red appeared for the pain seemed to come nearer. Then I saw the purple in the slit and I attempted to resign myself to the inevitable, *i.e.* I tried to suggest to myself that I would not feel the pain. But this thought did not help at all, and the pain was rather more intense than before.

(3) G. I became slightly afraid as soon as the disc commenced its movement. I remember that I thought that my feeling must in a slight degree resemble that of people about to be shot or hanged who hear the footsteps of their executors and cannot help knowing that their end is near. When the warning-colours appeared I knew similarly that I would receive a painful shock which I dislike very much. There was also a certain amount of tension. When yellow appeared I realised that the shock would soon follow and the former slight fear became stronger. I noticed that I was afraid of the pain and desired that I should not be given the shock. This fear became more intense still when purple appeared so that an unmistakable emotion of fear was present when the shock was at last applied.

(4) H. I paid careful attention to the disc and firmly expected that *E* was going to apply the shock at the first occurrence of purple. I was in a state of continual expectation of the shock and attempted to distinguish the colours properly so that I would know when it was due. I was very much afraid. I saw yellow and then red and then did not feel that it was necessary to look out for purple since I knew that the shock was coming. I felt more afraid while these colours were appearing than I did before since it seemed to me that they followed each other too soon to enable me to become mentally prepared for the pain. *E* then punished me and I soon wished that he would stop. I called out: "It hurts!" and hoped that my

exclamation would move *E*. It seems to me that the fact that the punishment which is very painful was repeated during this period a number of times in succession accentuated my fear for no respite was given. Yellow which used to be a colour I liked is becoming quite distasteful to me, and purple has been very disagreeable all along.

Fear and Relief. A. In a few cases yellow appeared and I remember thinking that I would receive a shock. I clearly noticed an increase of the fear and dread. In one case yellow was followed by red and I firmly expected purple and punishment. The fear and dread were quite intense while red was exposed and a well-defined relief arose when the succeeding colour was not purple. This relief seemed to be chiefly connected with my breathing. Then my attention again became fixed on the colours and in my mind the idea formed that I would be punished in any case and this was unpleasant. Then yellow and red again appeared and suspense and fear reasserted themselves. I thought that I would surely be punished now. The disc stopped at purple and I decidedly expected the shock. The fear remained while I awaited the pain. The latter was very intense and was accompanied as in other cases by a kind of confusion. I must mention the fact that I did not approach this experiment in the same spirit as former experiments. In the latter cases the fact that I had to react interested me and made me approach the experiment in a pleasant general emotional state, no matter what happened afterwards. In the case of this experiment, however, I am compelled to await the painful shock passively. This seems to give an unpleasant tone to my general state.

A further principle can be introduced in connection with a fourth disc, which is also so constructed that the *O*s can be entrapped regularly. This is that *O* is instructed to punish *E* for his (*O*'s) mistakes instead of the usual procedure being followed, namely the punishment of *O* by *E*. *O* then normally feels compassion for or sympathy with *E*—although sometimes shame and embarrassment occur. When we performed this experiment one *O* felt malicious joy at punishing *E* in a few cases. (Satisfaction at the usual correct reactions and exaltation at escaping the trap are particularly strong in this type of experiment whenever the *O*s make a special effort in order to avoid the necessity of punishing *E*.) The following disc can be used. A blue-green combination (*i.e.* half of the coloured strip blue and the other half green) which is preceded by a red is followed six times by a purple-black-brown combination. Once, however, black-purple-brown succeeds these warning-colours. The remaining divisions are painted other colours. Two measures for keeping *O*'s attention constantly fixed on the disc are contained in the latter; *viz.*, the insertion of red in a few positions without the succession of the combinations, and the irregular intervals between the purple-black-brown combinations. The *O*s are instructed to react as soon as purple-black-brown, which follows blue-green preceded by red, appears, but not in the case of black-purple-brown. The measures already mentioned as helpful towards entrapping *O*s can again be employed.

Compassion. (1) B. I did not feel in a very high state of tension because the fact that the warning-colours would appear set my mind at rest. My attention was chiefly occupied by watching for the warning-colours. I was caught in the trap, however, because I had not in the least expected it, partly since I had forgotten the trap and partly since the warning-colours seemed to me to be connected with the true combination. Besides this I was not clear as to my task and it seemed to me that this experiment was more complicated than former ones. Regretful irritation and an ashamed grin accompanied my discovery that I had been entrapped. At first I partially forgot that I had to punish *E*, *i.e.* thoughts concerning the punishment were only very vague. I supposed and hoped that it would not be necessary. Quite separate from former thoughts and emotions, there were other thoughts after the experiment, *viz.*, at the application of punishment. A very definite and strong emotion of pity for *E* of whom I thought as suffering for my mistakes and a really unpleasant shame and self-reproach arose. In short I extremely disliked punishing *E*.

(2) C. I began this experiment very confidently and was sure that I would react correctly. I also had a suspicion that the combination would appear soon. The appearance of red made me think this. I was not at all surprised when blue-green appeared and absolutely expected the reaction-combination. I was ready for reaction. When it appeared I pressed the key immediately and directly after this saw that I had fallen into the trap. I saw this even before *E* said so. I was very much disappointed. When I had to punish *E* I felt compassion for him in pain. I did not think of the fact that he was suffering for my mistake but was just sorry for him because he was being hurt.

(3) F. I watched the passing colours intently and noticed red passing a few times without being followed by the other colours mentioned by *E*. I thought that this formed part of *E*'s plan to entrap me. Then I thought that some of these colours might also have been brown and not red at all. I had forgotten at precisely which combination I had to react when black-purple-brown appeared and therefore I doubted whether it was the correct one. Then I reacted and was surprised when I heard that I had been entrapped. I remembered that now I had to punish the *E* for the second time in succession and I pitied him. I did not like to hurt him and I thought that he might easily think that I was making mistakes on purpose although this was not so. Indeed I sympathised with him. Then I thought that the appearance of the red colours I mentioned before and the trap really made it difficult for me to react correctly. I argued that I was not wholly to blame for my mistake and that *E* was partially to be blamed for it himself—he had made my task difficult. The compassion which I felt for him while I applied punishment was not so strong as on former occasions when I had only blamed myself.

Shame and Embarrassment. (1) C. I felt rather strained at the beginning of the experiment but not so much as before. I had more self-confidence. When the disc commenced moving I lost much of my confidence since I could not always distinguish the colours and thought that I might perhaps not recognise the correct one. I could not remember the first warning-colour. Then the combination blue-green appeared and I had a vague idea that it was somehow or other connected with the true combination. When I had allowed the true combination to pass unhindered owing to my uncertainty I felt sure that I ought to have reacted. I felt very much disappointed and strongly ashamed of myself. I expected *E* to say 'wrong' and winced at the thought that I would have to punish him. A painful embarrassment was present which I cannot describe further.

(2) D. I soon noticed that I had been entrapped. I had previously awaited the appearance of blue-green and it seemed to me to be practically certain that reaction would be necessary shortly after the appearance of this combination. I saw that my reaction was wrong immediately after I

had reacted and laughed embarrassedly. I heard *E* say 'wrong' and punished him. It seemed to me as if I should have noticed in time that the right combination was not exposed in the slit and that I had reacted half automatically and had not been attending properly. While I was punishing *E* I blamed myself for being too inattentive. The embarrassment was quite strong whereas there was only a slight unpleasant feeling when I had to punish the *E*. It was as if the embarrassment altogether predominated in my consciousness. I felt little affected by the punishment and also had the idea that *E* did not mind the shock very much. I do not remember thinking that I was punishing *E* for my mistake. I simply applied punishment automatically in these cases as a duty following upon wrong reaction. I no longer gave further attention to the application of punishment as was the case formerly when the thoughts about it made me pity *E*. The embarrassment accompanied the consciousness that I had reacted incorrectly.

Malicious Joy. A. I was not altogether concentrated on my task when red appeared. Its appearance made me expect purple-black-brown however. Still I was not quite ready to react when the combination appeared, and was entrapped. I had decided beforehand to attempt to react correctly without thinking of the fact that I had to punish *E* and without any recognised wish to avoid having to punish him. I do not remember thinking about the punishment when I saw with disappointment that I had been entrapped. A short time after I had noticed my mistake, I remembered for the first time that I had to punish the *E*. I do not remember that a definite feeling was present when I thought of this, for immediately afterwards my attention wandered to the disc which was again rotating after my mistake, and I thought that I should stop it first of all. I heard *E* say something and supposed that he meant I had committed an error. At last, just as I intended to stop the disc and to apply punishment to *E*, I felt a very clear emotion of malicious joy at the pain of *E*. There was also an impression as if I thought: "Aha, now you are going to get it." Out of the corner of my eye I saw him wince and this amused me somewhat. Then it occurred to me that I was pressing the button of the key rather too long and that it was not fair to punish him so immoderately. This thought was accompanied by an unpleasant feeling—I felt sorry for him. Then followed the idea that I must end the punishment and I immediately lifted my finger from the reaction-key.

Delight rather than satisfaction usually results at a correct reaction in connection with a fifth disc (which is also so constructed that the *O*s practically always react correctly under seemingly adverse circumstances), where the severity of the punishment is increased although not to an exaggerated and confusing extent. Whenever the *O*s are then punished unjustly (*i.e.* when their reactions are in reality correct) after they have avoided punishment several times by reacting correctly, anger is normally aroused. This only happens, however, if the *O*s recognise the injustice. The conditions are favourable for vexation to arise in connection with a disc of the same type when the *O*s are also punished unjustly but more leniently than in the former case although still painfully enough to be irritating. The disc can be coloured according to the following scheme. In four cases yellow and blue, and in four others red and green, succeed each other; each colour appears before the other of its pair in two out of the four cases. In a few other instances yellow and blue appear on the disc with another colour interposed. Similarly red, another colour and green sometimes follow one another. Other sequences of colours on the disc are yellow succeeded by red or green, and blue succeeded by red or green. Finally there are cases of the succession of pairs of colours somewhat similar to the pairs mentioned first; *e.g.* yellow and purple, vermilion and green. The remaining divisions of the disc are painted other colours. The *O*s are instructed to react whenever a colour appears which was immediately preceded by its complementary colour.

Delight. (1) C. At the commencement of the experiment I was in a fairly high state of tension since I had been thinking of the renewed difficulty of correct reaction while my eyes were closed and *E* was changing the position of the disc in the Ranschburg. A very strong desire was present to retain my record of having made no mistakes so far in connection with this disc. I was afraid of losing my reputation this time. I saw yellow appear and realised with relief that the experiment would soon be over. I absolutely expected the correct reaction-colour. When blue appeared I felt slightly nervous. Then I saw the correct combination appear and imagined that I could see that it was nearly ready to disappear again. I hesitated for a moment but suddenly realised that the combination was correct and reacted. A fairly intense delight, or joy, and self-satisfaction arose and the disappointment which had first arisen, when I did not react directly, disappeared immediately. The delight became more intense when the *E* said 'right.'

(2) E. At the commencement I was not in the mood to keep the instructions clearly in mind. When a colour appeared at which I thought I ought to react I did so without realising plainly what I was doing. I then feared that I had reacted wrongly. A consciousness of punishment which would follow was also present. When I noticed that I was right I felt delight tinged with excitement. My former doubt seemed to be suddenly displaced by the complete consciousness of having reacted correctly. When *E* pronounced judgment this delight increased. It was quite strong. I also realised that punishment would not be applied and was glad that I need not endure any pain.

Anger (*and Vexation*). (1) B. There was a fair amount of tension which increased as the disc revolved. This tension, I believe, arose in connection with my desire to fulfil my task. I did not think of the punishment. When I reacted correctly I felt satisfied but I do not know of any relief. I felt some doubt as to whether I was right and then thought of the punishment with some anxiety. Then *E* suddenly punished me for no reason. This punishment which was given although I knew I was right filled me with a vexed kind of anger. At the same time I remembered that I had placed myself in *E*'s hands for experimentation and could expect things to happen. I think that my anger was at first slight owing to this idea. I must say that this angry feeling is much stronger now and then when I think of the unjust pain caused me and forget the other considerations.

(2) D. I was sure that I had reacted correctly and felt a slight satisfaction. I heard the *E* say 'right' and then felt the punishment. This seemed to me to be contradictory. I closed my mouth tightly and felt a distinct tendency to obstinacy. I said to myself: "But it *is* correct." In spite of former unjust punishments I still had the idea that my correct reactions meant avoidance of punishment. I could not resist feeling ill-tempered. I felt angry. After a while, however, I decided that I had to tolerate the pain and my anger subsided.

Shame, finally, can be aroused in a fundamentally similar manner. A disc is so constructed that it appears to the *O*s to be easy to avoid the trap, although they are nevertheless regularly caught in it. They then become clearly conscious of their own incompetence. Furthermore, only a moderate punishment is applied which, besides ensuring a clear consciousness of the mistake, contributes to the intensity of the shame by creating the impression that *E* was certain that *O* would be caught, pitied him and therefore did not wish him to be punished too severely. The following disc can be used. Five divisions are painted green with three dots and two dashes and one green with two dots and three dashes; in each case blue appears two divisions earlier. The other divisions are painted other colours. The *O*s are instructed to react when green with three dots and two dashes appears, and are told that it is preceded by blue two divisions earlier. They are also warned against the trap, and told that it can easily be avoided so that being entrapped will be very discreditable to them and lead to punishment such as is meted out to a little child. Here again the previously described additional measures for entrapping the *O*s can be employed. (Satisfaction, and even delight immediately after a trap and the alteration of the disc's position, is felt when the *O*s react correctly at a true reaction-position.)

Shame. (1) C. I did not concentrate sufficiently on the rotating disc at first and felt startled and nervous when I noticed my inattentiveness. It was unpleasant. I awaited the colours in a very high state of tension and absolutely expected the true combination. I gave no thought whatever to the wrong one. I had just pressed the button, however, when I saw that I had been caught. I felt disappointed and very much ashamed. The latter emotion increased when *E* said "wrong—trap" and punished me. It was the first time that I really did feel the shock as a punishment. Later the disappointment increased when I considered the fact that the reason why I make mistakes is that I regularly forget the wrong combination although just before the experiment I decide to remember it.

(2) D. I noticed that the disc was moving fairly rapidly and became slightly confused. Owing to the excitement and tension, I did not distinguish clearly how many dots and dashes there were on the green. I reacted without certainty but with the idea that the chances were in favour of a correct reaction. During my reaction I already began to doubt whether I was right and when the *E* confirmed my doubt I felt disappointed and very much ashamed. I had determined to react correctly and now my record was again broken. The shame was so strong that I felt the blood rise to my head. The idea was also present that I was making mistakes like a small child. It seemed very humiliating to me. I did not think of the punishment while the shame predominated, but when I felt the shock the thought arose that I was being punished like a little child and this still further increased my shame.

(3) F. I looked intently at the colours as they passed by and noticed that I felt highly strained. The question as to when the blue would appear was continually in the foreground of consciousness. The continual possibility of blue appearing next made me strained. I did not determine how many dots and dashes were present but roughly reckoned that the number was correct, and reacted trusting that I would be right. There was never-

theless a suspicion that I might have made a mistake. *E*'s judgment that I had been entrapped filled me with a very strong shame and disappointment. The shame was stronger than on the previous occasion because this was the second time that I had been entrapped in succession. *E* waited a moment before he punished me and this increased my shame. I felt quite relieved when he did not punish me, for *no* punishment in spite of the experimental conditions would have made my shame hard to bear.

Delight. F. I sat watching the colours intently and in a high state of tension, which increased as the coloured strips passed. My tongue seemed to hang out as I sat looking at the colours. After *E* alters the position of the disc I never seem to know what is going to happen and feel flurried. The task came clearly to mind, but I did not think of the punishment. A few of the colours drew my attention but I did not seem to be conscious of the colour of the other strips—I only realised that they were not the green for which I was on the lookout. When the green with the dots and dashes appeared I just thought: "You must press quickly." A strong delight made itself felt. I knew that I had reacted as I should. I was very glad that I had reacted correctly even after the alteration of the position of the disc.

We may add that the inductorium should always be put in action before *E* allows the disc to commence its movement—except in special cases when the absence of the sound made by the inductorium is introduced as a special variation of the experimental conditions.

Many other tasks and discs can be devised on the same underlying principles. Though not done by us, one can introduce further variations in the experimental method, *e.g.* by arranging a competition between *O*s as to who should react first. Pride and jealousy will probably be aroused under the experimental conditions which will ensue if the variation just mentioned is employed—especially if the application of punishment is adapted to the particular circumstances resulting from such an arrangement of the experiment.

Fundamentally this series of experiments, although by no means perfect, possesses the following characteristics which can be demanded of an effective psychological experiment. (a) It must be possible for the processes, which are being investigated, to be produced by *E* at his discretion and under certain definite conditions when these processes can readily be observed. (b) All the conditions under which the phenomenon that is being investigated appears should as far as possible be under the control of *E* in order to enable him to vary them at will. (c) The total situation must be simple and yet natural in the sense that it must show fundamental similarity to the situations of everyday life without the confusing complexity in the resulting state of consciousness which is in most cases characteristic of the latter.

The following facts support the statement that the experiments under consideration satisfy the above-mentioned demands to a great extent. The experiments were very efficient in producing various emotions in *O*s in the psychological laboratory, even when (according to the evidence of the *O*s themselves) they had discovered what emotion was the object of the experiment concerned and attempted to resist the influence of the latter. Furthermore the nature of the emotion depends upon the varying of the conditions which are under the control of the *E*, such as: the task, the composition of the discs, the manipulation of the trap and the methods of application of the punishment. The emotions which arise are, according to the *O*s, quite natural in the sense of agreeing in quality with those of daily life, even though the contents of consciousness in connection with which the emotions arise are comparatively simple. The reproduction of various and irrelevant intellectual contents, which only produce confusing complexity, is prevented by utilising meaningless subject-matter. In this way complexity of emotional and intellectual processes is avoided and the total state of consciousness considerably simplified. Moreover *E* can to a great extent control the intellectual processes, which accompany the emotion produced, by a judicious use of the meaningless subject-matter, the task and the punishment. An important general advantage of this series of experiments is the brevity of the introspective periods which favour the reliability of the introspective reports. The duration of the period on which introspection has to be given, *i.e.* from the commencement of the rotation of the disc till *E* asks for introspection just after a correct reaction, a trap or a punishment, need never be more than five seconds and is very often less.

After the completion of experimentation the description of a somewhat similar kind of experiment came to my notice, namely, that of Levin.[8] A trial of this method convinced me, however, of the superiority of the one described above, especially in the control of the experimental conditions, in the relative certainty with which particular emotions can be intentionally produced, and in the absence, comparatively speaking, of irrelevant and disturbing elements.

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[8] K. Levin, Eine experimentelle Methode zur Erzeugung von Affekten, *Ber. VII. Kongr. f. exp. Psychol.*, 1922, 146.

THE CRITERIA OF CONFIDENCE

By Frederick Hansen Lund, Barnard College, Columbia University

Confidence, as an aspect of the processes of recall and recognition, of the attitudes of belief and disbelief, and of the various modes of perceptual life, has been a subject of growing interest to experimental psychology. It has been definitely shown that the recognition process is attended by varying degrees of confidence;[1] that the correctness of recognition tends to vary directly with the degree of confidence,[2] and that our belief-attitudes appear with varying degrees of strength, or varying degrees of confidence, assurance, or certainty.[3]

Since confidence is a common factor of our most fundamental experiences, it has proved a fruitful field of research. Studies thus far made, however, have been undertaken to determine whether there are different degrees of confidence, and to what extent the different degrees are correlated with the accuracy of recognition, of judgment, or of belief. A question of importance, however, is: "What are the criteria—the factors—which determine the degree of confidence?" It was to answer this question that the present study was undertaken. The results, though in no sense final, indicate the direction which the answer will ultimately take. First of all, however, let us consider, by way of illustration, a case in which the greatest possible range of confidence and recognition occurs.

Suppose one is waiting to meet a friend and finally a figure appears in the distance. For a while one is uncertain as to whether it is the friend. From the moment of uncertainty to the time when the person is clearly perceived, either as the expected friend or as some one else, there is a gradual recognition of the known characteristics in the one case or of the unfamiliar characteristics in the other. Every item recognized adds to one's confidence as to who the person is—whether this item be one of dress, gait, or stature—and every item becomes part of the total context which finally constitutes the clear perception. The increase in confidence and clearer recognition is attained, not only through an increase in the elements that seem familiar, but in the increase in 'intensity' of these—that is, not only do more elements come into evidence as the person approaches, but every element becomes more distinct and confidence increases accordingly. Whether the increase in confidence and recognition proceeds regularly in this manner or changes suddenly in either direction will of course depend on the nature and sequence of the items which determine them.

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[1] H. L. Hollingworth, Characteristic differences between recall and recognition, this Journal, 24, 1913, 532-544; Experimental studies in judgment, *Arch. of Psychol.*, 29, 1913, 1-96.
W. C. Trow, The psychology of confidence, *Arch. of Psychol.* 67, 1923, 7-26.

[2] Trow, *op. cit.*, 33-37.

[3] Hollingworth, *op. cit.*, *Arch. of Psychol.*, 29, 1913, 1-96.
F. H. Lund, The psychology of belief, *J. Abnor. & Soc. Psychol.*, 20, 1925, 180-190.

If this analysis is correct it would seem to indicate that the feeling of familiarity appears as a result of these unitary and successive reactions occasioned by the complex stimulus—each adding to the summative effect which leads to the complete recognition. Confidence increases according to the number and intensity of the particular items or stimuli which initiate a familiar reaction. In other words, the feeling of familiarity arises from the fact that the different characteristics of the figure occasion the same perceptual and other reactions which have been made before in the presence of the figure, and that the *ease* with which these items lead over into reactions previously made determines the degree of confidence. In neural terms this would mean that the feeling results from the passage of a nerve current along a previously used brain path or across a closely bound synapse. Recognition will be prompt and confidence greater in proportion to the frequency with which the same neural pattern has been active previously under similar conditions, and in proportion to the intensity of the present stimuli, and the absence of stimuli which produce reactions which have a different tonal quality.

If the principle presented in the above discussion recommends itself as worthy of consideration, the question is how might it be put to test under experimental conditions. According to our analysis the degree of confidence which a situation arouses is conditioned by the ease with which it leads into a familiar pattern response, and that this is determined by the completeness of the situation and the absence of contradictory elements. A situation which varies in completeness and the presence of contradictory elements should not be difficult to construct except in so far as it is desirable to have the elements within the situation equally weighted for recognition value and thus devoid of previously established associations. For this purpose nonsense material would seem the best available. Fairly uniform combinations of two nonsense syllables, which would allow for articulation, and which, so far as possible, might be free from associative connections, were accordingly chosen for the experiment. This material is given in Table I below.

Procedure. The experiment was conducted by the usual recognition method with a fall tachistoscope and exposure cards upon which the nonsense 'words' of Table I were printed. The words of Series A were first presented at 10 sec. intervals and later, when presented for recognition, were given with those of Series B and C. After every presentation *S* was asked to indicate by 'yes' or 'no' whether the 'word' was recognized and to note the degree of confidence of his judgment according to the following scale: 0 (don't know); 1 (think so); 2 (fairly certain); and 3 (quite certain). The 'yes' judgments were marked plus and the 'no' judgments minus.

Table I

Nonsense words used in the experiment: Series A, the test material; Series B and Series C, the confusion materials with which Series A was mixed when presented for recognition. The letters in italics are those changed. The numbers in parentheses show the order in which the syllables were presented in the recognition test.

No.	Series A		Series B		Series C	
1	GATOR	(13)	GATO*N*	(9)	GAT*EN*	(6)
2	MAREN	(16)	MARE*R*	(12)	MA*RUR*	(8)
3	DULEN	(22)	*B*ULEN	(19)	*SA*LEN	(17)
4	KUNEL	(29)	*S*UNEL	(27)	*GA*NEL	(26)
5	AUMON	(30)	AU*L*ON	(28)	A*NL*ON	(23)
6	FABAS	(25)	FA*L*AS	(20)	F*EL*AS	(18)
7	SETIR	(24)	S*A*TIR	(15)	*BA*TIR	(10)
8	BIDAR	(21)	B*E*DAR	(11)	*KE*DAR	(4)
9	PAMOR	(14)	PAM*I*R	(7)	PAM*IN*	(2)
10	CELOM	(5)	CEL*U*M	(3)	CEL*UD*	(1)

The *S*s were divided into 4 groups of 16, 34, 14, and 35 respectively. Groups I and II were given Series A *twice* before the final presentation with Series B and C. The sequence of the syllables in the recognition experiments is indicated by the numbers in parentheses. Groups III and IV were presented with Series A *once* only and then with the Series B and C in the *reverse* order of the numerals. The interval elapsing between the original and final presentations was no longer than necessary for the rearrangement of the exposure cards.

It will be seen that Series B duplicates Series A except that *one* letter has been changed in every 'word'; and that in Series C *two* letters have been changed. This 'duplication' was introduced in order to see whether there was any relation between degree of resemblance (or the degree of interference as the case might be) and degree of confidence, as well as to see whether a tendency to make false responses depends in any way on the number of similar elements. The control with respect to the order of presentation proved necessary as preliminary experiments had shown that the nonsense words of Series A would be recognized more readily and with greater confidence if they preceded the 'words' of the other series to which they were similar.

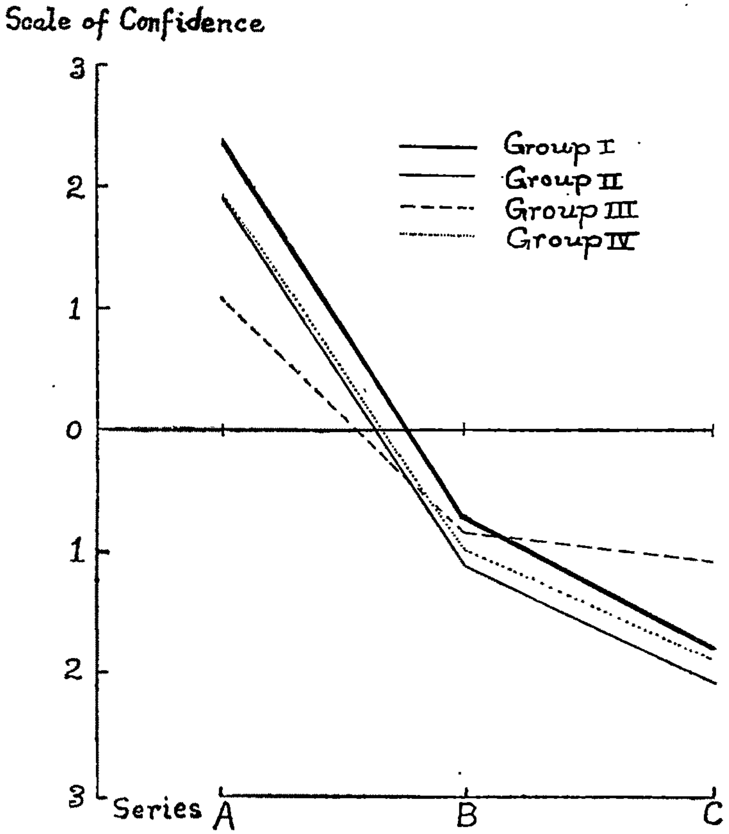

FIGURE I

Average ratings on the scale of confidence for the nonsense material of Series A, B and C by Groups I, II, III, and IV. The scale above and below the mid-line indicates degrees of confidence of the 'yes' and 'no' judgments respectively

RESULTS

Analysis. Figure I shows that two presentations were of little more value for recognition than one since there is no significant difference between the curves of the different groups. This brings out strikingly the importance of the order in the recognition series since the groups of *S*s which were given two presentations were given the recognition series in the order of the numerals, thus allowing the 'words' of Series B and C always to precede the 'words' to which they were similar.

Table II gives the average ratings of the 4 groups of *S*s for confidence and the number of false responses. A response was considered false if it was 'yes' to any 'word' of Series B or C, or 'no' to any 'word' of Series A. Figures I and II present these results graphically. As shown by these tables and curves, the confidence ratings for Series A and Series C were higher than for Series B, and the number of false responses occurred with greatest frequency for the Series B.

TABLE II

Average ratings for confidence and the average number of false responses made by four groups of 16, 34, 14, and 35 *S*s respectively. Groups I and II had two exposures of the presentation series (Series A). The recognition series for these two groups followed the order of the numerals (*cf.* Table I). Groups III and IV had only one exposure of the presentation series, while the recognition series followed the reverse order of the numerals.

*O*s	Series A			Series B			Series C		
	Ave.	S.D.	False Rep.	Ave.	S.D.	False Rep.	Ave.	S.D.	False Rep.
Group I (16 *S*s)	2.3	1.3	1.1	−0.7	2.3	5.0	−1.8	1.6	1.7
Group II (34 *S*s)	1.9	1.5	4.8	−1.2	2.1	8.5	−2.1	1.5	4.0
Group III (14 *S*s)	1.1	1.5	3.7	−0.8	2.4	5.4	−1.0	1.9	4.4
Group IV (35 *S*s)	1.9	1.7	5.8	−1.1	2.4	10.3	−1.9	1.8	6.2

Interpretation. (A) *The Reaction Hypothesis.* The degree of confidence is determined by the ease of redintegration,[4] that is, by the ease with which a present situation leads over into a familiar pattern response.

The results of the experiment confirm the analysis of the illustration used in the introduction of this paper. According to this analysis, confidence increased as the figure approached and this increase was due (1) to the gradual appearance of elements which combined to form perceptual reactions of a familiar sort, and (2) to the absence of contradictory and unfamiliar elements. It may be objected that in this experiment the range of elements within each situation (a nonsense syllable combination) was too limited. There can be no doubt as to the desirability of experimentation upon a wider range of material, or of the greater finality and conclusiveness of a more extensive investigation. It might be well to complicate the situation by introducing a variety of elements—color, form and other factors which would make for differences in quality and intensity—thus approximating ordinary life situations. The difficulty of this would be that it introduces unequally weighted elements and tends to obscure the processes incident to recognition and confidence. Our data, however, permit us to say that at least

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[4] The term 'redintegration' is used in the same sense as by Hollingworth (*cf. The psychology of functional neuroses*, 1920, 12-35; also, Meaning and the psychophysical continuum, *J. of Philos.*, 20, 1923, 436.), as referring to that type of process in which a part of a complex stimulus provokes the complete reaction that was previously made to the complex stimulus as a whole.

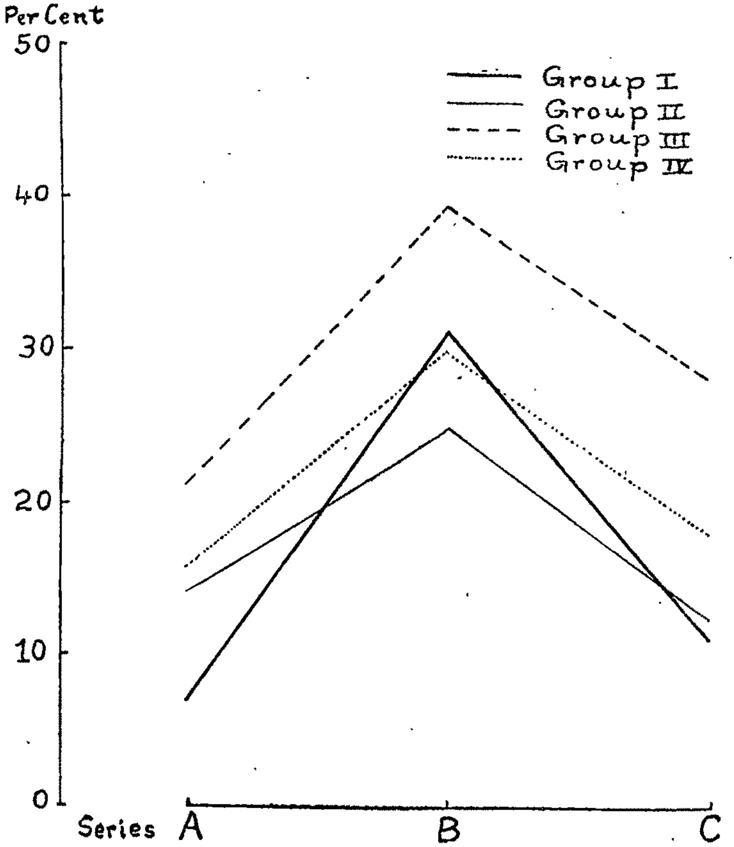

FIGURE II

Percent of false responses made by the *S*s of Groups I, II, III and IV in the recognition and confidence test employing the nonsense material of Series A, B and C.

within the limits with which we have worked the principle we have set forth holds. The greater similarity between Series B and Series A occasions frequent false responses in the case of the former—much more frequent at least than is the case for Series C which is less similar and contains more contradictory elements (see Figure II). This fact is paralleled by Series B taking a position intermediate to Series A and Series C in the average ratings for confidence (see Figure I). Thus the number of false responses and the confidence ratings both indicate that Series B tends to redintegrate the pattern response of the 'word' in Series A to which it is similar, and that this tendency is great enough to be barely offset by the presence of the contradictory element. The degree of similarity with Series A being greatly reduced for Series C and the contradictory elements increased, the tendency toward an affirmed negative reaction is increased accordingly.

There is evidence, then, that not only is recognition attended by a given degree of confidence, but that this degree is dependent upon the degree of similarity which a present situation holds to some past situation, or the ease with which a present stimulus leads into a familiar pattern response. But it may not be clear in what sense we are dealing with a *reaction* mechanism.

Chiefly due to the behavioristic account, certain misconceptions are current as to what we are to understand by a reaction. According to the behaviorists, the term is applicable to visually observable movements and to the operation of such mechanisms as will inscribe themselves on a smoked drum. Such a position reveals the extreme sensuous bias with which this group of investigators are obsessed for facts taken in visually —a position which seems least excusable in psychologists. Psychologically, an individual's reaction in many cases of overt movements must refer to a process for which the outward manifestations are but symbolic indications. If I am interrogated as to the number of angles in a triangle and I make the familiar response, the 'stimulus,' or better, the antecedent event taking the form of a question, initiates the action of a neural pattern on the cortical level and the conscious states experienced with it. This is the significant reaction and precedes the overt behavior by which the answer is made manifest; but whether the overt reaction is a speech reaction, or the significant movement of raising three fingers, or writing the figure three, is not important so long as it takes some conventional and well known form. One cannot say that the latter are not significant, but which ever one is resorted to is an 'accident' as compared with the reaction preceding it and which is common to them all.

In this sense perception, recall, recognition and other aspects of the memory phenomena, as well as ideational processes in their various forms, are reactions and point to characteristics of reactions as much as those which result in gross physical and outward manifestations.

According to our analysis recognition and the feeling of familiarity are characteristics of the states of consciousness arising from the re-arousal of previously active neural patterns and confidence is great or small according to the ease with which the situation redintegrates an earlier response—this ease in turn being determined by the number of familiar elements and the frequency and recency with which one has reacted to them. Briefly stated, recognition consciousness is a characteristic of reactions that are repetitions of former ones and attended by a degree of confidence proportionate to the ease of redintegration or the part-whole efficacy of the stimulus.

There are certain additional facts which may be summoned to the support of this account. The introspective observations of the subjects employed in the experiment furnish repeated instances of 'words' being recognized because they suggested some idea or associated idea which also occurred in the original presentation. The same report is made by Strong in his work on recognition.[5] Some of his *S*s were successful in their attempt to improve their record by allowing every item of the series to suggest some natural association. Müller reports similar observations in experiments dealing with recall and assurance of its correctness. When a nonsense syllable brought with it a word which it suggested in the process of learning this added to the *S*'s belief in its correctness.

From such evidence as well as from the observations of daily experience it appears that the cases in which recognition is most irresistible are cases in which an object recalls vividly and with fullness of detail the circumstances under which it was previously experienced.

Such instances give further emphasis to our statement of the mechanism of confidence and recognition: that an object is recognized because it reinstates previous reactions, perceptual or otherwise, and that the sense of having been seen before comes through the passage of a neural current through previously used brain paths or synapses.

(B) *Retroactive Effects upon Recognition and Confidence.* The observation that recognition and confidence for Series A was greater if it preceded Series B to which it is similar, is a fact which seems most readily explained as due to retroactive effects.

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[5] E. K. Strong, The effect of length of series upon recognition memory, *Psychol. Rev.*, 19, 1912, 447-462; The effect of time-interval upon recognition memory, *ibid.*, 20, 1913, 339 ff.

Apparently, interposing Series B had the effect of interfering with the connections and the neural set formed in the first presentation of Series A.

This interpretation does not accord with the findings of Müller who maintains that the difference between recall and recognition observed in the greater efficacy of the latter, is to be accounted for by the absence of retroactive inhibition in the recognition process.[6] This conclusion is based on an experiment in which in one case the presentation was followed by strenuous mental work and in another by a rest period. There was no difference in these two cases in the amount recognized. It is likely that Müller and his associates in this experiment were working too far above the threshold or even within the recognition span. However this may be, there can hardly be any doubt as to the presence of retroactive effects in recognition, or that the basis of recognition and recall, contrary to Müller's assertion, is different in any important respects. The fact that recognition is less efficient when a large number of objects is presented than when a smaller number, as well as the fact that a lapse of time between presentations makes a difference, should be sufficient to demonstrate retroactive effects.

Such effects are not peculiar to the type of memory phenomena usually considered under the head of recognition and recall. There is no reason why they could not be accounted for by *interference effects* of the same sort as occur in the card-sorting experiments when there is a change from one arrangement to another, or in the experiments of Fernberger on the influence of physical work on the formation of judgments in the weight lifting experiment,[7] or in those of Angell on similar phenomena in the field of audition.[8] In each case we are doubtless dealing with associative connections or neural patterns basic to certain forms of reactions which tend to disintegrate with the passage of time and the formation of new connections. To postulate basic distinctions between these processes and their interference effects, or to differentiate them as 'more physical' or 'more mental,' although in one case a portion of the gross musculature is involved and in the other it is not, appears as an unwarranted assumption.

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[6] Cited by Hillingworth, *op. cit.*, 532 f.

[7] S. W. Fernberger, The influence of mental and physical work on the formation of judgments in the lifted weight experiment, *J. Exp. Psychol.*, 22, 1916, 508-533.

[8] Frank Angell, Experiments on the discrimination of clangs for different intervals of time, this JOURNAL, 11, 1899, 67-79; also, Discrimination of clangs for different intervals of time, *ibid.*, 12, 1900, 58-79.

SUMMARY

The degree of similarity which a present situation has with a previous situation, and therefore, the degree of similarity which present perceptual and associative reactions have with previous ones, determines the degree of confidence. The ability of a present event to arouse reactions which carry with them the sense of having been experienced before, the ease with which present stimuli redintegrate a familiar pattern response, and the combined effect of elementary reactions afforded by a situation or the nature of the perceptual reactions to them as a group, are the determining factors or criteria of confidence.

THE MECHANISM OF CONSCIOUSNESS: THE PERCEPT ARC[1]

By F. Russell Bichowsky, National Research Council, Washington, D. C.

In a preceding paper an analysis was made of the mechanism of the phases of consciousness that intervene between the external stimulus and the sudden appearance of perception. In this paper the preceding analysis will be extended to cover those phases of the conscious process that involve perception.

It will be convenient to state the general conclusion at the outset, namely, that the direct relations between perception and other conscious content may be represented by assuming an arc, the percept arc, which stands in the same general relation to the pre-sensation arc, as that arc, according to the Hughlings Jackson scheme, stands to the next (unconscious) arc below it. Figure 1 graphically represents this fact, heavy parts standing for the new arc and the lighter parts for the pre-sensation arc. The motor half of the assumed percept arc is only outlined as the facts are not at hand to complete it. Moreover this paper does not concern itself with this part of the arc.

For physiological, anatomical, and histological reasons which are familiar, the percept centers will be located in the so-called projection area, *i.e.* for cutaneous sensations in the area posterior to the Rolandic fissure (Brodman's areas 4, 3, 2), for hearing, inferior to the fissure of Sylvester (Brodman's areas 41, 42), and for sight, around the Calcarine fissure (Brodman's area 17).[2]

The first of these areas has received the most careful clinical study. For lesions in this area "the change in sensation is not shown as a loss to touch, pain, heat or cold, but as inability to recognize special relations, relative intensity, or similarity and difference in various test objects."[3] This is a rather large list of symptoms, the last two of which may be due not directly to the loss of the sensory center but to the destruction of the path from that center to the next higher center—that of images. In any event, the most striking effect of an injury to the *gyrus posterior centralis* is the loss of power to fit the pre-sensations into a standard frame of reference. Thus, in cortical injuries of this type, the loss of thermal discrimination falls on only that part of the scale where "thermal recognition habitually depends on some previously existing standard," while in the case of tactile sensibility, the patient may be able to feel the stimulus, but he can not relate it accurately to his space or to his standard of intensity. Thresholds for cutaneous sensations are, therefore, difficult to obtain in cases of cortical injury in this area.

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[1] This is the second of a series of three articles on the 'Mechanism of Consciousness.' The first on 'Pre-Sensation' appeared in the October 1925 number of the Journal (36, 1925, 588-596).

[2] K. Brodman, *Vergleichende Lokalizationlehre der Grosshirnrinde*, 1909.

[3] H. Head, *Studies in Neurology*, II, 1920, 805.

All of this agrees with the much more extensive picture of the function and mechanism of this arc as deduced from introspection. The most satisfactory treatment of perception is found in the writings of the *Gestalt* school, beginning with Wertheimer.[4] According to certain of these writers, the perceived object is projected on a ground from which it is distinct. This is what gives it 'figure.' To perceive an object is, therefore, to relate it to a frame, which is ordinarily of the same modality as that of the perceived object. Thus when we see a faint light on a dark background we perceive the light set off from a residual visual phenomenon, *e.g.* blackness. Again, a sudden sound is perceived as contrasted to, and set off by 'auditory silence,' *e.g.* the ordinary level of faint noises usually present.

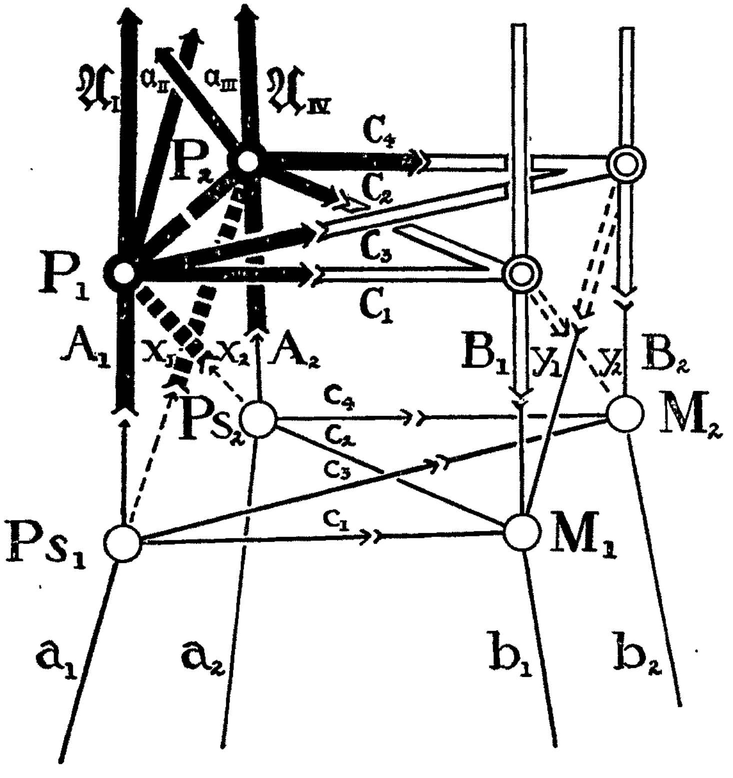

FIGURE 1

The mechanism of the pre-sensation and percept level of consciousness.

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[4] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161.

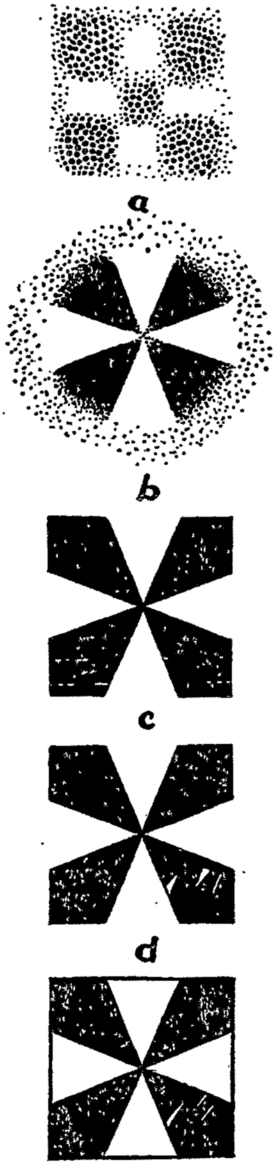

FIGURE 2

Let us investigate more in detail the nature of the 'ground,' which has the following introspective characteristics; it is non-focal, it possesses modality, it possesses no details, it is not spatially extended by itself but acts as a field, it has no outer boundaries, it is unified and it is disposed to fuse. The following are relevant observations.

"The stimulus took me unawares. I was saying to myself in voco-kinaesthetic terms: 'Statistical mechanics plus the quantum conditions ought to allow the calculation of the hydrogen molecule.' I was not prepared for the stimulus. There was dim awareness of other strain and discomfort (in the neighborhood of the eyes?) which was not part of the general voco-kinaesthetic strain commonly present in voco-kinaesthetic thinking. (It may have been due to contact with the 'blinders' on the instrument.) At the stimulus I was saying 'without knowing orbit.' The first I knew of the stimulus was hearing the faint s-s sound of the machinery. This seemed to be part of the 'it' sound of 'orbit,' giving an incongruous effect plus shock, *i.e.* kinaesthesis of quickly raised diaphragm, of movement of intercostal muscles, dry, cold feeling in the back of the mouth as due to a quick influx of air, kinaesthesis of raising eye-brows and focusing eyes. Then awareness all at once of gray visual 'feel,' not recognized as that, *plus* distinct activity of orienting the eyes. The strain activity around the eyes continued. The black-and-white rushed forward into focus, becoming localized in reference to the gray field in the center. While rushing forward the black-and-white were just feelings without figure. There were kinaesthesia and nausea, the details of which I can not remember. The instant the movement stopped the 'figure' was as presented in Fig. 2a. I had a voco-kinaesthetic image 'check' as if to say 'checkers.' This seemed to continue the previous voco-kinaesthetic activity. The image never really developed but seemed to be blocked off gradually by the figure which was now (Fig. 2b), changing rapidly into a cross on a white background (Fig. 2c). This white background had no border and the gray feel was not distinctly present, if at all. I became aware of the defects in the lower right arm (Fig. 2d) first as a check in the movement of the eye, then as a textural defect in the black arm; *i.e.* the cross looked as if drawn with a pen. The defects did not seem to be white spots; nor did the white seem at a different focal distance from the black as did the white of the background. There were ocular strains, ending with kinaesthesis of winking. The gray sensation during the wink closed in, and at the end of the wink I was aware of the figure as four white wedges on black (at a different distance) and the background (Fig 2e). The black boundary lines were now part of the background, while previously they had belonged to the gray haze. The instant I attended to the background, however, the whole figure became a pattern of a black square and the gray background."

These characteristics of the background seem to be identica with those of pre-sensation, and in fact, it seems impossible introspectively to distinguish background and any pre-sensation of the same modality which is present at the same time. The two insist on fusing. It is, therefore, not unreasonable to consider the background phase of perceptual activity as activity of the underlying pre-sensation arc on which the activity of the percept arc is projected; though this must be considered as a tentative hypothesis only and one not absolutely necessary to the general theory.

The most cogent argument against this simple view is that apparently in cases of synaesthesia, in which, according to this theory, percept activity is stimulated by activity of pre-sensation arcs of a different modality, the background to the percept is, apparently, of the same modality as the percept, not the pre-sensation. If this is true, it requires that the modality of the percept activity determine the modality of the ground; how this can be done is not exactly known.

Confining our attention for the time to this underlying ground, it will be seen that the assumption which makes the character of the ground depend on induced or direct activity in the underlying pre-sensation arcs accounts for the unified nature of the background experience, all pre-sensation activity having a unitary quality (due to prevalence of fusion). It also accounts for its experience-filling character, including its lack of outer boundary. The background even of the visual modality having no outer edge, limit, or place, where the visual field becomes non-visual. Of course, color and light intensity of the visual field fade off at the edge of the field as defined by the effective limit of the retina; but always at the outer edge—look as sidewise as one will—there is a background, sometimes black, but a visual black, and identical with a black background which can be seen objectively. This lack of an outer edge to the percept background is what would be expected logically from the theory that boundary to any experience depends on the co-existence of activity of a lower level. Since the pre-sensation arcs are apparently the lowest level arcs whose activity can be analyzed by introspection, it is, therefore, necessary that pre-sensational experience should be unbounded.

Putting this same argument in a form which will appeal to the more epistemologically minded reader, it is a reasonable assumption that consciousness includes only conscious events. If the simplest ultimate unit or atom of consciousness is the pre-sensation and all other conscious events can be analyzed into them; *i.e.* if it is impossible to be conscious of any non-pre-sensation, something not analyzable into pre-sensations and their compounds, it is impossible to be conscious of a non-sensational field which would serve as a limit to pre-sensations.

Internal limits, *i.e.* structure of percept background, seem at first thought common enough; but experiments with certain figures appear to show, though details can not be given, that consciousness of the detail in a background is perceptual. Whenever the observer is conscious of details in a background a new background appears in consciousness on which the details of the old background are projected. At the same time, the old figure becomes lost as such, becoming part of the new pattern, *i.e.* old figure—detail in old background, all being projected on the new background. It may be that something of this sort is responsible for the apparent lack of background of differing modality in cases of synaesthesia.

We turn now to figure-consciousness. The general theory has already been indicated. Figure-consciousness is assumed to be due to the activity of a higher arc superimposed on that of a lower. This statement is, however, hardly more than a description of the most obvious introspective characteristics of figure; namely, the fact that it is projected, is limited, but at the same time, is a spontaneous and a novel activity.

Just what are the precise conditions which lead to the stimulation of the higher arc is not entirely known. Possibly one condition is the nature of the previous activity in the pre-sensation level; *e.g.* the existence of a 'background' of the proper modality. At least, often when the observer is aware of a new stimulus of differing modality, the first content to appear is the pre-sensation of the new modality, then the figure. However, in my own case, I have not been able to consider this as a universal rule.

A more nearly universal condition is the co-existence of kinaesthetic pre-sensation or sensation which often seems to touch off the activity of the higher arcs. For instance, this seems to be the factor determining whether, in cases of binocular rivalry, an image from one eye or the other will dominate. The dominating impulse in my experience always being accompanied by kinaesthetic pre-sensation. It would seem, therefore, that when a group of impulses comes into the pre-sensation level they stand a greater chance of acting as stimuli for the percept arc when accompanied by kinaesthetic pre-sensation than when they come alone. This may be called the trigger-action of the kinaesthetic pre-sensation. It is a very prominent introspective fact.

However, it is hardly likely that the kinaesthetic pre-sensation would be the only 'trigger' pre-sensation and analyses have shown that, in reality, the action of kinaesthesis is not strictly that of a trigger; in fact, it usually shows its presence in the resultant percept activity—just as did the group to which it acted as an apparent trigger. Thus, a more accurate statement is that groups including kinaesthetic activity are more likely to act collectively or partially as stimuli for percept activity than more passive groups of pre-sensations. When put this way, however, it is seen that coördinate kinaesthetic activity can hardly be the sole condition for perception, for cases are known where other pre-sensations, not of the kinaesthetic type, act as 'triggers' both in the old and in the revised sense. Moreover, there is nothing constant about this behavior. On one occasion a given pre-sensation will act as a trigger, not itself appearing as a percept; on another it will act as part of a stimulating group and a third will fuse into the background.

The most promising suggestion is that the important factor controlling the appearance of percept activity is not the presence of some specific kind of excitant, 'trigger' or otherwise, or even the presence of previous activity of the lower level; but is the presence of a certain state of activity of the higher level. This is what would be expected from the general fact that the state of the highest arc involved is the controlling factor in all activity below it. From this point of view it would be expected that impulses from a lower level might break through the upper level if intense enough, no matter what the previous activity of that level might be, but that normally they would break through without loss or disarrangement only if the state of the synapses between pre-sensation and percept arcs were favorable; *i.e.* if passage of this gap was not inhibited by out-of-phase activity of the first percept neurone. This could only be if the previous activity of the percept arc bore a particular relation to that of the incoming sensational activity; that is to say, if the previous activity of the percept arc was 'favorable' for the reception of a particular grouping of impulses from the pre-sensation level.

This view accounts for one of the most marked features of figure consciousness, its dependence, both in character and occurrence on the previous activity of the centers involved which must be 'consistent' with it. One can hardly help being impressed on reading the elaborate introspection, as published by Wheeler and others, by| the 'logical' nature of the process of perception. One event follows another in consciousness, as the events in a play, each with its 'logical' cause, the whole account having what may be called 'plot.'

The percept arc may be looked at, therefore, not as an automatic mechanism for converting impulse into motor activity, as in the uncontrolled pre-sensation arc, but as a more or less plastic mechanism where the incoming impulse tends, other things being equal, to move parts whose friction has been reduced by previous use, but where the impulses, if suitably related among themselves and to the previous state of the mechanism, will cause new combinations of behavior. We may therefore consider the percept-arc as a machine for adapting the behavior of impulses in accordance with its behavior relations to the previous state of the machine. It is therefore the nature of previous experience, both immediate and fairly remote, which controls what groups of impulses stimulate percepts. This makes percept activity an individual matter depending on such things as training and previous behavior. More fundamentally it makes percept activity depend on the condition that the incoming group possesses a relation to the previous state of the percept arc which will serve for the well-being of the animal in the phylogenetic sense.

For this reason it would not be safe to expect a mechanistic explanation to be able to fix exactly what must be the relation between activities so that a given percept activity would ensue. The best that can be hoped is to describe some of the main features and limitations of percept activity in a more general way.

Of these features the possession of limit, position, extent and shape of form seem the most characteristic. The first thing to be noted is that all of these features are relative, either to the field or to the parts of the percept. This holds for the most careful introspective analysis, it being impossible to find any new content corresponding to these factors. All that is found in a figure is pre-sensation plus the additional property of not fusing with other pre-sensation, as it would if it were activity of the pre-sensation arc alone and possessing relations to other pre-sensation activity. In other words, a figure on its background (and, of course, a tone on its background) is not to introspection an essentially different sort of thing from a pre-sensation, excepting insofar as it does not fuse with other percepts or pre-sensations and has conscious relations to these other activities. A perception, therefore, may be considered as in essence the same type of activity as pre-sensation but repeated on a different level and without the tendency to fuse spontaneously to give a single diffuse activity which characterizes the uncontrolled activity of lower levels. Its most pronounced characteristic is its dependence for character and existence on the possession of certain relations to the coördinate activity and state of its own level and to that of lower levels.

That this is the case, and that perception involves consciousness of relation; *i.e.* that perception is percept-arc activity in relation to its parts and to the activity of its ground, can be detected by direct introspection of the best quality and from even those of second quality by noting the transitional, pseudo-independent and universal characteristics of perceptions as contrasted with pre-sensations. These characteristics, as has been shown,[5] mark relational activity within consciousness. Even without minute introspection, the fact that these special qualities of perception are called position, shape and extent is an indication of their relational nature. In short, perception is a primitive form of judgment and must be represented by a sentence; *e.g.*, "This activity has a certain relation to the ground." The percept is, therefore, not the activity of the percept-arc alone or the activity of the ground but the two in a definite relation—just as a sentence is not the subject or object alone, but subject so related to the object.

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[5] F. R. Bichowsky, this JOURNAL, 34, 1923, 231.

Now it is realized that this position does violence to the ordinary conception of the atomistic psychologist that all there is in consciousness is sensations. If he would accept the evidence previously given, he would see that, though it is true that relations by themselves have no objective existence (while pre-sensation apparently does possess existence apart from relations), it by no means follows that relations can safely be neglected, even in accounting for the objective character of the related nexus; for it is a matter of logic and common sense that groups largely depend, in their objective characteristics, upon the relations between their parts. Though there may have been a time for a purely analytical psychology, which merely described the atoms of which the conscious state was composed, such a time has long since passed.

The most interesting empirical reason for the point of view which considers the relation between conscious elements important, comes in the detailed study of the relations involved in perception from the dual point of view of introspection and physiology. It turns out that the relations found by introspection are almost, if not entirely, identical with the relations between the underlying physiological activity as far as these can be discovered.

To introspection the relation of extension seems to be of the two following types: (1) 'Voluminous' extension. Consciousness of extension of this type is independent of consciousness of boundary, shape, or size. It is difficult introspectively to distinguish the 'voluminous' percept from the pre-sensation which also has a very similar voluminous feel; but if the quantity of the stimulus is changed, the pre-sensation is normally modified in intensity, while the percept activity, which is very difficult to attend to, merely changes in 'voluminousness.' It feels bigger, as if more units were stimulated, which, of course, is an adequate description of the probable physiological fact. (2) Metric extension, involving also consciousness of shape and position. Such percepts of size involve the consciousness of the relation between parts and between parts and field.

Consciousness of location seems to be of two types: (1) Location with reference to the field. In my own case, consciousness of location of this type always involves, besides the visual background, kinaesthetic strain of the eye muscles. The system of coördinates used by me in location is, in fact, not a visual set, but is the magnitude of kinaesthetic strains. Without kinaesthetics of this type I am unable to place an object in either two or three dimensional visual space. This means physiologically a constancy of relation between the strain in the eye-muscles and visual stimulation of a particular part of the percept arc. It is this strain in this relation to visual activity that I find in cases of localization. (2) A second type of localization is also possible; *i.e.* relative to some other part of the object, but this can best be discussed in connection with perception of shape.

Perception of shape seems to be quite independent of perception of location. In the case of vision the mechanism is the same; *i.e.* the consciousness of the amount of eye-strain in moving from the one point to the other. The difference is that the standard of muscular strain is not the fixed tonus of the muscles, but the particular state of strain the eye-muscles were just in. Perception of motion has not been satisfactorily covered in this investigation. What seems to be the case, though this is hardly more than a suggestion, is that perception of motion is shape-perception inverted; that is to say that in perceiving motion instead of being conscious of strain due to real motion of the eye, as in case of consciousness of shape, the observer has a succession of images without coördinated eye-movement, and that, because of the habitual connection of movement of image with kinaesthesis, kinaesthetic perception is stimulated. The motion is thus a kinaesthetic, not a visual percept.

Of course, the information given above concerning the nature of the relations needed to account for the facts of location, shape, size and motion, is applicable to only certain of the percept relations, and is, moreover, none too firmly established. However, the assumption that the relational elements in perception do not require any new type of introspective 'substance,' but just are the relations which physiologically exist between the various activities of the arcs involved, is so attractive that it may be tentatively assumed, particularly as it is but a natural extension of the ordinary view that the psychological picture of things must be consistent with the underlying physiological mechanism and because there seems to be no other explanation of the fact that relations among perceptions do not appear to involve new conscious elements.

THE TACTUAL PERCEPTION OF FORM

By M. J. Zigler and Kathryn M. Northup, Wellesley College

This study endeavors in a somewhat preliminary and schematic way to throw light upon three related problems: (1) the degree of accuracy with which observers can render judgments as to the form, or shape, of certain common geometrical figures applied to the skin with vision excluded; (2) the psychological processes involved in the perception of tactual form and in particular the more salient features by which the forms of these figures are primarily apprehended; and (3) the rough determination of that magnitude of stimulus around which the threshold for the perception of these forms lies.

We used 5 straight-sided plane figures as stimuli: square, equilateral triangle, right-angled triangle, diamond, and hexagon. In our initial work, these were cut from cardboard and mounted upon a wooden handle which was convenient for manual application.[1] In every one of these figures the main dimension (the sides of the square and diamond, the altitudes of the triangles and the separation of the parallel sides of the hexagon) measured 20 mm. In subsequent work, and in particular for the performance of the third task, we used a series of 5 stimuli for every one of the figures named above. The main dimensions of the 5 members of every series were 5, 10, 15, 20 and 25 mm. These were cut from hard rubber. The threaded end of a brass handle could be screwed into a hole in the mid-point of every one of these stimuli. In order to insure a fair uniformity of intensity of application, the central rod of the handle passed through and worked against a small spiral spring, which was enclosed in the housing of the handle. A small slot running lengthwise in the wall of the handle-housing allowed an indicator, which was attached to the central rod, to protrude so that *E* could apply all stimuli at a uniform and determinate intensity. The intensity of application used was roughly 500 grm.

The *O*s include 3 undergraduates in advanced laboratory classes, Miriam Arrowsmith (A), Helen Land (L), Kathryn Ward (W) and the senior author of this study (Z). The stimulus was applied to the volar surface of the left forearm at a point about midway between the wrist and elbow. The stimulated member was passive throughout the application and was at all times screened from *O*'s eyes. The instructions read: "After the 'ready' signal a tactual stimulus will be applied to the volar surface of your forearm. You are to observe carefully and report the shape of the tactual impression and all observable criteria upon which your perception of shape is based." None of the *O*s, except Z, had any knowledge of the figures used. The results of our work upon the three questions follow.

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[1] M. Carnes and L. C. Shearer, this Journal, 27, 1916, 417 ff., demonstrated that careful manual application insures practically as good results as mechanical application.

Table I[2]

O	CLASS	Square	Equil. Triangle	Right Triangle	Diamond	Hexagon
A	Accurate	25	25	42	12	
	Confused	33	25	17	12	
	Inaccurate	33	33	29	54	92
	No Shape	8	17	12	21	8
L	Accurate	37	25	29		
	Confused	46	29	29	12	
	Inaccurate	12	42	33	83	100
	No Shape	4	4	8	4	
W	Accurate	21	29	37		
	Confused	21	12	21	8	
	Inaccurate	50	50	37	83	92
	No Shape	8	8	4	8	8
Z	Accurate	29	29	54	50	8
	Confused	8	12	33	12	12
	Inaccurate	46	46	8	29	71
	No Shape	12	12	4	8	8

I. The Accuracy of Tactual Perception of Form

After a short preliminary period of training the 5 cardboard stimuli were presented in haphazard order to every *O* 24 times to ascertain the accuracy and definiteness of tactual perceptions of form under our conditions of experimentation. The results of this work are tabulated below in four classes: (1) Accurate Perception: form was accurately perceived and reported without qualification; (2) Confused Perception: uncertain perception where *O* reported the true form of the stimulus as well as an incorrect form, but was unable to say positively that a mature and determinate form was perceived; (3) Inaccurate Perception: the report of a form different from that which was applied; and (4) No Perception: where the tactual impression failed to take an observable form. Table I shows the percentages of these several kinds of perceptions given by all *O*s and for all figures.

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[2] The percentages in the table are not carried beyond 2 places. Where a fraction exceeds ½ a unit is added, otherwise the fraction is dropped. The dropping of small fractions accounts for the loss of a unit in several columns.

The relative fewness of accurate perceptions is one of the outstanding results. Only one *O* secured as high as 50% accuracy of perceptions, and this was Z, who knew what stimuli were being used, so that he was more likely than the other *Os* to limit his judgments to the 5 figures used. The other *Os* were much more inclined to give a more extended variety of forms, such as: oval, circle, oblong, half circle, straight line. Three *Os* did not mention 'hexagon' throughout the experiment. It was perceived as such only by Z who worked with partial knowledge, and by him very infrequently. It usually gave rise to a perception of square, circle or oblong. The diamond also was difficult for several *Os*. It was reported by them very rarely and was usually confused with the triangles. The right-angled triangle, except in the case of one *O*, secured the highest percent of accurate judgments. The hypotenuse connecting the two acute angles and running diagonally across the forearm constituted a fairly dependable landmark for the accurate perception of this form.

Another item of interest is the fact that, in spite of the instruction to observe the shape of the tactual impression, all *Os* were often unable to perceive any shape at all, not even enough to warrant a guess—and this for practically every stimulus. The vague and indefinite perceptions are not confined to the fourth class in the table. The instructions demanded a report of shape, so that the *O* was inclined to guess even when the impression of form was so vague that only a pressure blur was perceived. These guesses fell into the inaccurate and confused classes; consequently, the perceptions in both of these classes may be taken as instances of indefinite perception of shape.

To assure ourselves that the stimuli were not too small for the facile perception of shape, we next ran through a brief series of experiments in which the main dimension of the stimuli was 35 mm. The outcome of this work, in spite of possible improvement as a result of practice, was, in the case of every *O* and for every stimulus, less (and in most cases markedly less) accurate than in the previous work. Because the larger stimuli were applied necessarily to a convex surface, they did not make so definite an impression along the contour of the stimulated field.

The most frequently confused and inaccurate judgments for the several stimuli were, in order of frequency: *square*—oblong, circle and oval; *equilateral triangle*—other triangles, semicircle, diamond and square; *right-angled triangle*—other triangles, diamond, oblong, square and oval; *diamond*—triangle, oval, oblong and square; *hexagon*—square and circle with about equal frequency.

II. QUALITATIVE ASPECTS OF THE TACTUAL PERCEPTION OF FORM

When form is most clearly given, tactual processes alone carry the meaning of the particular shape. A definite and complete form, however, was the exception rather than the rule in our experiment. The more typical reports of all *O*s show that shape is not usually mediated by tactual processes only. Usually certain salient and discrete features of a figure, *e.g.* three disconnected angles of the equilateral triangle, two acute angles of the diamond, two straight parallel sides of the square, were definitely given in terms of sensory tactual processes but the incorporation of these salient features into a meaningful shape was as a general rule carried in imagery. Visual imagery was most frequently reported but sometimes the completion was made in something like a tactual tied-image, the *O* asserting that parts of the figure outline were not given in sensory tactual processes but in tactual imagery.

The salient features[3] of the several figures most frequently mentioned, given in the order of frequency, were as follows. *Square:* (1) two parallel sides, usually those running transversely to the arm; (2) pressure pattern of vague contour but having dimensions perceptually equal transversely and longitudinally; (3) two or more right angles. *Equilateral triangle:* (1) three acute angles or points; (2) an acute angle with sides of variable length and a straight line on opposite side of figure; (3) two sides and an angle or point; (4) three sides. *Right-angled triangle:* (1) a right-angle or point with hypotenuse running diagonally across the arm on opposite side of field; (2) two acute angles and hypotenuse; (3) three sides with hypotenuse most obtrusive; (4) elongated pressure field pointed at one end and gradually wider at the other. *Diamond:* (1) two acute angles, the sides of which diverge so as to make a wide pressure area with unclear outline in center of field; (2) an elongated field with two points at extremes and a blurred area between; (3) four points or angles with completed or uncompleted sides. *Hexagon:* this was not identified often enough to allow us to speak of a mode by which it was generally apprehended. Most confusions of this figure were with the square and circle. In the several instances when it was reported accurately by the one *O*, a field of equal dimensions longitudinally and transversely but lacking long straight sides or right angles was perceived.

From the foregoing it is patent that acute angles are more potent landmarks for the tactual perception of the form of figures than side-lines, while the side-lines are more likely to be apprehended and to figure in the realization of the form of a figure than obtuse angles, and quite as likely as right angles. Acute angles are perceived much more definitely than right and obtuse angles. Because of increased sensitivity, the parts of the figure applied toward the wrist were generally more clearly apprehended than the parts falling more proximally.

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[3] In an earlier study (this JOURNAL, 31, 1920, 273 ff.) the visually salient features of such figures were pointed out.

As to whether form is mediated primarily by peripheral or by central processes, our results do not shed very conclusive light. It is apparent that central factors are involved, in view of the fact that imagery is employed to complete the form of a figure incompletely given sensorily. But there were a few instances when *O* asserted positively that the form seemed completely given in sensory tactual processes. There were also a number of reports to the effect that the realization of sensory pressure and of a pressure field having definite outline were temporally disjoined. In all such cases, the apprehension of a 'pressure blur' preceded the realization of a pressure having a particular shape. But this temporal disjunction form of report was offset by reports, not nearly so frequent, that the perceptions of pressure and of shape came simultaneously. There were frequent instances, too, when in spite of instruction to observe form, *O* could only report a 'pressure blur,' as though some factor, perhaps central, failed for some reason to appear. The apprehension of pressure was quite generally more definite and unfailing than the perception of pressure having such and such shape. It is conceivable that our filled figures may at times have hindered or delayed the perception of shape, and that outline figures might have enabled us to determine, had time permitted, with more assurance the immediacy and the definiteness of the apprehension of shape.

Another indication of the possibility of central coöperation in the perception of form is the fact that all *O*s showed more or less pronounced daily sets for particular forms. On a given day the *O* would report one form much more accurately than the others and at a later sitting the same *O* manifested a similar pronounced tendency toward another form. On the same day, however, the *O* would also report the favored form for inappropriate figures. The temporary central predisposition seemed to tend both toward the accurate perception of the particular form for which a temporary set was assumed and toward the realization of that form even if a different stimulus was applied.

III. THE APPROXIMATE THRESHOLD OF SIZE FOR THE PERCEPTION OF FORM

The limen of form of several geometrical figures has already been determined by Major.[4] However, several points of difference between his work and our own must be noted, if comparison is to be made. Major stimulated more sensitive parts of the skin surface, employed a method involving knowledge, and used only one of our figures—the equilateral triangle. He expresses the opinion that a procedure without knowledge should give "practically the same" limens as the procedure with knowledge.[5]

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[4] This JOURNAL, 10, 1898, 143-147.

We arranged series of hard rubber stimuli of every one of our 5 figures for the purpose of making our determinations. In every series there were 5 stimuli having main dimensions, taken as given above, of 5, 10, 15, 20 and 25 mm. These were presented in haphazard order, the and *O*s were instructed to name the form of the impression occasioned by the stimulus. Table II embodies the aggregate percentages of accurate, confused, inaccurate, and failure judgments of form given by the 4 *O*s. The results for the hexagon were again as unsatisfactory as in the previous work. It was reported correctly by only one *O* again, and all told in 5 instances only.

TABLE II

		Dimension				
Form*	Judgment	5 mm.	10 mm.	15 mm.	20 mm.	25 mm.
Square	Accurate	0	7	13	25	20
	Confused	0	0	25	33	40
	Inaccurate	0	13	28	30	23
	No Shape	100	80	33	12	17
Equil. Triangle	Accurate	0	10	25	27	22
	Confused	0	8	30	33	35
	Inaccurate	0	20	33	33	40
	No Shape	100	62	12	7	3
Right-Angled Triangle	Accurate	0	8	23	30	22
	Confused	0	12	28	27	35
	Inaccurate	0	18	30	33	33
	No Shape	100	62	18	10	10
Diamond	Accurate	0	3	13	23	25
	Confused	0	3	20	30	20
	Inaccurate	0	8	33	32	35
	No Shape	100	85	33	15	20

*Results for the Hexagon omitted because it was reported by one *O* only (Z), and by him only 5 times.

The stimuli of 5 and 10 mm. quite generally gave rise to the perception of a mere point or a blunt point of shapeless pressure. In the few instances when the forms of the 10 mm. stimulus were correctly reported, the shape was described as very indefinite and the judgment as essentially a guess. The minimal magnitude of stimulus for the passive apprehension of shape in the region stimulated seems to fall between 10 and 15 mm.

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[5] *Idem*, 146.

The optimal dimension seems in general to be 20 mm. The accurate judgments of the 10 mm. stimulus in every case amounted to less than 50%, and those of the 15 mm. stimulus in every case to more than 50% of the accurate judgments of this optimal stimulus magnitude. Hence our conclusion that the liminal magnitude falls somewhere between the 10 mm. stimulus and the 15 mm. stimulus.

CONCLUSIONS

(1) Tactual form is not so definitely and unfailingly apprehended as tactual pressure when vision is excluded and the member kept passive during stimulation. Tactual pressure is invariably perceived, while tactual form is not definitely given in a considerable percentage of cases, and is not apprehended at all in an appreciable number of instances.

(2) The realization of form is minimal in cases of 'mere pressure blur,' where pressure totally devoid of outline or highlands of pressure boundary is apprehended; and maximal where the completed contour of a figure-form is given at all points in terms of the tactual impression. Between these extremes are intermediate degrees of definiteness of form, where salient topographical features of the tactual impression of a form are perceived in isolation (the contour being broken between these tactual highlands) that must be completed either in visual or in tactual imagery or in both to occasion the perception of the particular form.

(3) The stimuli of common geometrical figures must have a main dimension of 12 to 15 mm. to give rise readily to a tactual impression of determinate shape, if stimulation is made on the middle of the forearm. Impressions occasioned by stimuli of smaller main dimension are described as 'blunt points without contour,' 'shapeless pressure,' or 'mere sensory pressure stimulation.' The optimal main dimension for the mid-forearm by our method of stimulation is somewhat larger, about 20 mm.

THE EFFECT OF COLOR ON APPARENT SIZE AND WEIGHT

By Carl J. Warden and Ellen L. Flynn, University of Wisconsin

The present study is a preliminary attempt to determine the effect of color and arrangement on the apparent size and weight of a series of cartons, identical in actual size and shape, and differing only in the factor of color and serial position. The first experiment is concerned with the illusion of size (color-size illusion); the second with the illusion of weight (color-weight illusion).

The cartons used in the study were of cardboard, rectangular in form, measuring 16.5 cm. high, 9 cm. wide and 5.5 cm. deep. Eight of these boxes were used. They were covered respectively with red, yellow, green, blue, purple, white, grey and black Hering papers. The paper was stretched tightly and was so cut that no seams appeared on the sides toward *S*.

Inasmuch as the primary interest in the problem was practical, being connected with the display of merchandise, an attempt was made to duplicate the conditions of retail trade. The cartons were placed 5.5 cm. apart in a glassed light oak case which stood 4.5 ft. from the floor. They were placed directly against the back wall of the case, which was 4 in. from the glassed front, with the wider side (9 cm.) toward *S*.

The first serial arrangement of the cartons was made by chance. The succeeding arrangements, however, were obtained by a systematic rotation of the boxes from left to right so that every one became the initial member of the series. The immediate context of a given box, that is the color of the carton to the left and right, remained constant under all the arrangements except when it was either the initial or final member of the series. The 8 arrangements of the colored boxes were as follows:

Arrangement	Colors
A	Gy, Y, B, W, Bk, G, R, P
B	Y, B, W, Bk, G, R, P, Gy
C	B, W, Bk, G, R, P, Gy, Y
D	W, Bk, G, R, P, Gy, Y, B
E	Bk, G, R, P, Gy, Y, B, W
F	G, R, P, Gy, Y, B, W, Bk
G	R, P, Gy, Y, B, W, Bk, G
H	P, Gy, Y, B, W, Bk, G, R

Everyone of the 8 arrangements was ranked by 6 college students—3 men and 3 women. The final score for a given color is therefore the average of 48 rankings.

Experiment I. The purpose of Experiment I was to determine the extent to which the apparent size of a carton was a function of its color. The order of the boxes was rotated so as to eliminate the effect of specific arrangement.

S was handed a card on which had been typed the names of the colors in the order corresponding to that in which the colored cartons were arranged. He was then instructed to indicate by numbers opposite the colors the relative sizes of the different cartons. The fact that the cartons were identical in size was kept from the *S*s. The experiment was, from their point of view, purely a test of size discrimination.

TABLE I

The Effect of Color on the Apparent Relative Size

Color	ARRANGEMENT								Ave.	S.D.
	A	B	C	D	E	F	G	H		
Red	4.5	4.2	3.9	4.8	5.0	4.5	5.0	3.2	4.4	.47
Yellow	2.7	2.8	4.5	6.0	4.8	4.7	6.9	6.5	4.9	1.33
Green	4.2	4.2	3.7	3.3	5.3	5.8	4.8	3.0	4.3	1.06
Blue	4.5	4.2	4.7	4.8	6.5	5.5	4.3	3.7	4.8	1.21
Purple	6.5	5.3	4.2	3.0	3.0	3.2	3.2	5.3	4.2	1.30
White	2.2	3.0	4.3	5.7	5.8	6.7	6.0	5.7	4.9	1.57
Gray	4.2	6.2	5.7	4.7	3.2	2.5	3.5	6.3	4.5	1.46
Black	6.8	5.5	4.2	4.2	3.2	4.8	4.0	2.0	4.3	1.46

The results of the experiment are presented in Table I. It is evident that the color-size illusion is present to a marked degree under every arrangement. The amount of the illusion, however, varies considerably from one arrangement to another. For example, the rankings in Arrangement A vary from 2.2 for white to 6.8 for black; while in Arrangement C rankings vary only from 3.7 for green to 5.7 for gray. Almost as great a difference in ranking occurs in Arrangements D and H. It is clear, therefore, that the amount and definiteness of the color size illusion is in part a function of the specific arrangement of the colors.

By referring to Table I it will be seen that the apparent size-value (relative) of a given color varies with the serial arrangement. For example, the average rank of yellow varies from 2.7 in Arrangement A to 6.9 in Arrangement G; White, from 2.2 in Arrangement A to 6.7 in Arrangement F; while red, which maintains the most stable rank position of all, varies from 3.2 under Arrangement F to 5.0 under Arrangement E.

When the rank values for every color throughout the 8 series are combined, the differences between the averages so obtained are so slight as to be insignificant. The standard deviations (S. D.) show that these averages have little or no reliability. The size-value of a color seems, therefore, to be dependent upon the specific arrangement.

While apparently a function of the serial arrangement, the color-size illusion is not due primarily to the contrast effect of the adjacent colors, for as will be recalled, the immediate context (preceding and succeeding color) was the same throughout the 8 arrangements except when a given color was the initial or final member of the series. No greater consistency in the relative ranking obtained at the end positions than obtained at the other 6 positions in which the immediate context did not vary Furthermore, no general uniformity appears in the data as to the effect of the end positions on light and dark colors.

TABLE II

The Effect of Color on the Apparent Relative Weight

Color	ARRANGEMENT								Ave.	S.D.
	A	B	C	D	E	F	G	H		
Red	6.2	3.8	3.8	5.7	4.5	5.0	5.2	4.7	4.9	.51
Yellow	3.3	3.3	4.5	4.5	3.7	2.0	2.7	4.3	3.5	.98
Green	5.2	4.7	3.7	4.5	3.8	4.3	3.0	3.2	4.1	.32
Blue	3.8	4.7	6.2	3.2	4.5	4.8	5.3	4.8	4.7	.60
Purple	4.3	6.2	4.2	5.5	5.5	2.0	6.5	4.5	4.8	1.07
White	3.3	2.8	4.3	5.3	4.2	1.7	1.0	2.2	3.1	1.36
Gray	4.7	3.8	6.0	3.2	5.5	5.2	4.3	6.0	4.8	1.13
Black	6.3	6.2	4.2	2.8	5.5	7.0	8.0	6.5	5.8	1.46

The apparent size-value of a color does not under our conditions, depend upon its own intrinsic quality or upon the contrast effect of its immediate context. The illusion must be dependent upon the specific arrangement of the series taken as a whole, that is, upon the *ensemble* of contrast effects produced by the total series. The data offer, however, no basis for an analysis of the specific factors involved.

Experiment II. In Experiment II an attempt was made to determine the extent to which the apparent weight of the carton was a function of its color. The arrangement of the cartons varied as in Experiment I, and the general procedure was also the same.

The results are given in Table II. The color-weight illusion, as will be noted, varies widely from one arrangement to another. For example, the rank values vary from 1 to 8—the greatest possible variation—under arrangement G; while the variation extends only from 3.7 to 6.0 under arrangement C. Similar comparisons can be made from the table. The amount and definiteness of the color-weight illusion is thus in part also a function of the specific arrangement of colors.

The data indicate, however, that the apparent weight-value of a given color is not wholly dependent upon the specific serial arrangement. Most of the colors have a consistent ranking that holds throughout the experiment. The difference between the average rankings show, in most cases, a relatively high degree of reliability. White, gray and black show a fairly consistent tendency, regardless of serial position, to form a series of qualities in which the color-weight illusion increases. The colors yellow, green, blue, purple and red form a similar series, although the rankings of blue, purple and red are not markedly different. It is not possible to determine, from the data gathered to what extent the color-weight illusion may be due to the contrast effect of the immediate context which remained constant in 6 of the 8 arrangements.

Conclusions. The results of these experiments may be summarized as follows:

(1) The color-size illusion, under the present conditions, did not depend on the intrinsic quality of the color nor upon the contrast effect of the immediate context, but, as would appear, upon the specific serial arrangement.

(2) The color-weight illusion was greater than the color-size illusion for this series. It occurs independently of the specific arrangement of the series, and appears to be due in part to the intrinsic qualities of the colors. The factor of immediate contrast on this illusion was not measured.

SOME CHARACTERISTICS OF THE WORK-CURVE WITH SHORT WORKING UNITS

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INTRODUCTION

In 1916 F. M. Phillips described a research on the curve of work in the fundamentals of Arithmetic using 1-min. work-units with a large number of subjects.[1] The results show a surprising superiority for the first minute of the ten-minute sequence. The lowest point in the work-curves occurs in the second minute in eight of the twenty curves. In the remaining curves the lowest point is after the fifth minute. Chapman[2] had used 2-min. work-units with 46 men, the task being to add columns of ten one-place numbers for two 10-min. sequences on each of 5 days. The curve which he constructed from these 460 measures showed a surprising smoothness. Individual variations were largely eliminated and the common characteristics emphasized. The results in his case again showed high initial performance with an abrupt fall for the first four minutes; this fall gradually becoming less abrupt till after 10 minutes when the curve had become almost horizontal.

A common characteristic of these two researches is, then, surprising superiority of the initial performance. The question arises whether the division of the work sequence into units smaller than 2-min. would produce a work-curve with peculiarities not obvious from the larger divisions. G. E. Phillips,[3] who employed one-min. units, showed that some of the characteristics of both sets of the curves mentioned above appeared. The high initial performance still obtained; then there was a marked depression in the second minute of work; a rise during the third, and finally a gradual fall to the horizontal. Phillips used cancellation as a task with four subjects.

We must be quite clear, in work of this nature, as to what factors have been controlled by the conditions of experiment. Good technique is not content with a general exhortation to work as rapidly as possible. Incentives must be used to keep

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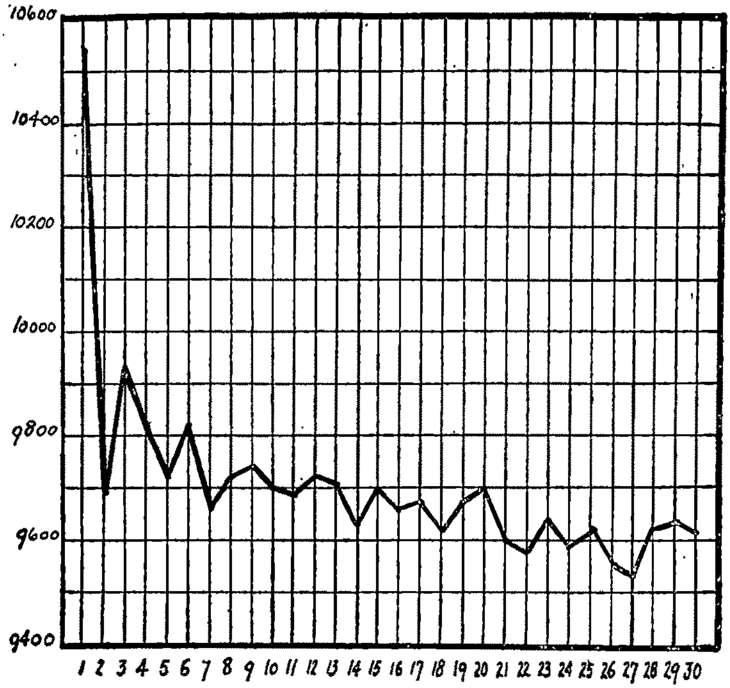
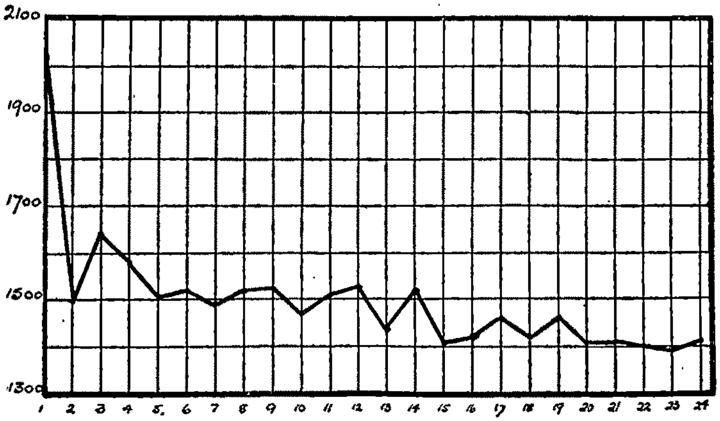
[1] F. M. Phillips, A comparison of the work done in successive minutes of a ten minute practice period in the fundamentals of arithmetic, *J. of Educ. Psych.*, 7, 1916, 271-277.

[2] J. C. Chapman, A study of initial spurt in the case of addition, *ibid.*, 6, 1915, 419-426.

[3] G. E. Phillips, Mental Fatigue, *Rec. of Educ. Soc.*, 1920, Teachers College, Sydney, 1920.



CURVE I
Curve of work for *Cancellation* in ½-min. periods



CURVE II
Curve of work for *Addition* in ½-min. periods

the endeavour as high and as constant as possible, especially in the case of experiments in the schools. We must be quite clear also as to the part played by practice, especially in experiments on fatigue.

G. E. Phillips used monetary rewards as incentives. The practice factor was controlled by a prolonged preliminary sequence. Not until the practice-curve had reached the horizontal, indicating that no further improvement at this stage was probable, was his experiment proper commenced. The readings through forty-five days which constituted his experiment show slight practice effects.

It is worthy of note, therefore, that, besides the general fall in the curve from a high initial performance to a horizontal which is a characteristic of all three researches, we find in the one-min. curve a minimal point at the second minute of work, but in the two-min. curve, no such peculiarity. It may be, as Phillips himself suggests, that the division of work into units larger than one-min. swamps the depression at the second minute.

THE CURVE OF WORK USING HALF-MINUTE WORKING UNITS

In our own experiments an attempt was made to find whether these characteristics were obvious with a 12-min. work sequence divided into half-minute units.

A preliminary practice was first carried out. Cancellation of four letters from a sheet of capitals was given for 15 minutes each day, at the same hour, for 23 days. At the conclusion of this period the curves for all three *S*s had been horizontal for 6 days. Though slight variations were obvious from day to day, the main effects of practice had clearly past.

Incentives were used in the form of rewards for improvement. The first experiment consisted of a continuous addition test of 12-min. duration divided into ½-min. units, on 40 successive day, excepting Saturdays and Sundays.

The total scores by ½-min. for all *S*s are represented in the curves I and II above. The main characteristics are: (a) high initial output; (b) a minimum-point for the second half-minute; and (c) a gradual fall from the third half-minute to the horizontal.

It will be noticed that the minimum-point occurs at the second half-minute not at the second minute, which seems to suggest that it is the second point on the work curve, not essentially the work done during the second minute, which determines this point. This depression for the second unit-performance is clearly marked in all three individual curves.

The degree of reliability that attaches to the difference between two readings can be estimated by the formula (Whipple):

$$\text{P.E.} = 0.6745 \sqrt{\frac{\sigma_1^2 + \sigma_2^2 - 2r\,\sigma_1\,\sigma_2}{n}}$$

As the difference between the mean performances for the second and third points on the curve is 6.3 with P.E. 1.9, the result appears to be not a matter of chance or random error.

The same experiment was carried out using, first, simple addition with three *S*s, and secondly, dictation of numbers at a maximal speed from a Kraepelin sheet, with one *S* under similar conditions, after a preliminary practice period of 20 days. Both average curves, as well as all four individual curves, show the characteristic high initial performance, a minimum at the second ½-min., a subsequent rise, and a gradual fall. Again the evidence tends to show that it is the second work-unit not the second minute which shows the characteristic depression. The characteristic form appears to be independent of the task and common to all subjects. This latter result was arrived at also by G. E. Phillips with 1-min. work-units.

In the cancellation test 7% more work was done in the initial half-minute than in the average of the subsequent three ½-min. periods. For the addition test the percentage was found to be 28%, and for the third test 9%.

The Effect of Fatigue

Another interesting point of comparison between the curves of Chapman and G. E. Phillips is that the effect of fatigue seems to be confined to the first twelve minutes of work. Chapman does not quite reach the horizontal stage; but it is quite clear that, if the curve were prolonged in accordance with its general trend, it would achieve the horizontal at approximately 12 min. We have already pointed out that for these two curves entirely different work was involved.

Referring to our own curve for cancellation, it will be noticed that the fatigue-effect is confined to the first thirteen minutes—the curve becoming horizontal before the 26th half-minute. In the second experiment, using addition, the curve becomes horizontal just after the 12th min. (24th half-min.) of work.

There would appear, then, to be some additional evidence for G. E. Phillips' assertion that the unsatisfactory work-curves of the Kraepelin School, using 5-min. units, are due in part to the failure of recognising that the fatigue-effect is confined to the first three points on the curve.

We might further question whether the total depression from the initial point to this horizontal phase bears any relation to the nature of the task or to the individual. Lehmann[4] found in ergographic records that the depression bears a relation to the rate of working. Where the interpolated rests are short the depression is great; with rests of a certain length there is no depression. In other words, there is no fatigue-effect when there is no depression; the depression is greatest when the fatigue is greatest. Lehmann maintained that the depression is inversely proportional to the length of the rest interpolated between each contraction.

G. E. Phillips found that, in cancellation, the depression varied in the case of his three individual curves. By examining the depression in three different tasks for one *S*, he concludes that there is a probability that the depression is dependent on the nature of the fatiguing work.

If we standardise, for comparison, the depression of a number of points on the curve as a percentage of initial performance, thus, $\% = \frac{d \times 100}{I}$ (where d = depression and I = initial performance) we get the following table.

TABLE I

Cancellation

S	20th	22nd	24th	26th	28th	Mean	P.E.$_M$
1	8.8	9.0	7.5	9.4	9.0	8.8%	.21
2	8.1	7.5	6.8	10.6	9.7	8.6%	.42
3	7.5	10.7	10.1	8.4	8.4	13.0%	1.25

Addition

S	18th	20th	22nd	24th	Mean	P.E.$_M$
1	28.9	25.3	24.1	28.9	26.8%	.72
2	24.4	28.6	36.5	30.0	30.1%	1.47
3	35.7	39.3	34.0	32.1	35.3%	.89

Dictation of Numbers

S	20th	22nd	24th	26th	28th	Mean	P.E.$_M$
1	11.9	13.5	13.5	14.4	14.1	13.5%	.33

The depression of the horizontal phase below the initial is certainly not constant for all our *S*s. Both in addition and in cancellation there is a clear difference between the percentage depressions. Our *S*s are not equally fatigued by working at maximal pressure for the same time at the same task. These figures, then, indicate the *fatiguability* of the *S*s for specific tasks.

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[4] A. Lehmann, *Grundzüge der Psychophysiologie*, 1917.

What is the relation between this fatiguability and the ability of the worker as represented by his initial performance?

TABLE II

	Cancellation		Addition	
S	Initial Perf.	Fatiguability	Initial Perf.	Fatiguability
1	400	8.8	83	26.8
2	320	8.6	63	30.1
3	335	13.0	56	35.3

In these three cases (see Table II) ability in a task seems to be inversely related to the fatiguability of the individual for that task. But no generalisation is possible of course from only three *S*s.

Does the depression depend on the nature of the task?

TABLE III

S	Cancellation	Addition	Dictation (Numbers
1	8.8%	26.8%	13.5%
2	8.6%	30.1%	
3	13.0%	35.3%	

On reference to Table III it will be evident that the depression for addition is over three times the depression for cancellation for the same individual.

This cannot be explained entirely in terms of random variation. Table I implies that the standard deviation and the P.E. for the sequence of measures are indicative of reliable data. Random error is certainly not significant enough to account for the difference between the readings for the three tasks. It appears, then, that the depression varies with the individual and with the task which produces the fatigue.

THE PLACE OF OCULAR MOVEMENTS IN STROBOSCOPIC PERCEPTION

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It has long been known that the apprehension of movement in the visual field may, under certain conditions, rest upon slight involuntary shifts in ocular fixation. Thus the apparent movement of objects experienced after bodily rotation has been attributed, at least in part, to nystagmus; for, with the cessation of the eye-movements, the world no longer spins. Again, purely mechanical manipulation of the eyeball with the finger will give rise to the visual perception of movement. Pressure in this case seems to change the retinal pattern in such a way that certain objects are actually seen as moving although the remainder of the visual field may be at rest. We also know that observers report, in connection with the usual laboratory experiments upon the projection of the after-image, that the after-image moves about in various directions as a result of involuntary eye-movements. And, finally, various illusions of motion are commonly (though not always) referred to slight and unobserved shifts in fixation; *e.g.* autokinetic movements observable with a dim light in a dark field; a type of movement frequently reported in an earlier study of ours,[1] where we used a dim fixation-light in total darkness.

It appears, then, that ocular shifts variously produce the perception of movement. Is this *generally* true? Or are movements of the eyes but one of many ways in which this perception may be induced? What shall we say of the perception of movement under discrete retinal impressions where points on the retina are successively stimulated? Can we here too invoke eye-movements as an explanation? The purpose of this paper is to point out certain facts which were observed in connection with a study of such apparent movement and to show that the *assumption* of slight ocular shifts as a condition of the apprehension of apparent motion introduces a common element of unity by means of which a fairly large number of these facts can be bound together. We do not offer these facts as *proof* that eye-movement is a general condition of these perceptions; but we merely suggest that such an assumption most adequately explains the observations in question.

(1) *Observers report ocular movements.* The clearest evidence of eye-movements under the method of successive stimulation

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[1] This JOURNAL, 37, 1926, 63-115.

such as we used comes from the actual experiences of our observers.[2] *O*s reported clear kinaesthesis from the eyes with the meaning that there was a change in fixation when the lines appeared. They also reported that the eyes seemed to follow the lines up and down, or that the eyes jerked up or down between the appearance of the first and the second object. Others interpreted these experiences by the conjecture that while they saw clear movement it must have been given kinaesthetically.

(2) *Presence of intermediate members.* Every one of a score or more of our *O*s reported, at times, that there were three, four or five successively seen lines in the field or that one line in moving stood out sharply as many times. That is to say, all reported the existence of various members intermediate to the two terminal positions. This sort of report upon the presence of secondary lines is not limited to our own experiments. Some of Dimmick's *O*s[3] also apprehended them; and they are diagrammatically represented in Dimmick's later study upon apparent and real movement.[4] In our study these figures were, as a rule, not simultaneously apprehended. There was an observable sequence, the particular direction depending upon the order of appearance of the stimulus objects. Moreover, these intermediates were generally less intense and not so clean-cut as the end-members. Often they were decidedly fragmentary or broken into halves or thirds. When colored lines were employed as stimulus-objects the saturation and the brightness of the lines tended to be less than in the end-positions.

(3) *Movement of negatively colored objects.* Certain *O*s reported that a bluish-green line or a blue line of low chroma moved across the field. These reports came when we were using the slitted cards with standard mazda bulbs. Under these conditions the lines were actually yellowish in hue. Here, as it seems, is a clear indication of a change in fixation with the appearance of negatively colored after-images. That such negative effects may occur under short exposures we know from Mach's work upon the simultaneous apprehension of colored objects under short periods of illumination.[5]

(4) *Movement beyond the second member.* From time to time *O*s observed one line move to and beyond the position of the second figure and then immediately back to it. They saw a weak line where no line could possibly have been, *i.e.* below or

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[2] For apparatus, method and observers the reader is referred to the article cited, pp. 76-79.

[3] F. L. Dimmick, An experimental study of visual movement and the phi-phenomenon, this JOURNAL, 31, 1920, 317-332.

[4] F. L. Dimmick & C. H. Scahill, Visual perception of movement, *ibid.*, 36, 1925, 416.

[5] E. Mach, *Beiträge zur Analyse der Empfindungen*, 1906, 205-6.

above the position of the second member, depending upon the order of sequence. Most *O*s saw the first line actually overshoot the final position and subsequently withdraw to it before disappearing. Others interpreted the experience differently. It meant that the one line by assuming the position of another forced the latter for a moment from its position. Regardless of this difference in interpretative report, all saw a weaker line that appeared momentarily beyond the position of the second member before moving back and disappearing.

(5) *Dual movement.* The assumption of eye-movements seems also to cover the facts of dual movement. Where the intervals were very short the *O*s sometimes reported that the two lines appeared and moved together, as if cut from a single piece, in one or two directions. If the sequence was horizontal-oblique the movement was upward for a few degrees and then down; the reverse order of presentation gave movements in the opposite directions. The upper and lower limits of each of these movements might be defined by lines which formed two small angular fields filled with light over which the movements occurred. We know that where one line is added to another, without interval, there is movement in the direction of the added member and that, when one member is subtracted from the total figure, movement is in the direction of the remaining line. If we bring two lines almost simultaneously into the field, but with a slight temporal interval, one sees first a movement of both lines in the order of sequence. But if the first disappears a trifle before the second, it is quite possible that the eyes shift again in the direction of this change in the visual field due to the disappearing line and thus condition movement in that direction. We do know that the eyes tend to shift toward any change going on in the visual field. May we not assume, then, that the eyes shift downward and then back under the sequence of appearance and disappearance and that along with this shift go the after-images of the lines in various positions? The apprehension of these lines together with ocular kinaesthesis would then produce the apparent dual movement.

(6) *Spatially dislocated lines.* Another group of facts pointing towards the occurrence of eye-movements at the time of apprehended motion has to do with spatially dislocated members. The position of each line with respect to the other varies from time to time in a puzzling way. The oblique may be seen as a vertical, as a horizontal, or as an oblique, but so reversed in position that it now approaches the right end of the horizontal instead of the left. In addition to these changes in the position of the lines, there are slight variations in the position of the oblique varying through 20-75 degrees. Furthermore, the horizontal may be seen as an oblique. This upward shift of it oc-

curred especially when the oblique was seen around a vertical position. With the parallel-horizontal arrangement of the lines, one member might actually be seen moving as an oblique. The clearest evidence in the general connection of the dislocation of the lines comes, however, from the way in which the true oblique might move to the horizontal or the horizontal might move to the oblique. In either case there was frequently no clean-cut turning movement as around a pivot. The lines seemed to move *directly* up and down. That is to say, the oblique moved as an *oblique* so that it actually bisected the horizontal.

(7) *Momentary disappearance of the first member during movement of second.* When one member was added to the other, *O* occasionally reported that during movement the first line momentarily disappeared only to reappear a fraction of a second later and to stand out clearly with the second member, forming in this manner either an angle or two parallel lines. This same experience has been reported in connection with a problem upon which we are now engaged, *viz.*, the apprehension of visual movement with short delays but without temporal intervals. Our more naïve *O*s sometimes report that the earlier of the two lines, to which the other is added, disappears for a second while the second is moving to its final position. Here again is a clear indication of change in fixation during movement.

(8) *Movement of a single line.* At times a line was actually seen to be *moving* when it appeared in the field; *i.e.* movement was simultaneous with appearance. We find a similar sort of report in Dimmick's study (pp. 325-328). Good visual movement of one member may be followed by the perception of the second member in a stationary position. Movement, then, may actually be apprehended before the second member appears. It seems to be independent of the second member except that it is in the direction of the subsequent position of that subject. It is as if the organism anticipated the order of phenomenal succession and apprehended the first as moving even before the other appeared.

(9) *Phenomenal differences in intensity, brightness and extent of the stimulus objects.* When the two members were given together by turning on both lights within the apparatus and a detailed description called for, *O*s reported that both objects were equal in intensity, brightness and extent. When the lines were successively presented, however, the one member might be apprehended as much more intense or as brighter than the other. *O*s frequently commented upon the striking differences in intensity, brightness and extent of the members under the movement experience. Assuming always a constant fixation, it is extremely difficult to understand why the objects should not manifest a fairly constant degree of similarity even though ex-

position-times (equal for both) were short. But if we do not assume a constant fixation the matter is made clearer. Then these differences lend themselves to an explanation in terms of rapid movements of the eyes; because what is seen at the beginning or during a flickering glance is unlike that observed when the eye is fixed.

These nine groups of facts embrace a large range of observations. We have not attempted any quantitative survey of the total number of reports. Our primary interest has been to call attention to this striking variety of facts and to raise the question whether they may not be brought together and made more intelligible through the *assumption* of eye-movements.

It has been asserted that eye-movements cannot be significant for the visual apprehension of movement because the temporal intervals between the exposition of the successive members are smaller than the time necessary for the movements of the eyes to occur. This argument, we must insist, is wholly fallacious. One cannot argue directly from the physical order to the phenomenal. The rise and the temporal course of the neural excitation is not identical with that of the stimulus. Because one member follows another with a certain fixed temporal interval does not imply that the second will be seen at the precise moment of its presentation. Members of the *Gestalt* school have tried, as we know, to set up a precise correspondence between the physical and the phenomenal—a sort of physico-phenomenal constancy; but we have but to turn to a major investigation by Korte (a student of Koffka's) to see this 'constancy' break down. Korte shows that, although the physical sequence may be of one sort, the phenomenal movement may be in the opposed direction.[6] Bethe had already demonstrated the same fact. Thus when a series of adjacently placed lights was simultaneously lighted the observer obtained a clear sequence along the series, the direction of the sequence depending not upon the order of the physical lighting but upon other ("subjective") factors.[7]

Conclusion. It is commonly agreed that certain visual perceptions of movement of the clearest sort are conditioned by movements of the eyes. Can we say as much for the perception of apparent movement under discrete retinal stimulation? At least we have evidence that eye-movements do frequently occur under such stimulation. This evidence appears (1) in the direct reports of the *O*s upon eye-movements; (2) in the presence of various intermediate or additional members; (3) in the movement of negatively colored lines; (4) in the actual movement of a line before the second member is seen; (5) in simultaneous movement of two lines in the same direction; (6) in the spatial dislocation of the stimulus object; (7) in the momentary disappearance of the first member while movement is in progress; (8) in the movement of a line beyond the position of the second member; and (9) in striking phenomenal differences in the intensity, brightness and extent of the stimulus objects. Now these facts certainly point to eye-movement as a common condition; and, on the whole, they seem best to be explained from this direction. While we cannot assert that eye-movements chiefly determine the apprehension of movement under successive stimulation, we are sure that they do frequently occur at the time movement is seen. It appears not at all unlikely, then, that they serve as one of the conditions upon which perceptions of this general type rest. In view both of the factual and the theoretical importance of ocular movements in these stroboscopic perceptions it is obvious that the movements themselves must be very much better controlled and recorded than they have thus far been. Upon the experimental means to this end we are now engaged.

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[6] A. Korte, Kinematoskopische Untersuchungen, *Zeits. f. Psych.*, 72, 1915, 193-296.

[7] A. Bethe, Beobachtungen über die persönliche Differenz an einem und beiden Augen, *Arch. f. d. ges. Physiol.*, 121, 1908, 1-12.

REACTION TIME TO PHOTOMETRICALLY EQUAL CHROMATIC STIMULI

By JOSEPH L. HOLMES, Columbia University

Is the simple reaction-time to light conditioned only by the intensity of the stimulus or is it also influenced by wave-length? That the latter is true is implied by Henmon[1] who measured, by the method of discriminative reactions, the time for the perception of difference between pairs of colors. His experiments were limited in the main to the red end of the spectrum; though other pairs of colors, as well as black and white, were also used. The stimuli were derived from colored cards. No attempt was made to equate for intensity.

The following table gives a summary of the results from one *S*:

STIMULI DISCRIMINATED	REACTION TIME
Black and White	196.6
Red and Green	203.2
Blue and Yellow	199.8
Red and Blue	212.1
Green and Yellow	211.7
Red and Yellow	217.7
Red and Orange	245.2
Red and Orange mixed with 25% Red	253.3
Red and Orange mixed with 50% Red	261.2
Red and Orange mixed with 75% Red	269.5

In discussing these results Henmon says, "It is hardly necessary to state that the colors were in each case rich, saturated colors. There is, however, the normal difference in brightness, and as in the previous experiments it is the opinion of the writer that the differences in time of perception are due in some degree to the differences in brightness in the case of red and blue. Of the two variables present in the stimuli the color tone should have given, if anything, a shorter time,[2] and therefore the reasonable assumption is that the brightness causes the differences."[3]

In the present experiment the simple reaction-time method was employed and the stimuli were photometrically equated red, green, yellow, blue and white lights. The time of reaction was measured with a Hipp chronoscope in which a weight was substituted for the springs to balance the armature, the usual precautions being taken to insure accuracy by frequent tests with a gravity chronometer.

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[1] V. A. C. Henmon, The time of perception as a measure of differences in sensations, *Arch. of Phil., Psych., and Scient. Meth.*, 1906, 1-75.

[2] By "shorter time" Henmon means that the reaction time, *i.e.* the discrimination time, should have been shorter.

[3] *Op. cit.*, 9.

S was concealed from *E* and the apparatus by a screen extending from the floor to a point well above his head. The table and screen were illuminated by a 50-watt daylight lamp at the back and to the right of the *S*'s head thus preventing dark adaptation. At the edge of the table was placed the head-rest of an opthalmometer which secured a constant distance (58 cm.) between *S*'s eyes and the stimulus and thus a constant visual angle. The stimulus was in the median line and on a level with the eyes. At *S*'s right, on the table, was placed an ordinary telegrapher's key with which he reacted by an upward flexion of the wrist.

Extending at right angles from the screen behind which *S* was seated, was a track 30 cm. wide and 407 cm. in length. On the track was a box 90 cm. high which moved on wheels. On this box at its end farthest from the screen was placed the mechanism which exposed the lights. This included two electro-magnets. From the armature of the magnet extended an arm about 3 in. in length which was attached to the back of a front silvered mirror hinged at the bottom. By allowing the current to pass through the magnets the angle of reflection of the mirror could thus be changed. The source of light was a 25-watt tungsten daylight-lamp in an optical box placed 40 cm. in front and below the level of the mirror. On the screen and on the level with the mirror and *S*'s eyes was a hole 3 cm. in diam. covered with finely sanded glass.

To expose the stimulus, then, a switch was closed, allowing the current (kept at 8 volts) to pass through the magnets moving the armature, which in turn deflected the mirror and so reflected the light from the optical box into the hole in the screen. Beneath the arm which connected the armature with the mirror was a small rod bent downward at each end to dip into mercury cups and supported at the middle by a pin passing through it to uprights, thus allowing it to oscillate. When the arm moved backward, deflecting the mirror, it tipped this rod, raising it so as to break the connection in one mercury cup, and thus starting the chronoscope, and immediately after making it in the other. The circuit through the lower coils of the chronoscope was broken by *S*'s key.

Between the source of light in the optical box and the mirror were placed gelatine filters furnished by the Research Laboratory of the Eastman Kodak Company. The light transmitted by them was as nearly monochromatic as is usually obtained by gelatine filters. They were equated with a standard flicker photometer at the Electrical Testing Laboratories, New York City, the source of light used in the photometer having been matched with the light of the lamp used as a source during the experiment. Then the relative distances at which the lights were photometrically equal were obtained by varying the distance of the mirror from the screen, these distances being proportional to the readings of the photometer-scale.

When photometrically equated the red, green and blue were rich, saturated colors, only the yellow being affected in color tone (said by the *S*s to be "gold color"). Described in wave-length the colored filters were as follows:

Green	560-510
Blue	510-360
Red	700-650
Yellow	700-500

E was seated at a table before and to the side of the screen with the chronoscope, a voltmeter and a switch for the magnet circuit before him. When this switch was opened the mirror tipped forward again, causing the stimulus to disappear. The duration of the stimulus was for the time of the reaction, the switch being opened as soon as the *S* had reacted. The armature of the exposure-apparatus was well padded and the other parts lubricated to prevent any distracting sound. The chronoscope was stopped only for rewinding.

The current used was that of the university lighting system reduced by rheostats to 4 volts for the chronoscope circuits and 8 volts for the exposure-apparatus. In the lamp of the optical box 120 volts were used with a voltmeter to detect any appreciable change. There was a constant fluctuation of two volts (119-121). Only once during the experiments was this exceeded, and then observations were discontinued until normal conditions prevailed.

The following directions were given to each *S*:

"The subject will seat himself facing the table with both arms resting upon it, the right hand at the key. He will look at the screen before him, focusing upon the hole where the stimulus appears. The experimenter will start the clock and say 'Ready.' The subject will then press the key with the first two fingers and get ready to react, giving his attention to the stimulus (the reactions are to be sensory). After the appropriate interval the stimulus will be exposed and the subject will react by releasing the key with an upward motion of the wrist."

The interval between the "ready" signal and the exposure of the stimulus varied from 2 to 4 sec.

Two *S*s (G and N) were employed, both thoroughly practiced, having previously served in extensive experiments upon reaction-time. G gave two sets of reactions to the five stimulus lights (including white.) In both sets 10 reactions to each stimulus made a series, the order being varied to counterbalance the effects of fatigue and practice. Each series began with a new stimulus, the first stimulus of the second series being the second of the first and so on until each stimulus had occupied each position twice. N completed one set.

The following tables give the average reaction times to each light, their mean variations and probable errors, and the differences with their probable errors.

TABLE I, SUBJECT G

Set i

	WHITE	GREEN	YELLOW	BLUE	RED
Av.	167.89	169.16	169.74	170.19	170.59
M. V.	2.91	4.27	6.39	2.92	6.53
P. E.	.77	1.14	1.70	.77	1.74
Differences		1.27	.58	.45	.40
P. E.		1.54	.82	1.06	1.77

Set ii

	YELLOW	WHITE	RED	GREEN	BLUE
Av.	177.93	178.43	178.98	179.48	180.0
M. V.	4.74	5.09	4.96	5.43	5.18
P. E.	1.26	1.35	1.32	1.44	1.38
Differences		.50	.55	.50	.52
P. E.		1.21	1.20	1.01	1.39

TABLE II, SUBJECT N

	BLUE	YELLOW	GREEN	WHITE	RED
Av.	171.51	172.29	172.84	173.49	176.25
M. V.	9.07	8.35	9.20	7.88	9.85
P. E.	2.42	2.22	2.45	2.10	2.62
Differences		.78	.55	.65	2.76
P. E.		1.25	1.77	1.87	.92

Inspection of the tables shows that the differences in time are small and unreliable. In each set the order for the five lights is different, being as follows;

S	GREEN	BLUE	WHITE	RED	YELLOW
G Set i	2	4	1	5	3
G Set ii	4	5	2	3	1
N	3	1	4	5	2

Individual differences in the perception of brightness, as well as differences due to the action-time of the retina, would seem—if they exist at all—to be obscured by errors of observation. The results seem to justify the conclusion that the time of reaction is not a function of wave-length.

A LABORATORY FOR THE STUDY OF CONDITIONED MOTOR REFLEXES

By H. S. LIDDELL,[1] Physiological Field Station, Cornell University

It is unfortunate, in view of the theoretical importance of data on the conditioned reflex, that so little information is available concerning the construction of a suitable laboratory and the technique of conducting these experiments. Practically all the information has been published in Russian and is inaccessible to American investigators. With this in mind it was thought that a description of our laboratory which was established (in the course of an investigation on the effect of thyroidectomy on the higher nervous functions of the sheep and goat) for the study of conditioned motor reflexes, might prove helpful to those contemplating similar experiments.

The animal room, shown in Fig. 1, is partially sound proofed by a layer of heavy felt, a 4 in. air space, and an inner lining of celotex. Between the animal room and the apparatus room (Fig. 2) is a window fitted with 4 panes of glass, with air spaces between, through which the experimenter can observe the animal's responses. An animal room completely sound and vibration proof is, of course, the ideal but as our laboratory is situated in an orchard away from the road and some distance from the city the precautions described have proved adequate. It is also desirable, as Pavlov has pointed out, to maintain a constant room temperature and to employ artificial illumination of fixed intensity. Moreover, the experimenter should be invisible to the animal during the experiments. This requirement can be met by working behind a screen or, better still, in another room. The above precautions are necessary to shield the experimental animal from stimulation by fortuitous environmental agents such as sudden noises, cues obtained by the animal from watching the experimenter's movements, etc. In addition to a suitable room for the animal, apparatus is necessary for the registration of the stimuli presented and of the responses obtained. This apparatus is shown in Figs. 2 and 3.

A brief recital of the procedure employed in establishing a conditioned motor reflex to tactile stimulation will be the simplest way of describing our equipment. The animal is taught to remain quietly in the straps suspended from a beam above the table. The wool is shaved from the skin of the left foreleg and a leather bracelet wrapped with brass wire (Fig. 1, A) is attached to the limb. This bracelet is connected with the secondary coil of an inductorium in the adjoining room. The primary coil is in circuit with a key and electromagnetic signal so that the

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[1] National Research Fellow in the Biological Sciences.

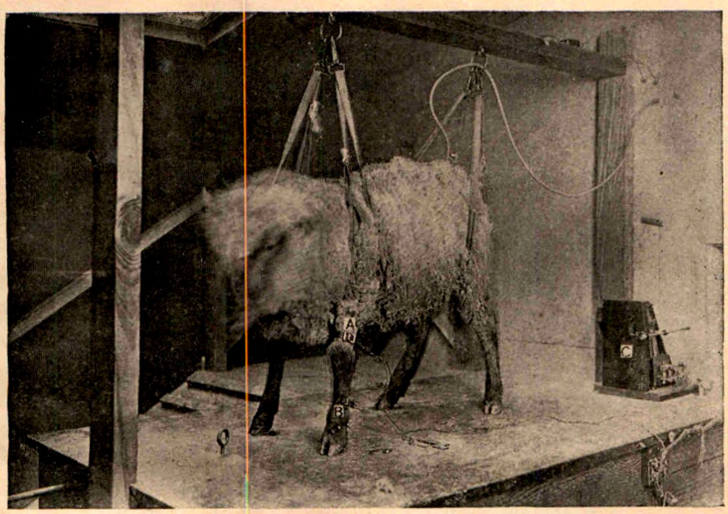

FIGURE 1

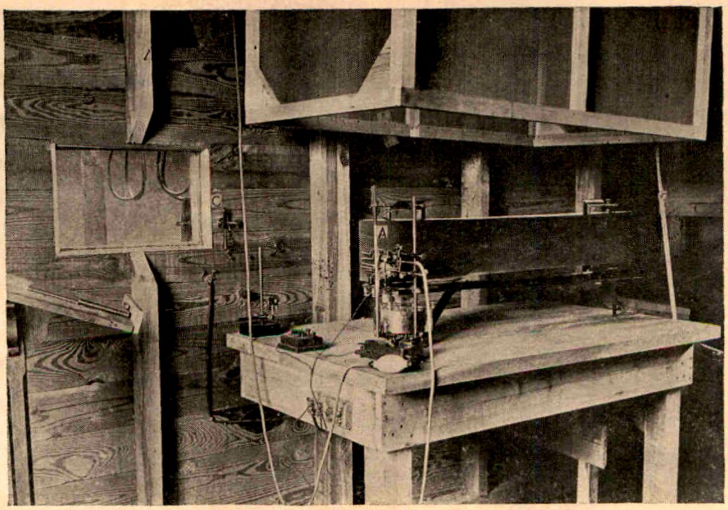

FIGURE 2

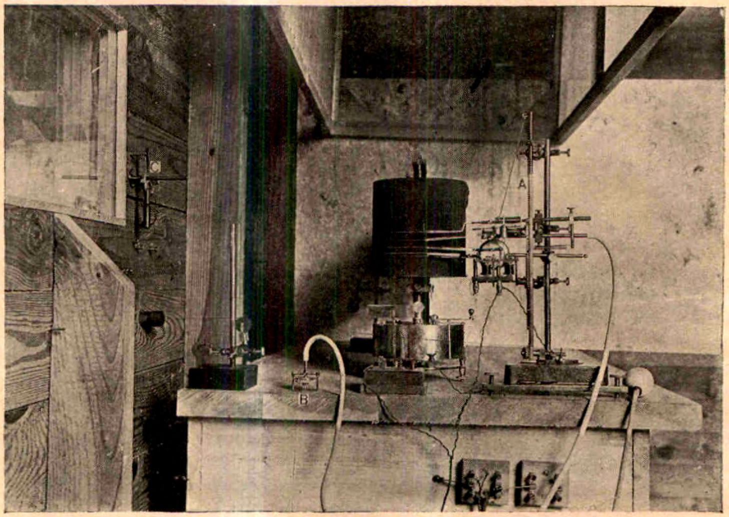

FIGURE 3

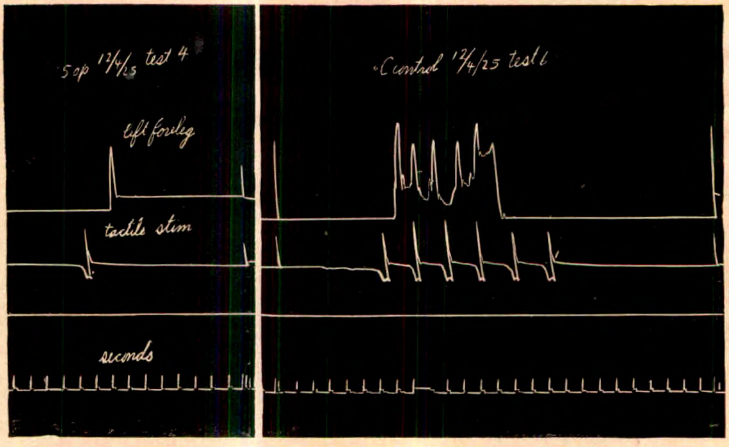

FIGURE 4

application of the faradic current is recorded on the kymograph shown in Figs. 2 and 3. When the electric shock is applied to the skin of the animal's leg a defensive movement results. A thread attached to the leg by a light leather strap (Fig. 1, B) transmits the movement to a reducing lever (shown best in Fig. 3, A) which records it on the smoked paper. This unconditioned response of the leg to the noxious stimulus depends upon permanent connections within the animal's nervous system. To build up a conditioned motor reflex to tactile stimulation, involving the formation of temporary connections in the nervous system, a small skin area is shaved and a spot tatooed. The tactile stimulus is delivered by means of a tambour (for purposes of description shown in Fig. 3, B). To the rubber membrane of this tambour is cemented a wooden block bearing the stimulating agent, which consists of 5 sharp points—pins, coarse bristles, or any other form of tactile stimulus may be used. This tambour is actuated by a rubber bulb and a side tube leading to a recording tambour makes possible the registration of the stimuli as they are presented. The perforated brass disc of the stimulating tambour, through the aperture of which the pins pass in descending to press upon the skin, is fastened to the skin with a cement composed of resin, beeswax, and ferric oxide. The stimuli are delivered once every two seconds and precede the application of the electric shock from 2 to 12 sec. The conditioned stimulus is also at times applied coincidently with the shock. The interval between combinations of conditioned and unconditioned stimuli is varied from 30 sec. to 5 min., for only in this manner may the development of a conditioned reflex to a definite time interval be avoided.

Fig. 4 shows two records of conditioned reflexes of the type just described. The top line of the tracing shows the leg movement resulting from the stimulation of a spot on the skin, traced by the recording tambour on the second line from the top where the drop of the horizontal line denotes the moment of tactile stimulation. The third line from the top marks the application of the induction current to the skin but in the tracing shown no shock was delivered. At the bottom of the tracing time is recorded in seconds.

The metronome provides a convenient conditioned stimulus. Its operation when used in the conditioned reflex experiments will be clear from an inspection of Figs. 1-3. The metronome is placed behind the animal (Fig. 1, C) and can be started and stopped by the experimenter from the adjoining room by means of a rod passing through the wall, bearing at one end a short bar and at the other end (Fig. 2, C; and Fig. 3, C) the knife of a single pole switch. When the switch is closed the bar releases the metronome pendulum and at the same time an electric circuit is closed through a signal magnet which traces the metronome beats on the smoked paper of the kymograph.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

Communicated by WINIFRED F. HYDE

I. SPATIAL SYMBOLS IN THE APPREHENSION OF TIME

By J. P. GUILFORD, Cornell University

The following Study was an attempt to determine the extent to which time tends to be apprehended in spatial terms, and whether there are any space symbols for time which occur more or less universally. The results bear upon the problem of imagery in thought as well as upon the perception of space and time.

The method is introspective, with the exception of two parts of the Study in which motor attitudes of the *S*s were observed. The *S*s were an unselected group of students in general psychology. All of them were somewhat acquainted with introspection, since the experiment was given near the end of their course.

The investigation was inspired by the observation that, in the writer's own experience, time takes rather definite and vivid spatial attributes. For example, the author finds his past projected to the rear and downward, with the future extending to the front and upward. From various other observations and the reports of temporal experiences of others, the following questionnaire was developed, along with a list of instructions for applying it to the *S*s:[1]

INSTRUCTIONS AND QUESTIONS

A. See that the student is sitting up facing the front, head up, eyes to the front, yet not in a rigid position. Read these instructions and watch the student for any significant motor responses. Does he look up or down?

(1) Think of some event or circumstance connected with your graduation from high school.

(2) Recall your earliest school experience, your first day at school, if you can.

B. Have the student write answers to the following questions:

(1) What spatial form does your past take, *i.e.* how is it presented to your mind or imagination? Describe it as accurately as you can.

(2) What is the earliest memory you can recall? How old were you at the time? Give age in years and months as nearly as you can.

C. Read the following and watch for significant motor reactions:

(1) Imagine yourself 5 years from now. 20 years from now.

(2) Imagine yourself at the age of 70.

D. Have the student write answers to the following questions:

(1) What spatial form does your future take, *i.e.* how is it presented to your mind or imagination? Describe it as you did your past.

(2) If you were to represent your past, present, and future by a drawing, what sort of a figure would you make? Draw it.

(3) What form (or qualities) does all historical time, past, present, and future, take for you? Represent by means of a drawing if it takes any definite spatial form.

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[1] Acknowledgment is made to Professor H. B. Alexander who suggested the problem and gave helpful suggestions for the questionnaire.

(4) What does a year mean in your time experience? a month? a day (24 hours)? day and night?

(5) How do you picture the day upon rising in the morning? upon retiring at night?

(6) How do you picture the night upon waking in the morning? upon retiring at night?

(7) What significance, spatial or temporal, attaches itself to New Year's? to Thanksgiving? to the 4th of July?

(8) How do you picture the passing of time at the present moment?

(9) How do you picture 3 p. m. in your imagination? 3 a. m.? 12 noon? 12 midnight?

(10) What would be the effect of turning the clock ahead one hour on your time experience?

Note: Do not in any way suggest to the student what you think is the normal time conception.

If questions (4), (7), or (9) in D are especially difficult, suggest that the student get a 'snapshot' of his consciousness as the words 'New Years,' '3 p. m.,' etc., are spoken. Encourage graphic representation in the student's answer to any of the questions.

Some of the questions have no direct bearing upon the present purposes of the experiment, but it was thought that the opportunity should be taken to ask for other items of interest relating to time experience. This report mentions the results of only those parts which are most significant to our problem.

When the instructions under *A* and *C* were read, the *S*s were watched in order to observe any significant motor responses or attitudes. This was done to test a supposition that, if it were the common response to think of one's past as back and downward, *S* in attempting to recall some past event would tend to look down, and, if the future for most individuals is projected to the front and upward, there would be the opposite tendency to look up or forward. Two difficulties presented themselves in getting numerical data on these general motor tendencies of our *S*s. In the first place, the questionnaire was read to groups of 10 to 12 *S*s and it was impossible for *E* to get an accurate count of the number looking up or down. Secondly, it was difficult to decide in some cases, due to movements on the part of *S*, whether to class him with the 'ups' or 'downs.' It was, however, the consensus of opinion of those who gave the experiment, that, while there is a general tendency for *S* to look down when thinking, either of the past, of the present, or of the future, the tendency is much greater when thinking of the past than when thinking of the future. In the latter case there is a slight tendency to look upward.

An important part of the Study was question *D* (2), in which graphic representations of one's personal past, present, and future are called for. Eighty *S*s, or 91% of those who were questioned, were able to comply with the instruction. The responses were extremely varied and classification of the figures is rather difficult, but, with the exception of one, which represents time as an expanding circle, they can be roughly grouped into 5 types. The classification merely shows the direction of the curve. All horizontal movements, with one exception, were from left to right, a fact which is doubtless due to our conventional habits of writing. The vertical movements are significant. Forty-six *S*s, or 58% of the cases who reported spatial forms, represented the past below the present and the future above it. In other words, the curve has an upward slant; it ascends. The simplest figures of this type were curves beginning with a straight line which rises at an angle of 45°, representing the past, continues in a horizontal line, representing the present, and ends again with a 45° rise, representing the future. Twenty-six figures were as simple as this, with only minor variations. Other figures of the same type were more complex, such as one in which the individual represented himself climbing an incline resembling a ladder, or another, which showed a wheel rapidly spinning up a hill. Four cases of the ascending type lay only in the vertical direction. The next most frequent type represents the past, present, and future on the same level; there were 20 cases, or 25%. Some of these figures were merely lines, straight, wavy, or broken. Others were three-dimensional figures, 3 of them with two cones whose bases are in common at the center (the present), and 5 with two cones or pyramids with their apices joining at the center to represent the present. Ten *S*s, 12.5%, pictured the present at the crest of a wave; the curve was convex upward. Two *S*s drew a curve which was convex downward. In only one case was there a descending curve, and then the drop was a vertical one.

Several difficulties present themselves in an interpretation of these results. We are not sure that the figures drawn by the *S*s are necessary for the meaning of extended time or, granted the necessity, that we know that they are the *only* ways in which the *S*s could represent their time experience; therefore we can not certainly call these forms habitual images, although they appear to belong in a class with number forms and the like. Nevertheless we may assume that the figures obtained at any one time are a fair sampling of the most suitable or common spatial forms. It may be objected, too, that the *S*s were influenced by the conventional symbols which represent life as an uphill struggle or progress. While this may be true in part, it is also true that a negligibly small number of *S*s used any such current figures; most of them drew rather abstract and simple curves. In any case we have here a sample of the time conceptions held by people at large. Again it must be noted that we are not sure that in 90% of our *S*s spatial images are functional in their usual time experiences, for in this experiment some *S*s had to be urged to find a spatial figure to represent time. For some the task was easy: a figure readily suggested itself; for others it was difficult. Whether this difference is due to lack of spatial imagery or of the power of introspection we are unable to say. Since 90% report spatial imagery, however, the chances are that it is present universally in some degree, and undoubtedly contributes to the meaning of time experience. The data therefore tend to show that every individual carries with him a spatial frame of reference in which events, past, present, and future, find a place, and that there is some uniformity in the general character of these spatial forms.

The answers to question (3), concerning the images of historical time, throw light upon the same problem. In this case all *S*s reported imagery of some sort, but only 69, or 78%, drew abstract spatial figures. These 69 figures may be classified into three types. One type is a rising figure very similar to that representing personal time. Forty-three, or 62%, drew this type of figure. Horizontal figures were given by 21 *S*s (31%), and 5 *S*s (7%) made use of a circle or other closed figure. Other imagery reported was as follows: of historical events, 8 cases; of dates, 7 cases; a train, a stream, vast space, a container, one case each.

Answers to question (8), "How do you picture the passing of time at the present moment?" show some rather naïve introspections on the experience of the duration of time. Sixty-two of the cases reported some more or less definite images of the passing of time. Classification of these images was still more difficult, but for the sake of brevity of report they may be grouped, without doing violence to the original reports, as follows: a 'slipping away,' 7 cases; a line, 6 cases (3 a moving line); events, 5; rhythmic units, 5; a watch or clock (image of hands or of ticking), 5; a wheel, 4; a train, 4; simply 'a movement,' 4; an endless chain, 3; a flying arrow, 3; crest of a wave, 2; climbing a hill, 2; work being done, 2; a point, 2 (one a moving point); celestial movements, an hour-glass, a stream, a crawling snake, a crawling snail, 'hurling through space,' a 'zigzag,' one each.

What general conclusions can we draw as to the perception of passing time from this array of pictures of duration? It is true that our reports may be partly determined by the fact that the *S*s were already acquainted with the elements which psychology has determined as aids in the perception of time. Nevertheless there were almost a negligible number of responses which gave simply the terms 'rhythm,' or 'events,' or 'bodily changes' as elements in the awareness of passing time.

Three general elements occur often in the list. The most frequent is that of movement, with at least 37 references either to some moving object or simply to movement in the abstract. A second element is that of rhythm: there were 17 references to time as being cut up into units of some sort. A third element, which is not quite so evident, is that referring to work being done, or of energy being expended. We may suppose that indirectly, by empathy, nearly all references to movement involve this element which depends upon kinesthesis for its basis.

Thus, so far as these introspections reveal, we can reduce the perception of time into the simple elements of kinesthetic sensations and images, accented by rhythms. Whereas the images secured in the reports upon long temporal extents tend to take the form of traversed space for the past, and traversible space for the future, the images given to represent the passing of time in the present are for the most part images of movement, or of the traversing of space. Thus it would seem proper to say that time is in a psychological sense 'traversed space.' This would not mean that space experience is more primary than time experience, for other data might show the converse also to be true, that the perception of space is dependent upon temporal images, or that space is 'lived time.' For example, the length of a line might be perceived because of an image of the eye-strains necessary to traverse it; the durative attribute of the strain would here be the most important aspect of it for the meaning of distance. Whether the 'lived time' during a bodily movement is more or less primary than the 'traversed space' we cannot say; both depend upon kinesthesis for their content and their meaning.

Conclusions

1. Ninety percent of our 88 *S*s had spatial images of their past, present, and future of such definiteness that they could represent them by means of a drawing. Fifty-eight percent of these *S*s drew a rising figure.

2. Eighty percent were able to draw spatial figures to represent historical time. Sixty-two percent of these figures were ascending, 31% were horizontal, and 7% some closed figure.

3. Seventy percent reported a definite image for the passing of time at the present moment; 60% of these picture it as movement of some sort, and the remainder experience it in rhythmic units, or as some work being done, or as energy being expended.

4. In more general terms, we may say that spatial images, which are probably often of the habitual type, carry the meaning of time and furnish a frame of reference for personal and historical time. The experience of passing time is often accompanied by images of movement, which again shows the importance of spatial images in the apprehension of time.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by JOHN P. NAFE

XXX. THE PSYCHOLOGY OF COUNTING

BY MILES A. TINKER

We originally began this investigation with the intention of studying the influence of peripheral vision in the visual process of reading.[1] As preliminary we started to repeat the work of Jastrow and Hall on counting visual stimuli.[2] Before we had progressed very far the problem shaped itself into a study of the conditions and process of counting. Our quantitative results are of little significance here and so we shall present merely the qualitative aspects of counting visual stimuli.

Dr. E. C. Sanford, Mr. C. E. Dickinson and Mr. M. A. Tinker, members of the graduate department of experimental psychology, acted as *O*s in the experiment.

In the first part of the experiment letters were printed (with a typewriter) 1 cm. apart in a horizontal row on a white background which was placed on the drum of a kymograph. The order of the letters was determined by chance. The *O*s viewed the letters through a slit 1 cm. high and of variable width cut in a gray card-board screen. In the preliminary work the width of the opening was varied from 1 to 5 cm., but in the later experiments the opening was 1 cm. wide. *O* held a card before the slit until the ready signal was given. The revolving drum gave the conditions of visual reading except that the eyes remained comparatively stationary and the letters or dots moved. In all except the preliminary work counting dots was used instead of naming letters. We had an even-spaced series, one dot at every centimeter, and 13 odd-spaced series where the distance between dots varied from 1 to 6 cm., the amount of space being decided by chance. This gave irregular grouping of the dots.

Procedure. Only a few observations were taken with the naming of letters. In the work with the even-spaced dots the number of units in the series was changed with each observation and the speed was varied from a very slow speed up to the maximum speed for the particular *O*. With the odd-spaced dots the series was changed with every observation but the speed was constant for any single sitting. However it was changed from slow to a faster speed in consecutive sittings. The maximum speeds were found at which the uniform and irregular series were counted correctly. At the end of every series *O* was questioned concerning the ease or difficulty he had experienced and also concerning his assurance that the number he had obtained was correct. At certain times *O* was also asked for a detailed introspection on an observation. The *O*s were thoroughly familiarized with the following directions:

"This is an experiment in counting. After the ready signal you are to remove the small card from the opening in the screen and as soon as the dots begin to appear you are to commence counting them out loud and continue to the end of the series. You must count each dot separately."

The maximum rate of counting uniform series of dots was found for 2 *O*s when the alphabet, French numerals and German numerals were used for the verbal response, and for one *O* when Chinese numerals were used.

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[1] The experimentation of this study was done with Dr. E. C. Sanford at Clark University in 1923.

[2] Joseph Jastrow and G. S. Hall, Studies in Rhythm, *Mind*, 2, 1886, 55-62.

Results. Counting proves, on analysis, to be a reaction process. In some cases it is quite like the process in reaction-time experiments, in others a little more complex. The simplest case is reacting the same for each unit. This is like counting things that occur serially at a uniform rate such as counting the pulse-beat with a mechanical counter. Here the counter reacts to each stimulus as it occurs by a prearranged motor response or if he is using a single word he reacts by a prearranged speech movement. This form of counting is common among primitive peoples.[3] The rhythm in this simple form of counting is the same as in verbal response to a series of units. Verbal counting is practically the same as the simpler form except there is a different number name for each unit. It is an elaboration to the simple reaction and is like reacting in a specified way. The mental set is important. *O* counts as the stimuli appear, and every number as presented and responded to, induces an inhibition of itself.

In counting a uniform series *O* attempts to establish a rhythm as soon as the dots begin to appear. If he is able to do this in the first 5 or 10 units the counting goes on smoothly; if not, he is uncertain as to the correctness of the number obtained. The maximum rate of counting a regular series depends on *O*'s ability to establish this rhythm. Rhythm is obtained by grouping units in 2's, 3's, 5's and 4's. The 4 group is obtained by linking together two 2 groups. With medium rates rhythm is comparatively easy to obtain. With very slow speeds, however, it is harder and at maximum or above maximum rates it is still harder to get. Whenever rhythm is established assurance as to the correctness of the number obtained is high. On analysis it appears that high assurance consists in a complete motor response that begins and comes to summation before another response begins. This holds true for the counting of irregular series as well as for uniformly spaced single units. The reaction in counting a group must be completed before the response to the next group begins in order to have maximum assurance. Maximum rate for irregular spaced dots is 2 to 2.4 dots per sec.

When the alphabet or foreign language numerals were used in counting, the process was essentially the same as with other verbal counting except it was a little more complex due to less familiarity with the verbal symbols. Maximum rates for uniform series are: French, 1.7 and 4.6 dots per sec.; German, 1.8 and 1.9 dots per sec.; alphabet, 3.4 and 4.3 dots per sec.; Chinese, 1.7 dots per sec.; English, 3.6 and 4.8 dots per sec. For all *O*s the counting in English was fastest.

Simultaneity in counting is like reacting with a uniformly spaced ready signal and tends to a 'motor' form. The counter gives to every object a pulse of attention, making the corresponding word in the number series simultaneously with the pulse of attention.

When the dots to be counted occur serially but irregularly spaced the process is a little more complex and is more difficult than in the regular series and is analogous to reacting without a ready signal. It is more difficult to match stimulus with response than in a uniform series. Here the reaction cannot be simultaneous and tends to 'sensory' form. The maximum rate in this kind of counting depends on the effective memory span. When the rate for the instant is too fast for immediate connection of stimulus and speech reaction, the response is completed from the memory, and assurance is higher if there is sufficient space to allow the completion of the response for that group before the next dot or group of dots appear. Difficulty in counting irregularly spaced dots is influenced by the speed, grouping of the dots (number in group) and the space before and after any given group.

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[3] L. L. Conant, *The Number Concept*, 1886; The Historical Development of Arithmetical Notation, *Ped. Sem.*, 2, 1892, 149-152.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF ANTIOCH COLLEGE

I. The Apparent Warmth of Colors

By Meryl F. Mogensen and Horace B. English

There exists a widespread belief that colors have a fairly constant 'apparent' or 'psychological' warmth. In this experiment the question is raised whether this apparent warmth is able to affect one's judgment of objects tactually perceived. Using a slightly modified method of paired comparison, we presented 6 saturated colors from the Zimmermann series of colored papers. These were wrapped smoothly about two slide rheostats which were kept at a constant temperature of 42°C.

The more important part of the instructions was as follows: "This is an experiment to test the apparent influence of color upon the warmth of an object. The temperature of the colored objects will be changed from time to time. You are to test the warmth of the two colored objects simultaneously and give your judgment as to which seems the warmer in terms of the colors. . . . . You are to look at the cylinders while judging them." The cylinders were firmly grasped for the space of one second and the judgment given a second later to a ticking metronome. Between judgments the palms were laid upon a neutral grey surface kept at 36°C. Three judgments were made of each pair before change to another pair.

Twenty-five undergraduates, 18 men and 7 women, acted as *S*s. The following table shows the number of times each color was judged warmer. The maximum possible is 30.

Table I

Number of Times Each Color was Judged Warmer

Subject	Green	Blue	Yellow	Orange	Purple	Red
A	21	10	12	18	15	14
B	24	16	12	18	5	15
C	15	18	15	7	14	21
D	18	21	15	5	17	14
*E	13	15	10	17	9	26
*F	19	16	19	13	10	13
G	14	15	13	20	12	16
*H	18	16	14	13	14	15
*I	16	19	13	20	8	14
*J	17	10	17	28	6	12
K	16	13	21	16	12	12
L	15	18	22	11	9	15
M	18	14	13	15	13	17
N	18	21	11	12	12	16
O	15	14	14	16	19	12
P	17	14	13	17	16	13
*Q	13	19	15	16	11	16
*R	17	20	19	14	7	13
S	14	16	12	20	11	17
T	15	13	20	9	19	14
U	16	20	13	12	9	20
V	17	11	18	16	14	14
W	12	19	10	19	15	15
X	19	16	20	14	7	14
Y	19	21	16	14	17	3
Total	416	405	377	380	301	371

*Indicates that S is a woman.

Individual preferences of a fairly constant sort appear; the exact significance of such preferences requires more extensive investigation. But it is clear that there are few significant and constant preferences common to the group. The probable errors of the differences are in most cases too large to warrant ascribing the difference to anything but chance. The outstanding exception is purple. The superior 'warmth' value ascribed to green and blue as compared with purple can scarcely be ascribed to chance; the difference in the number of 'warm' judgments in their favor is respectively 7, and 6.5 times the probable error of the differences. Less certain, but still hardly due to chance, is the greater 'warmth' of red, yellow and orange. The differences are 4.5, 4.3, and 4.1 times the probable error of the difference.

The curious thing about this result is that purple—especially the 'royal purple' here used—is generally considered a warm color while blue and green are cool colors. But purple is also less pleasant than other saturated colors and it is quite questionable whether we have not to do here with a judgment of pleasantness. The surprisingly low position of red is also probably not due to chance. Yet there is little question that red is to be placed among the 'warm' colors.

Although the data are slight, the conclusion seems warranted that the apparent warmth of colors is insufficiently intrinsic to enter into a total configuration or *Gestalt* in such a way as to modify the judgments of tactual warmth.

II. Reaction-Time Symptoms of Deception[1]

By Horace B. English

The chief finding reported by Marston under the above title was the exsitence of two types of 'liars:' good liars, whose word-association time is shortened and whose variability is less when they are attempting to deceive, and bad liars whose time and variability are both greater.[2] As well as we could from Marston's somewhat sketchy description of his methods, we repeated the first part of his experiments. Two lists of ordinary words were printed on appropriate small cards. Before turning up a card for reaction, the *S* was directed by *E* to read either the right or the left list, and to read from the top or from the bottom. To each word the subject was to react as quickly as possible with another word. Following a definite scheme, however, *S* at irregular intervals reversed *E*'s directions but endeavored to keep him in ignorance as to when the directions were followed and when they were reversed. To heighten the emotional tension, some stress was laid upon detection by means of the nature of the words pronounced as well as by the reaction-times. Time was taken for each list as as whole by means of a stop-watch.

Twenty unselected undergraduates took part in the experiment and coöperated heartily in the effort to 'beat the game.' Whether any or all suffered under the consciousness of deception may well be doubted.[3]

Of the twenty *S*s, 7 showed a significant increase in time when endeavoring to 'deceive,' while 9 showed a decrease. Six were significantly more variable in their 'deception' lists as measured by the mean variation, and 7 were less variable. But the *S*s who were faster were not necessarily the less variable, nor those slower the more variable. Marston's two criteria of the good and bad liars run criss-cross. In the following table, a +

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[1] The writer wishes to acknowledge the assistance of Robert Edwards in acting as experimenter and in drawing up a preliminary report.

[2] W. Marston, Reaction-Time Symptoms of Deception, *J. Exper. Psychol.*, 3, 1920, 72-87.

[3] Compare E. R. Goldstein. The Consciousness of Deception, this JOURNAL, 34, 1923, 562-581.

stands for increase in time or an increase in variability when 'lying.' Conversely, a — stands for shortened reaction time and less variability. o stands for a lack of statistically significant difference.

TABLE I
Subjects

Greater Mean	A	B	C	D	E	F	G	H	I	J
Reaction Time	+	—	+	—	—	o	+	o	o	—
Variation	+	—	—	+	—	+	—	+	+	o
Greater Mean	K	L	M	N	O	P	Q	R	S	T
Reaction Time	+	o	+	—	+	+	—	—	—	—
Variation	+	o	o	o	—	o	—	o	—	o

By Marston's two criteria, but 2 of our *S*s are poor liars and 4 good liars. The rest belong to neither type.

The point raised by Goldstein seems to us, however, peculiarly relevant.[4] Does the experimental setting really involve the attitude of deception? Goldstein's results seem fairly conclusive that only irregularly is this the case. The question therefore remains for decision as to the factors which explain fairly consistent and significant differences between individuals in their response to the experimental setting. As no introspective reports were taken, we have no evidence. With all deference to our behaviorist brethren, it seems likely that a supplementary verbal response (introspective *Kundgabe*) would have thrown light on our results as it did on Goldstein's.

III. The Effect of Fatigue on Learning

By Margaret Shelley and Horace B. English

In 1920 Dr. J. J. B. Morgan described experiments to measure the effect of continuous mental work upon memorizing.[1] He used a modified form of the method of paired associates. Ten pairs of German-English equivalents were visually presented serially, 4 sec. to each pair. In recall, 2 sec. were allowed to the German word before the exposure of the English equivalent for a further 2 sec. Three such combined learning-recall periods were given each list. At their conclusion a new list was immediately begun. This was continued for 3 hrs.—850 pairs were employed.

We used Morgan's original word-list, and although our apparatus was manually rather than automatically controlled, the experimental setting seemed very similar. Four college students acted as *S*s.

For scoring purposes the work-learning period was divided into 5 equal, consecutive sections of 36 min., during each of which the *S*s worked on 17 lists or 170 pairs of words. The score is the average number of associates learned in each list of ten words.

TABLE I
Consecutive Periods of Work

Subject	1	2	3	4	5	Average
A	1.32	1.48	1.72	1.50	1.66	1.54
B	0.35	0.58	0.80	0.47	1.08	0.65
C	1.32	1.64	1.72	1.52	1.64	1.56
D	0.04	0.32	0.40	0.38	0.12	0.25
Average	0.75	1.00	1.16	0.96	1.12	

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[4] *Cf.* G. J. Rich, Dr. Marston on Deception Types, this JOURNAL, 37, 1926, 307.

[1] J. J. B. Morgan, The Effect of Fatigue on Retention, *J. Exper. Psychol*, 3, 1920, 319-333. He carried out further experiments, as his title indicates, on retention. With these we do not deal.

As in Morgan's experiment, there is a tendency to show a spurt in the last time section. But where in his results the first section was otherwise the best, in ours it was the worst. Fatigue, uncomplicated by the end-spurt, might be looked for in the fourth period. Three *S*s do less well than their own average performance here, but the difference is a slight one, amounting to but 2.6, 2.7, and 1.3 percent in the 3 cases. The fourth *S* does better in this period than his own average. If the loss in this period is calculated from the *S*'s best score, a slightly better case can be made out but this difference is not great enough to be very important, even if statistically stable. These results are consistent with the earlier results of Oehrn, Woodworth, Vogt, and others cited by Morgan and somewhat at variance with his own. We must conclude that with untrained *S*s ability to learn paired associates does not act as a *test* either of fatiguability or of fatigue.

Indeed, is not the concept of fatigue an *ignis fatuus*? Muscular or nervous exhaustion are clearly defined conditions susceptible of measurement by suitably refined technique. But 'fatigue' is like charity in covering a multitude of sins, the chief of which is ignorance. One of the writers has recently suggested some of the discriminable factors commonly included under the term.[2] No doubt others can be found. The truth is that the term fatigue is popular rather than scientific. It is a symptom, not a disease syndrome, and has therefore about the same place in psychology as 'fever' has in medicine as a statement of a patient's ailment.

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[2] H. F. Whiting and H. B. English, Fatigue Tests and Incentives, *J. Exper. Psychol.*, 8, 1925, 33 ff.

A REVIEW OF THE MARGERY CASE

By Walter Franklin Prince*

Margery the Medium. By J. Malcolm Bird. New York, Small, Maynard and Co., 1925, pp. 518. Referred to hereinafter as *M.M.*

Margery-Harvard-Veritas: A Study in Psychics. Anonymous; issued by M. W. Richardson, C. S. Hill, A. W. Martin, S. R. Harlow, J. DeWyckoff, and L. R. G. Crandon. Privately printed; copies can be obtained from Dr. L. R. G. Crandon, 366 Commonwealth Ave., Boston, Mass. 1925, pp. 109. Referred to hereinafter as *M.H.V.*

Articles in the *Journal of the American Society for Psychical Research*, vol. 19, 1925 (referred to hereinafter as *J.A.S.P.R.*), in *Scientific American*, 1924-25 (referred to hereinafter as *S.A.*), etc.

In May, 1923,[1] there blazed out the most brilliant star in the firmament of alleged physical mediumship that America has seen in fifty years, 'Margery,' the wife of Dr. L. R. G. Crandon, a Boston surgeon. At hundreds of sittings[2], it is claimed, 'ectoplasmic' limbs—extruded from her body and afterward reabsorbed—have performed various acts, such as touching persons seated nearby in the darkness, shoving, lifting and throwing objects, overturning a small table, ringing the bell in a box activated by a contact cover, producing phosphorescent lights, etc.[3] The establishment of these claims would have profound interest for science, since they imply the exercise of energy in a manner at present unknown to physics, and modifications of the human body revolutionary of present physiology. If it be a fact that the vocal and whispered utterances and the whistling sounds which are so frequent an accompaniment are 'independent,' that is, not produced by the vocal organs of any living person, and yet requiring the presence of a peculiarly-constituted person, the fact has deep significance for physics, psychology, and physiology. Also the appearance of live pigeons and flowers, either by special creation or by passage through matter, if factually established, would considerably enlarge the domain of either biology or physics.[4] Many other phenomena are alleged, and they all purport to be manifestations of a spirit, always or nearly always 'Walter,' mainly exercised by the materialization of organs and limbs for the purpose.

The literature of the case has been extensive, as the following incomplete table, confined to this country, shows.

(1) Hundreds of newspaper articles, including many specially prepared by the inner circle, assailing investigators who have reported adversely.

(2) Many favoring articles in Spiritualist and even some other religious organs.

[*Dr. Prince is the Research Officer of the Boston Society for Psychic Research and holds no academic position; cf. *M.H.V.*, 5 f.—Ed.]

[1] *M.M.*, 13.

[2] *M.M.* gives more or less details of upwards of 260 sittings (see table, 486-499) up to Dec. 27, 1924.

[3] For the period before Dingwall's coming, *M.M.* gives the fullest description of phenomena from its author's point of view; for the meagre but best, printed account of the sittings in the period presided over by Mr. Dingwall, see his address printed in *J.A.S.P.R.*, 1925, 125-134; and for the period of the Harvard Group consult the report of its spokesman, Mr. Hudson Hoagland, *Atlantic Monthly*, 1925, 666-681; also the sitting-notes printed (with important omissions) in *M.H.V.*

[4] Chapters XVIII and XX of *M.M.* give the details regarding the pigeon and the flowers. The phenomena of 'apports' seem to have disappeared from the case before any decisive tests were applied.

(3) Articles of essential advocacy by Mr. J. M. Bird in the *Scientific American*,[5] the verdicts of its Committee members,[6] an article by Dr. Crandon attacking the "preliminary" verdict,[7] and an answer claiming to point out his divergences from fact.[8]

(4) A pamphlet by Mr. Harry Houdini, claiming to demonstrate fraud by 'Margery.'[9]

(5) A book, *Margery the Medium* of 518 pages by Mr. Bird, in defense of her phenomena.

(6) A report of the Harvard investigation, by Mr. Hudson Hoagland, in the *Atlantic Monthly*.

(7) Seventeen statements, discussions and letters relating to the case in the *Journal of the American Society for Psychical Research*, March-December, 1925. Of these 6 were by Bird (made a research officer of the A.S.P.R. following the announcement of his conversion), 3 by Dr. Crandon, 2 by his personal friend; three times the material in favor that there was against, and everything against carefully answered by Mr. Bird or Dr. Crandon.[10] The record shows that fuller details of the Harvard findings were refused although the *Atlantic Monthly* report was assailed by Bird, and that proof offered from outside the group that he had distorted the facts failed to interest the *Journal of the American Society for Psychical Research*.[11]

(8) Lately the Crandon group has gratuitously distributed an anonymous pamphlet of 109 pages, entitled *Margery-Harvard-Veritas*.[12]

The Research Officer of the A.S.P.R. has advocated Margery before many Spiritualist and general audiences, and Dr. Crandon and others have similarly proclaimed her genuineness. I know of no platform propaganda in opposition, aside from the diatribes of Houdini.

It is time that an outline history and critique of the case should be written. This I am attempting on the sole basis of a great mass of printed and manuscript documents. I abstain from mentioning any of the unpublished observations of the phenomena by myself, not acting as a witness or an attorney, but strictly as a reviewer.

I. *The First Year of the Phenomena.* About two-fifths of the 518 pages comprising *M.M.* are devoted to the 133 sittings from May 1923 to April 1924, the materials having been furnished by Dr. Crandon.[13] But as the author tells us that "no record was made of the arrangement of the sitters or the degree of control" at the first 63 sittings, and that such vital information only "sometimes appears incidentally or otherwise" in connection with the 70 which followed,[14] this part of the compilation can hardly impress the scientific reader, however convincing sittings may have been to persons present. Hope quite abandons such a reader when he finds the records mostly chopped into bits, classified and reassembled for dramatic effect under such titles as "The Gobble-Uns'll Git You Ef You Don't Watch Out," and "Ghostly Fingers in the Dark."[15] The anecdotes are liberally sprinkled with the compiler's subtle and somewhat oracular remarks.

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[5] *S. A.*, July and Aug., 1924. [6] *S.A.*, Nov. 1924, Apr. 1925. [7] *S.A.*, Jan. 1925.
[8] *S. A.*, Feb. 1925.

[9] *Houdini exposes the tricks used by the Boston medium 'Margery,'* 1924, pp. 32.

[10] See file of *J.A.S.P.R.*

[11] Both letters of declination and the manuscript of the proffered reply are on file in my office. Bird's article is reproduced in *M.H.V.*, 83-93. I find it impossible to square many of its statements with the evidence.

[12] *M.H.V.* in these notes.

[13] *M.M.*, VIII

[14] *M.M.*, 486, 489.

[15] Sometimes scraps of a sitting are scattered through seven chapters. See table, *M.M.*, 489-491.

II. *The Scientific American Investigation.* The Committee formed by the *Scientific American* to study cases brought to its attention consisted of Dr. Wm. McDougall, Harvard psychologist, then president of the American Society for Psychical Research; Dr. D. F. Comstock, formerly professor in the Massachusetts Institute of Technology, interested in psychical research; Mr. H. Carrington, author of books dealing with psychical research; Dr. W. F. Prince, then research officer of the A.S.P.R.; and Mr. H. Houdini, magician. Mr. Bird became, not by action of the committee, secretary to the committee and, as he says, "stage manager."[16] In the latter capacity he was chiefly responsible for procedure, the strict function of committee members being to act as judges. That bane of scientific research—premature publicity—was precipitated in spite of protests from the Committee.

Never was an investigation more hampered, both at this and subsequent stages, by prohibitions and arbitrary rules.

(1) An investigator ought to be able to see, at least in some kind of light. But the darkness was relieved by red light, more or less, only when 'Walter' gave the signal,[17] reminding one of the magician who withdraws the curtain after a wonder is prepared behind it. Darkness may be required by a 'psychic law,' but it is also convenient for hoaxing.

(2) It ought to be possible to make reasonable use of those important instruments of investigation, the hands. But hands were held fast except momentarily under strict regulations. The 'ectoplasmic limb' may shove, lift and throw objects, slap sitters and pull their hair, and even overturn the table or rip away the wing of the cabinet, but the slightest touch laid upon it unbidden, it is explained, might fearfully shock the medium. This seems unreasonable and yet investigators obeyed the inscrutable law.

(3) Investigators ought to be permitted cautiously to test how far alleged laws are actually valid. An investigator on one occasion ventured to move his own and the psychic's hands slowly into the area between her and the bell-box, and no objection being made continued to do so.[18] Since the bell rang nevertheless, the law against this founded on the theory of 'psychic rods' seemed disproved, yet after-experimentation of the kind was forbidden. If in his one daylight sitting the two momentary muffled tinkles really came from the box and not from beneath the medium's skirts five inches distant, the important discovery was made that the box-bell could be rung under the most favorable conditions.[19] She was uninjured and fresh enough to have another sitting that very night, and four more within a week,[20] but he never could get another daylight sitting.

(4) Scientific investigators are supposed to plan the nature, particular time and duration of their tests. But here 'Walter' autocratically directed, consenting or refusing, unexpectedly switching phenomena or terminating a series of particular type before the hiatus in the proof was filled.[21]

(5) Besides the above traditional ritual restrictive of scientific liberty, the following rules were invented, constituting, whether or not so intended, an effective device to hamper investigators consenting to be so bound, and to embarrass them after an unfavorable verdict.

(a) Every member must sign a blanket report composed of individual observations dictated during the sitting by different members, without a proviso that he is responsible only for his own observations and descriptive terms. The member who declined to assume responsibility for any errors other than his own has since been voluminously berated for not subscribing to the individual observations of his colleagues since they were colleagues,[22] yet the members have been as vigorously scolded for not repudiating the observations of Houdini, also a colleague.[23]

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[16] Mr. Bird in *Banner of Life*, Aug. 8, 1925. See also *M.M.*, 151.

[17] "For fear of injury to the medium no lights shall be turned on at any time without permission of 'Walter'." Draft of rules written by Dr. Crandon, dated Aug. 4, 1924. But Dr. Crandon could turn on white light in the midst of phenomena, with impunity. See *M.M.*, 139.

[18] *M. H. V.*, 9.

[19] Margery's version in *M.M.*, 471-473; Prince's statement in *S. A.*, Feb. and Apr. 1925; *Banner of Life*, Nov. 28, 1925; Dr. Crandon's version in *M. H. V.*, 15.

[20] *M.M.*, 498.

[21] *M.M.*, 26, 161-162, 165, 224, 357, 400.

(b) Another rule, not set forth to any of the investigating commissions, seems to have been in Dr. Crandon's mind. Every particular word or expression dictated during the sittings, no matter how empirical and tentative such necessarily were, especially during the first stages of an investigation, no matter what accidental infelicities of extemporaneous language might have entered, no matter how later discoveries negatived earlier deceptive impressions, must be held sacred forever. There can be no other point in italicising passages of the sitting notes printed in *M.H.V.*

(c) Everything deemed important must appear in the contemporaneous record,[24] the corollary being that nothing not there appearing can ever afterward be deemed important. A more paralyzing rule could not be devised. Frequently facts which at the time seem unimportant and which are clearly remembered by several persons afterward are found to be very important, fitting into the now apparent mosaic. Dr. Crandon and his spokesmen frequently disregarded the absurd rule which they would impose upon others.[25]

(d) The dictated words "control perfect" and the like, shall imply that the possibility of fraud is eliminated;[26] yet thousands have erroneously supposed themselves successfully controlling a medium's movements, and the most intelligent may be deceived for a time. A rule to make a blunder sacred forever or to convict an investigator for becoming wiser is an oddity.

(e) Any observed indication of fraud must be announced immediately.[27] This rule would have the effect of coaching a fraudulent medium and preventing the development of fraud to the point of proof. Houdini's compliance with the rule and the consequent wrangle have been denounced,[28] and so has Hoagland's delaying the announcement until the sitting was over.[29]

(f) At one sitting the investigators allowed Dr. Crandon to dictate into the notes an expression which, as it then read, is rather of a truism: "Unless there is a suggestion of fraud in this record, IT IS *assumed* to be non-existent, *in our opinion*."[30] But by altering this to "if no suspicion of fraud is entered in these notes, it SHALL BE *deemed* non-existent,"[31] there is made a law that an assumption based upon the opinion of an hour shall perpetually have the force of a declaration of fact.[32]

(g) The husband of the medium must be one of her controllers. Exceedingly few of hundreds of sittings were exempt from this condition.[33] Such a situation is scientifically suspect and in view of the fact admitted in detail even by Bird, that Dr. Crandon's testimony as to continuity of control is unreliable,[34] it greatly increased the difficulty of ascertaining what took place in the darkness.

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[22] *M.M.*, 454; *M.H.V.*, 9; *S.A.*, Jan. 1925, 65; filed letters by Dr. Crandon.

[23] *Psychic Science*, July, 1925, 127; *S. A.*, Jan., 1925; *Banner of Life*, Oct. 19 and 31, 1925.

[24] Put in writing by Dr. Crandon, and on file.

[25] For one example, see *M.H.V.*, 12. Nothing is in the official notes about Houdini's passing his hand along the medium's arm in the box, though the fact is stated correctly; an "intelligible reason" for the act was given by Houdini at the time. Nor does the record say that Houdini measured the distance from psychic to bell-box at the beginning of the sitting, but it does say that he measured at its *close*. See also *M.M.*, 434.

[26] Put in writing by Dr. Crandon, and on file.

[27] *Ibid.*: "Any sitter believing that he observes fraud shall proclaim it at once."

[28] Houdini's pamphlet, 17 f.; Doyle in *M.H.V.*, 9.

[29] *M.M.*, 23.

[30] Sitting-notes, *M.H.V.*, 43, caps and italics mine.

[31] Dr. Crandon's version, *M.H.V.*, 22, caps and italics mine.

[32] Had the expressions extemporaneously employed by persons guessing in the darkness been thrice as incautious as they sometimes were, to hold them sacrosanct against all after discovery, reflection and analysis, is as intelligent as cabalism.

[33] Dr. Crandon was beside Margery in 109 out of the 112 sittings of the *Scientific American* period whose sitting order was recorded; *M.M.*, 491-496.

The restrictions set forth in the above Sections 1 to 4 inclusive show why the majority of the first commission were not able absolutely to determine that no supernormal phenomena were displayed. In the nature of things the restrictions prevented the absolute determination of fraud if fraud existed unless the rules were violated or revealing accidents occurred.

It was by springing the many-toothed legalistic trap shown in Section 5, that an unreflecting or emotional share of the public have been persuaded that the first commission (as well as the two subsequent ones) was inconsistent and vacillating. It is mainly a contention as to words, yet, barring a few colloquial lapses in the notes, the inconsistencies seem those which inevitably attend the progress from a state of mind unable to explain to one able to explain.

Readers of *M.M.* should be warned to take its utterances with liberal grains of salt. Certainly many of them grossly misstate and warp facts of vital importance. A list of sixty selected examples with their refutations from inexpugnable data was offered for Dr. Crandon and the author of the book to defend, without avail, except that the former wrote: "I can probably find more inaccuracies in Bird's book than you can."[35] Hence it is not here necessary to support the warning by citations.

Yet some of the revelations in this book seem naïve. We learn from it and from other sources[36] that Dr. Crandon paid Messrs. Carrington and Bird their expenses for investigating his wife, and entertained the former in his house 44 days and nights and the latter 57. "Houdini and Prince preferred to stop at a hotel." This preference was perhaps on the ground that it would be embarrassing to render a verdict against persons on whose bounty they had been living, and more embarrassing in the event of a favorable verdict to incur the imputation, if not the actual risk, that friendship and knightliness might affect their judgment.

The verdicts of the *Scientific American* Committeemen were as follows, in brief:[37]

Dr. Comstock: "Rigid proof has not yet been furnished."

Dr. McDougall: "As long ago as November, 1923, . . . I was inclined to regard all the phenomena I had observed as produced by normal means. . . Since that date . . . the inclination described above has grown steadily stronger in the main, in spite of some minor fluctuations, and has now become well-nigh irresistible."

Dr. Prince: "No sitting at which I was present was to me convincing. . . . In fact, I could write a chapter of indications which, in the absence of contravening proof, seem to tell the story of normal and deceptive production."

Mr. Houdini: "Everything which took place at the seances which I attended was a deliberate and conscious fraud."

Mr. Carrington: "Many of the observed manifestations might well have been produced fraudulently—and possibly were so produced. . . . But I am convinced that genuine phenomena have occurred here."

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[34] *M.M.*, 157-159, 256; Bird even states this was "usual."

[35] It is proper to quote this sentence, since it is taken from a letter related to the one which Dr. Crandon in *M.H.V.*, 19, opens to the public. "Dr. Prince declares in writing that he can make out an equally good circumstantial case against Margery or Houdini," meaning if the matter of the rule in the box were taken by itself, disregarding the testimony of many persons, printed or on file, that they observed *other* indications of mediumistic fraud, and since he refers to another letter (making Prince back down when it was Dr. Crandon who wrote that *he* preferred "to let it drop"). The letters, as well as the "sixty examples," are on file and open to inspection.

[36] *M.M.*, 155; *M.H.V.*, 9.

[37] *S.A.*, Nov., 1924; April 1925.

Mr. Bird, likewise convinced, and since the most voluble defender, was not a member of the committee.[38] He was, however, one of the two editors who had "worked out a plan," namely the formation of the Committee, to furnish "material which would have to be accepted as authoritative." These are his words.[39]

III. *Mr. Dingwall's Investigation.* Dr. Crandon arranged with Mr. Dingwall, Research Officer of the Society for Psychical Research, to come from London as a sort of appellate court. The great feature of this series, Dec. 1924 to Feb. 1925, was the production of 'ectoplasm,' sometimes like an ill-formed, cold hand feeling like "blanc-mange," and lying on the table or in the medium's lap, sometimes a curious substance seen either issued from or poked into her ear. The substance was never seen in the process of actual materialization. Unfortunately, Dingwall's hopes that the missing links of authentication would be supplied were disappointed, for in his report he says:[40] "The control of the medium appears to be rigid, and is faultless if we accept Dr. F. H. [Dr. Crandon] as a *bona fide* investigator, which, under the circumstances, he is himself the first to admit is impossible. The conditions, therefore, of the sittings are such that I cannot at present affirm my belief in the authenticity of the phenomena."

Dr. McDougall attended a number of these sittings, and there were indications which greatly increased his doubts. Certain photographs of the "ectoplasmic hand," especially, seemed to him to resemble lung-tissue, articifically shaped into resemblance to an ill-formed hand.[41] In at least one the annular bands of a trachea and a tubular opening appeared to be recognizable.[42] Refused prints of the photographs by Dr. Crandon, it was not until a year later that he could submit them to experts. Now Dr. W. B. Cannon, professor of physiology, and Dr. H. W. Rand, associate professor of zoology, both in Harvard University, support McDougall, stating that the 'ectoplasm' undoubtedly was composed of the lung tissue of some animal. Other biologists and physicians coincide with this judgment.[43]

IV. *Investigation by a Harvard Group.* Four instructors and a graduate student—having been somewhat impressed by the phenomena as seen in the Crandon home—formed at 'Walter's' suggestion a circle for study. They secured the use of a room of the Harvard psychological department. The University authorities gave Hoagland permission, if the results should constitite a substantial contribution to knowledge, to submit them and their discussion as a Ph.D. thesis.[44]

A number of professors and physicians were invited to assist, their attendance being limited by Dr. Crandon's requirement that but one or two should be admitted at a time. For six sittings, May 19 to June 24, 1925, no trickery was discovered and the majority of the sitters seem to have become impressed by phenomena. Several reports of the sittings, against the protest of the investigators, appeared in print. The last declared: "It is a joy to observe this study being made by honest men with open minds."[45] But on June 29th and 30th a number of things happened.

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[38] Mr. Bird says he had no doubts of genuineness left after his 12th sitting (*M.M.*, 438) That was Apr. 28, 1924 (*M.M.*, 88ff.). Yet in *S.A.* of Aug. 1924, writing after he had been present at 22 more sittings, he had said: "Manifestations have not yet occurred in our presence under full test conditions."

[39] *M.M.*, 149.

[40] *J.A.S.P.R.*, June, 1925.

[41] *J.A.S.P.R.*, April, 1925, 198.

[42] This photograph has never been published, although evidentially valueless ones have appeared (as those of "spirit" hands, opposite p. 480 of *M.M.*, which might or might not be from the plaster casts shown opposite p. 340).

[43] *N. Y. Times*, Feb. 28, 1926; Mar. 14, 1926.

[44] Statements by members of the group on file.

[45] *Banner of Life*, June 13, 1925, 20, 27; July 4.

Mr. Hoagland, as spokesman of the Harvard group, published a report in *The Atlantic Monthly* of November. Dr. Crandon protested that this was in violation of agreement. On eight grounds appearing in the documents I think this contention unjustified,[46] but, after all, the nature of the facts is not affected by the formal question whether they should have been suppressed. Following is a digest of the principal features in the report.

The great phenomenon was an "ectoplasmic limb" and its acts; but the ectoplasm differed in shape, consistency, reach and many other particulars from that of the Dingwall period.

The report alleges more than twenty items of evidence (some complex), direct and collateral, leading to a verdict of normal production.

Witnesses on the 29th saw a luminous anklet upon the floor; the claim that it fell off because too large was disproved;[47] 'Walter' denied but at the next sitting admitted that it had been off;[48] and it was surreptitiously replaced. Hoagland distinctly saw the silhouette of a foot, its toes holding the luminous disc, and he traced the form of the leg to the knee.[49] The two discoveries gave the clues by which under the same conditions Code was able to reproduce the performances to the entire satisfaction of previous witnesses.

The plasticine, after the 'ectoplasm' had pressed it, showed skin marks, lint microscopically identical with that in the medium's slipper, and also inclusions of sand and an insect skeleton such as would be found upon the floor.[50]

The ankle-bands being firmly secured on the 30th, the leg was out of question, but the controllers (Professor Shapely and Mr. Code) observed the medium work both hands free from control, and one of them detected her conveying objects from the region of her lap and afterward returning them.[51] Internal search of the medium has never been permitted.

The 'ectoplasm' of the 30th differed from that of the 29th by numerous characteristics. Its movements correlated with those of the luminous armlet. Various signs indicated a mechanical contrivance worked by hand. The plasticine received an imprint like that of a chain, but no skin-marks, though there were three deep impressions resembling those which cloth-covered fingers would make. What the feet could not do this evening the hands could do, as Code's duplication under like conditions proved. Thus both on the 29th and 30th the correct scientific method was employed; observation followed by hypothesis, hypothesis followed by verifying check.

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[46] There is space but to allude to the principal of these, viz: that Dr. Crandon formally assented in May to Mr. Hoagland's plan to use the results, if suitable, for a Ph.D. thesis; that no intention finally to suppress them was ever broached; that Dr. Crandon allowed, against protest, data about the sittings to be printed in four issues of the *Banner of Life*; that solely for defense of himself and colleagues against premature gossip Professor Shapley on June 29th stipulated that names should not be given out without specific authorization and that attempts should be made to avoid publicity; that this agreement was not signed by Dr. Crandon and was binding only on those who did sign it; that nine members of the group did authorize giving out a report after the sittings ended; that Dr. Crandon wrote a letter on July 6th saying that he agreed [to Code] not to publish the Harvard sittings except with the consent of the majority of the signers, yet without obtaining such consent he furnished the notes from which a garbled account appeared in several issues of the *Boston Herald*, before the report by Hoagland came out in *Atlantic Monthly*.

[47] The anklets used this evening, for the fourth time, were pinned by the medium to the circumference of her own, were tight enough to leave skin marks, and had the safety pins still in place when they were taken off.

[48] *M. H. V.*, 73, 78.

[49] Hoagland's addendum, expunged from p. 77 of *M. H. V.*

[50] The writer examined the plasticine imprints and found the lint, etc., stamped into the clay, which was clean outside the periphery of the impressions.

[51] Particulars in this paragraph and the next to be found in sitting notes omitted by *M. H. V.* See dots occuring before the signature, *M. H. V.*, 82.

The above, with many other focussing collateral details, left no perplexities in the minds of the Harvard group, including the woman physician whom the Crandon pamphlet attempts to make appear a witness in Margery's favor.[52]

Hoagland's report stresses, at least provisionally, the theory that the trickery is largely automatic, subconscious. But the incorporated argument by Code to that effect can be applied in most of its parts to every professional medium, including those known to purchase their apparatus and to consult with each other as to methodology.

Certain acts and expressions of Code which have been criticised appear to have been effects of friendship for the Crandon family and of his desire to shield it so far as possible, but they only emphasize his reluctant testimony as to the vital facts, which, by the way, do not rest upon his witness alone.

Subsequently, to correct false rumors in the newspapers that they were not agreed, all the members of the Harvard group who could be reached at the time signed and printed a statement that "the group is in absolute agreement that the only conclusion possible to them is that trickery accounted for all the phenomena; that the only possible difference of opinion in the group is to what extent the trickery was unconscious." The signatories were: Harlow Shapley (director of Harvard Astronomical Observatory), S. B. Wolbach, (professor of pathology in Harvard Medical School), Edwin G. Boring (director of Harvard Psychological Laboratory), Hilbert F. Day (surgeon), Deborah Fawcett (physician), Hudson Hoagland (engineer and now Harvard assistant in psychology), S. Foster Damon, Robert Hillyer and John Marshall (all three Harvard instructors in English).[53]

The anonymous pamphlet *Margery-Harvard-Veritas*, lately issued by Dr. Crandon and his friends, attempts to discredit the judgment of all sixteen persons composing three accepted tribunals except Mr. Carrington, and also claims to present the entire notes of the Harvard sittings, "absolutely as they were written and signed."

As regards the first factor, the contradictions, perversions and suppressions of established data are so numerous and similar to those in *M.M.* as to suggest a common hand. It is asserted that after a rule was found in the box Houdini went into "a state of prostration" and exclaimed, "I am willing to forget this if you are," although everyone present knows that the prostration is a myth, and that the words were uttered after 'Walter' apologized for a profane and vulgar sentence which had issued from the region of the medium's mouth.[54] "Dr. Prince . . . declares he does not know whether a bell that lies in his own lap rings or not,"[55] is the twist put upon the incident given above (p. 433) and correctly reported to Dr. Crandon in writing. *M.H.V.* asserts that "there was no luminous doughnut" on June 30th, although pages 80-81 show its use; that "they brought no apparatus except the bell-box" on that date,[56] though various pieces of apparatus are named in the notes printed on the same pages; that the plasticine balls were on June 29th "all piled in a bag" unidentified and "made off with" by Dr. Wolbach,[57] whereas they were carefully marked and placed in boxes. It is asked if the use of the 12-inch anklets on the seventh sitting was "to form a way of escape for the honorable investigators,"[58] although the same anklets had been used at previous sittings.

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[52] *M.H.V.*, 27; see note 61.

[53] "Excessively young men" (*M.H.V.* 83). Their ages range upwards to 40, and the youngest is older than Pitt when he became Chancellor of the Exchequer, Hamilton when made the valued aide of Washington, Huxley when already engaged in the researches which made him a Fellow of the Royal Society, and Crookes when he had begun his important discoveries.

[54] *M.H.V.*, 12. [55] *M.H.V.*, 15. [56] *M.H.V.*, 25. [57] *M.H.V.*, 24. [58] *M.H.V.*, 32.

Secondly, the exact assurance of *M.H.V.* is: "We append herewith *the official notes* of the Harvard sittings. They appear *absolutely as written and signed*" (italics mine).[59] This statement proves, on comparison, to be triply untrue:

(1) *Changes.* The prefatory paragraph on page 40 of *M.H.V.* has been altered, and "Dr. Crandon" or "Crandon" changed more than a hundred times to "F. H." —excusable alterations but for the denial of any changes. There are other alterations, unimportant, but yet alterations.

(2) *Additions.* A paragraph of 108 words on page 82, declaring that in "our minds" is "no doubt that this mediumship is one of the most important ever recorded," masquerades as part of the sitting-notes.

(3) *Subtractions.* Two recorded vulgarisims of 'Walter' are missing from page 40, and a passage of 49 words, containing an especially offensive utterance, has been expunged from page 42. All "damns," references to the Deity and Hades are carefully omitted from pages 43, 45, 47, 61, 62, 66, 70, 72 and 78.

(4) *Omissions.* Now we come to prime offenses, the omission of notes of crucial importance. Although various addenda of the earlier and uncertain stage of proceedings are properly printed, including one written twelve days after the sitting,[60] Mr. Hoagland's addendum of 210 words relating to his discovery of the leg, made during the sitting and announced later the same evening, is entirely expunged, although regularly incorporated with the original official notes and typed on the same page with the matter at the top of page 77! Also, the final 4 1/2 pages of June 30th, partly composed of dictaphone notes of the same evening and partly of addenda supplied by members of the group, embodying their conclusions and in particular vitally important facts of that evening, such as the description by two observers of how the psychic freed all of one hand and the fingers of the other, the use she made of them, the peculiarities of the plasticine prints and other particulars, are completely obliterated, making it appear that all such statements were after-thoughts! Altogether I find that about 1440 words have been stricken out of the official notes of the sittings, although parts of this material, secured from Dr. Crandon, had been printed in the Boston Herald before the *Atlantic* report appeared. On page 82 of the *M.H.V.* version itself the tell-tale dots appear, signifying omission of notes "as written and signed." And on page 27 recourse is had to some of the material, "Notes, June 30, p. 9," which is expunged from the pamphlet, giving the very page of the typed manuscript on which it was found.[61]

(5) *Misdirections.* Italicizing passages nullified by discoveries made after the passages were dictated in good but mistaken faith is as bad as altering the text, since it equally misdirects readers. The most of these passages consist of expressions like "Control O. K." Yet the sponsors for the pamphlet well know that after it was learned that a luminous band could be removed and replaced without detection, and while off still appear in the darkness to be on unless the medium forgot the precaution of partly obscuring it, every previous assurance as to the foot had lost its force. The most flagrant instances of misdirection are where passages similar to "feet and hands visible" are italicized, for those responsible for them know that the luminous bands shed no light on nearby objects and that actually the hands and feet were not visible. The expression is a colloquial one, metonymy for the bands then supposed to mark the exact position of hands and feet. Psychic research is not a game of words but an inquiry as to the facts.

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[59] *M H.V.*, 37. [60] *M.H.V.*, 59.

[61] Nor does the paragraph correctly represent Dr. Fawcett's testimony. She did not say that "hiding was catamenially impossible" nor anything which necessarily implies that.

Italicized dictations, such as "seems to have very crude fingers," "in general the visible operation of the hand seemed clumsy and awkward," "ill-formed hand," are exactly what the discoverer of the foot in action would desire the reader to attend to.[62]

Those who have the information which explains certain other passages italicize them, apparently, to confuse readers who lack that information. "Foot control checked at Code's request by having each foot move separately in a different direction."[63] As though the free anklet could not be shoved along the floor by the toes! The reader may well be puzzled by the note on page 76 saying that the investigators were to blame for the bands coming off. They at the moment trusted the psychic's statement that the bands were loose—not until afterward did they find that she had pinned the anklets to exactly the dimensions of her own impeccable ones. They should have measured at once, but I have yet to meet the investigator who never forgot anything he should have done. But there are few passages which should puzzle the reader, unless he intuitively has more knowledge than the actual observers who were convinced of particular frauds. In the light of those discoveries many otherwise perplexing notes become not only clear but buttress the discoveries themselves.

The pamphlet puts various questions of such quality as, "If the garter fell off why didn't Code pick it up?"[64] It seems to be held both that the garter did not come off and that it came off because too loose. But it appears that Code was busily engaged in running the dictaphone at the time and, moreover, the rules definitely forbade his doing what is suggested. The garter was off, and this fact was afterward admitted by Walter; did it 'fall' on again? Why, it is asked,[65] did not Hoagland proclaim his discovery of the leg at the moment? And precipitate a wrangle as Houdini did when he proclaimed fraud, and afterwards have it said that he ought to have been thrown out! Of all cases this seems to be the one where an investigator, whether he does a thing or its opposite, is most certain to be asked why he did not do the other thing.

Dr. Crandon continues with extraordinary perseverance to put forward his medium and to attack the lengthening line of accepted and unconvinced investigators.[66] Beyond all other American cases, the scientific investigation of this had been hampered by traditional ritual, "psychic laws" to be taken for granted and attempts to impose arbitrary and irrational rules. So far as investigators consented to such rules they are denounced for the acts or omissions to act resulting from the rules, and so far as any of them declined to be bound by novel and artificial conventions they are denounced for their contumacy. On the other hand there has probably never been a similar American case which has been tried so open-mindedly and even hopefully, so patiently and fairly. Nor, my acquaintance with the documents compels me to add, have any adverse decisions probably been rendered with such extreme forbearance and economy in the use of pertinent material.

The uncommon hold which the Margery case retains upon public attention is, I think, due to several reasons:

(1) The ordinary difficulties of guessing in the dark under traditional conditions aided by a set of regulations, arbitrary beyond precedent.

(2) Uncommon cleverness on the part of the medium, both innate and consciously or subconsciously acquired.

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[62] *M.H.V.*, 54, 56, 62, 63, 67, 69, 71, 73.

[63] *M.H.V.*, 71. [64] *M.H.V.*, 23. [65] *M.H.V.*,23.

[66] *M.H.V.* indeed sets against the verdicts of the three official commissions the confidence of "three hundred" persons of unknown qualifications who are said to entertain no suspicions (*M.H.V.*, 4.). But so could Cagliostro, Slade and the Davenport Brothers, and so can living and demonstrated fraudulent mediums, point to their hundreds or thousands of believers.

(3) Superior technique in accordance with the psychology of deception; exchanging one type of phenomena for another before the study of the first is complete, and devising new types after a verdict has been rendered, so that the medium always keeps one step ahead.[67]

(4) The exclusion of investigators soon after they begin to express doubts and reasons therefor, thereby limiting the number able to declare actual discovery of fraud.

(5) The personal charm of the medium, winning emotional advocacy, especially among men.

(6) A persistent propaganda, persuading many by its very vehemence that its allegations must be true.

(7) Paucity of response from the opposition, to which the indignities of this kind of controversy are distasteful.

(8) The indifference of the masses to dry logic and analysis, and their instinctive gravitation toward the highly sensational.

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[67] To no scientific commission which has yet reported have the "voice machine" and glass cabinet been submitted. I am therefore debarred by the plan of this article from discussing their modes of operation.

BOOK REVIEWS

The Psychology of Human Society. By CHARLES A. ELLWOOD. New York, D. Appleton and Co., 1925, pp. xv, 479.

Written for use as a text, this book represents the maturation of the author's social theory. Its thesis is that the most adequate explanation of social phenomena is to be derived from the "study of the psychic processes involved in the origin, development, structure and functioning of social groups" (p.16.) This "psychological view of the social life, as essentially a collective behavior process mediated by conscious interstimulation and response, furnishes therefore a basis for the synthesis of other theories, and so becomes itself the sociological view" (p. 466). In regard to method, the author says "A complex science such as sociology demands, for a complete and adequate scientific method, a synthesis of the results of deduction from the principles of antecedent sciences with the facts secured through the inductive study of social life by means of anthropology, history, observation and statistics" (p. 35). It is hardly necessary to point out the difficult position of a science which bases its theory so widely. Even a purely descriptive science would have great difficulty in synthesizing such materials, but where in addition the discipline also seeks to set standards and actually control its subject matter, *i.e.* the behavior processes of society, the obstacles are multiplied a thousand-fold. The purpose of sociology, according to Ellwood, is to determine what social progress is, and to devise the most advantageous means of attaining it. Thus it is a combination of explanatory science and ethical technology. Education is accepted as the proper tool for manipulating society's opinions, standards and behavior; and the interesting statement is made that we may readily prevent the over-development of material civilization at the expense of spiritual culture "by devoting more energy and attention to the spiritual side of our civilization" (p. 450).

The psycho-behavioristic approach is, in the main, general and quite sound. The discussions of instinct, imitation and feeling, and their relation to group life are excellent summaries of the recent theories on these subjects. Particular stress is laid on the modifiability of human behavior, and a long discussion is given the part played by intelligence in group life. The neurological speculations, which are dragged in, are not relevant nor always clear as is well illustrated by the following sentence: "If we include in intelligence the physiological processes of the cortex associated with it, it is surely entitled to be regarded as a phase of behavior and as much a force as feeling and desire" (p. 312). When intelligence is called "a problem-solving ability," one is on fairly safe ground, but to add that it was "developed by the organism when the ready-made reactions of instinct and habit no longer suffice to meet the situation" (p. 313), is teleological to say the least. The fact that intelligence is an adaptive agent does not warrant the conclusion that it was created for purposes of adaptation.

From the position of a layman, it is of value to consider the *a priori* bases of this applied sociology. Briefly, they may be stated as follows: Viewed scientifically, there is such a thing as social progress, and it may be defined as the "increasing of men's capacity to survive, the increasing of their efficiency of achievement, and the increasing harmony of their relations with one another" (p. 424). "The purpose and goal of society is the progressive realization of a perfect society consisting of all humanity" (p. 477). Through the rationalization of social institutions, progress can be effected and science is the method of producing this rationalization.

Although our author evidently identifies "rationality" with social adaptation, he also uses it in reference to the critical attitude toward truth produced by the growth of science. Thus, he admits that the rationality of an institution can be tested only by the experience of generations and that it would be impossible to determine directly whether it is adapted to human needs. Yet he goes on to say that "the influence of the scientific attitude will ultimately rationalize all phases of human social life," and that by the progress of science we mean that rationality (*i.e.* critical reason) shall permeate art, morality, religion, government and the family life. The desirability of this is at least susceptible to doubt; and it is not a tremendous assumption to identify the scientific attitude with the social attitude, and to suppose that the increase of scientific knowledge assures a better society. Reason is a neutral and just as frequently destroys adequate social adjustments as it aids in their creation. In fact, our author tells us that "the most intellectual people of antiquity, the Greeks, seem to have had little practical social genius, as their social life was characterized from early times by social disunity and disharmony, and at length by corruption and degeneracy" (p. 330). At this juncture, not only do we wonder what "practical social genius" is, but are tempted to ask how it has been "scientifically" demonstrated that the indefinite continuation of a standardized group would be more desirable than a supreme, if temporary, outburst of culture such as that of Greece.

As regards social progress, Ellwood says that it is the business of the sociologist and the social psychologist to favor reforms, such as: the removing of social and economic inequalities which hamper the normal development of the individual; the minimum wage for a "human standard of living;" legislation for protecting the worker against accident, disease and too long hours of labor; in short, the abolishment of industrial poverty. No attention however, is given to the problem of over-population.

As an illustration of the influence of knowledge on social progress Ellwood cites the prohibition movement. He says: "through the accumulation and diffusion of knowledge regarding the physiological and social effects of alcohol, and through the growth of humanitarian sentiments, a great movement has arisen in the more progressive societies of Western civilization which seems about to sweep away the use, if not of all, at least of some of the stronger alcoholic beverages. Let us note, however, that this movement has been successful only to the extent that standards and habits have been inculcated in the group through the imparting of scientific knowledge and the cultivation of humanitarian sentiments" (p. 445). A most surprising illustration, and one which, as every man of science knows, hardly squares with the facts.

No one doubts that there are scientific facts concerning society, hence facts for sociology, but would it not be profitable for writers of textbooks to differentiate between "scientifically established principles" and "social opinion," particularly when that social opinion proposes to mold society!

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Conservatism, Radicalism and Scientific Method. By A. B. WOLFE. New York, The Macmillan Company, 1923, pp. xi, 354.

The author, finding students unable to consider problems in an objective scientific way (because of the stubborn intrusion of emotional and sentimental factors), prepared this book on scientific method with a view of correcting these habits of biased outlook. The book is badly proportioned. It is too large, because of much unnecessary elaboration; and, yet, withal points which are essential to the logical structure of the work are too scantily treated. The consequence is the author because of his brevity on the one hand and verbosity on the other frequently contradicts himself. For example: he commits himself to behaviorism of that liberal sort which does not reject consciousness, and then on later pages proposes such things as the explanation of 'spiritual' unrest in physiological terms—then in the same breath acknowledges the dualism of restlessness due to an overactive thyroid, and that to a 'conviction of sin.'

The definitions of basic concepts occupy only four pages (pp. 8-11). Sentiment and attitude are hastily defined after citing only two scientists—Warren and Shand. Attitude is defined as "how we respond," yet he speaks of it, not as the response itself, but as a mental attitude (p.12). This confusion of attitude, first as act, then as mental set, is characteristic of the book.

Conservatism is defined as a mental attitude causing an individual to accept things as they are. Radicalism is for a new order of things. These are logical rather than real classes. The worst radical will be conservative in some things. There are varieties and sub-varieties. "Interested conservatism is motivated by narrowly selfish, egotistical, individual or class interests. Disinterested conservatism is motivated by fear, habit and similar 'inertial' factors. That is, though they may be selfish, it is an uncalculating selfishness. This is in effect to deny that conservatism is ever motivated by unselfish impulses—preposterous and untenable in the opinion of the reviewer.

In a chapter on disinterested conservatism we are told that "all conservatism is a safety first attitude, that "conservatism and habit are roughly synonymous" (p. 23). But he now seems to neglect the selfish conservatives who are deliberately and consciously defenders of the existing state. Unless he is willing to go the full length of Watsonian behaviorism, he cannot say that conservatism (if it is a mental attitude) is "roughly identical with habit," but his rejection of Watsonian behaviorism for a liberal sort has already been mentioned. Fear in a social situation is "always essentially a defense mechanism or state" (p. 25). "The animal fears for his life, man fears for his habits." The author forgets that in a large part of our life we fear, not that our habits will be broken, but rather that they will not be broken. Surely, the radical may be afraid that the existing state cannot be overthrown.

It is sheer assertion, out of all touch with historical fact, to say that radicalism is always the outgrowth of personal discomfort. What a warped judgment this leads us to make regarding those Divine radicals, the martyrs of history! But, with his usual inconsistency, our author now modifies this extreme view in a footnote apology to Victor Yarros who has taken him to task for exaggeration. Yarros had suggested that opinions about radicals should be based on inductive study. "There can be no question," says Wolfe, "that the suggested inductive study should be made." Then, with that blissful amnesia which easy-chair empiricists enjoy, Wolfe promptly forgets this precept, preferring a picture of radicalism to suit his preconception to the effect that the motives of radicals are essentially personal and selfish.

The final chapters on scientific method are elementary discussions of scientific method in general. These chapters constitute the constructive part of the book. The reviewer, however, misses in them a discussion of an issue which the book raises. Should such effective factors as emotion, sentiment, wish, be allowed to play no part whatever in forming our social and economic beliefs? If not, why not? In short, if social science is at all normative, if it involves the problem as to what *ought to be*, can this 'oughtness' be intelligently determined by pure reason alone, shorn of the last shred of wish and desire and prejudice?

Wells College C. O. Weber

Anfänge der Reifezeit. Ein Knabentagebuch in psychologischer Bearbeitung. By Dr. William Stern. Leipzig, Quelle and Meyer, 1925, pp. 125.

This interesting volume is the first of a series planned by the author dealing with the psychology of adolescence. It is a psychological treatment of those portions of a boy's diary written during the period of transition from childhood to adolescence, and the early part of the adolescent period, *i.e.* from the middle of the twelfth year to the close of the fifteenth. Through the analytical study and interpretation of the entries in the diary the author organizes his materials according to their psychological significance, traces the development of adolescent characteristics and interests, and indicates the growth from the restraints of the transition period toward the larger individuality and independence of youth. Quotations from the diary given throughout the book, supply concrete examples upon which the general discussions and interpretations are based.

The author first points out the restraints of the transition period, the range of interests manifested, and then traces the gradual development of self-consideration and self-judgment, the consciousness of growing manhood, and independence, and the realization of sex. In the succeeding four chapters the development of more specific characteristics is given attention: (1) Interest in other human beings as, interest in the observation of people in general, relations with parents and relatives, the forming of friendships and the opposite, and the development of interest in the other sex. (2) Intellectual interests, pedagogical interests both as student and tutor; interests in politics and culture, and in nature. (3) Specific intellectual interests in music, the plastic arts, literature, original composition and religion.

The book is very suggestive as indicating a technique for the psychological study of adolescence based upon diary materials where the student is more likely to find spontaneity than in other forms of written expression. As the writer admits, however, this method of study is limited to that type of youth who makes use of the diary to represent himself as a personality. For the great majority of youth with whom the urge for self-expression does not take this form, it is not directly applicable. The method will have much of value probably, in spite of its limitations, to the whole problem of adolescence; and this little volume of Dr. Stern's will undoubtedly be found useful and suggestive by all students of this phase of psychology.

Cornell University E. N. Ferriss

Determinism in Education. By William Chandler Bagley. Warwick and York, Inc., Baltimore, 1925, pp. 194.

In this little volume of eight chapters Bagley has assembled, with slight modifications and additions, seven papers thrown by him into the unending controversy over the relative influence of nature and of nurture upon the 'intelligence' of man. Several of the papers have attracted wide attention and provoked much discussion—notably the one on "Democracy and the I. Q." and the other on "The Army Tests and the Pro-Nordic Propaganda."

The two chapters devoted to the papers named are, by all odds, the most significant of the volume. In the first the author sets up a fairly lifelike 'straw man,' which he then proceeds to rend and scatter, yet does not wholly demolish. In the second he attacks with great vigor and cogency the assumptions, the method, and the conclusions of Carl C. Brigham in his work, *A Study of American Intelligence.* In this attack, to a confessedly sympathetic reader, he appears to be entirely successful. The demolition is complete; and in the demolition Bagley has done much to stimulate a healthy repercussion against lack of science in use of the only 'science' with which the study of education is said to be furnished, the "science of mental measurement."

But though his forensics are often admirable Bagley's own assumptions, methods, and conclusions are often not scientific. He confesses a 'bias,' and shows it on every page. Frequently he assumes the sort of thing, sometimes the very thing, which he condemns his opponents for assuming. He adopts methods that he disapproves for his opponents, and he builds large generalizations upon evidence as scanty and shaky as that upon which, he charges, his adversaries have constructed the fabric of their "pessimistic determinism." His least controversial and most constructive chapter, that on "Moral Ratings in the Several States," is a first class example of the weakness in assumption and method that he deprecates in others. The chapter on "Education a Creative and Equalizing Force" is also a good example of the chief logical fallacy which he attributes to the 'determinists.' Indeed, to quote his own criticism of Brigham, sometimes it seems that if the "assumptions and methods are valid, the actual facts . . . prove just the reverse of the contention he has set forth."

For the 'pessimistic determinism' which he refutes Bagley would substitute an optimistic determinism. In that philosophy 'systematic schooling' becomes preponderant over 'native intelligence,' the school-master dominates the germ plasm. Since man of the now can undoubtedly do a better job for man of tomorrow than can an alogical and biologic nature, there is hope for democracy. Hence educational determinism is not determinism at all but an optimistic yet 'rational equalitarianism."

Despite the fact that the author appears much more effective in destructive than in constructive argument the book leaves a wholesome impression. Like all of Bagley's books it is thoroughly readable, and in style far above the level of most compositions in the field of education. It reveals continually the quick insight and apt striking power, as well as the faith in democracy and the common man, for which the author is so well and so favorably known. It is having already an influence on the evolution (*absit omen*) of the theory and practice of education.

Cornell University T. H. EATON

Outline of General Psychology. By R. H. GAULT and D. T. HOWARD. New York, Longmans, Green and Co., 1925, pp. iv, 474.

The authors of this book state in their preface that they hope to appeal to two classes of readers, "to undergraduate students on the one hand, and on the other, to the body of intelligent readers outside of academic circles." We should expect to find, then, not necessarily any contribution to the science of psychology in the way of fact or system but instead a presentation of the familiar material from an angle that will interest such readers. The first four chapters are concerned not at all with the facts of psychology but with those about the organism with which the novice must become acquainted before he can compass adequately the inquiry into the nature and functions of the experience of that organism.

The chapter on attention may be taken as a key to the general treatment of the subject matter. Attention is a selective process. It is not, however, a special process but only "an aspect of the mental process." "To be conscious is to discriminate . . . in a word, to select." The mental and motor aspects of attention and the range, duration, etc., include the usual facts. The 'forms of attention' are extended to cover 'sensorial' and 'intellectual' attention though the only distinction seems to depend upon whether the object is sensory or imaginal. Voluntary attention is given over to 'will,' 'the whips of conscience,' 'the spurs of good or evil prospects,' without any attempt to reduce this 'will' factor to simpler terms. The function of attention for the organism receives its full share of discussion.

It is "to further the ends of human life;" "it is anticipatory—a problem seeking activity;" "it is organic response and adjustment;" and on the physiological side it is "an equilibrium-maintaining process." In the same manner in perception, imagination, memory, thought, emotion, instinct and action psychological, physiological facts are considered with emphasis upon the part that they play in the life of the organism. Such a treatment was forecast in the statement of the aim of the book, and though it will meet with objections from the more strenuous proponents of the several 'isms' it has been carried through consistently. It is disappointing, however, that the facts, etc., taken over from the various schools have not been presented in many cases with their original significance, *e.g.* experimental 'introspection' remains a 'looking within;' Titchener's attentive clearness becomes cognitive clearness. Perhaps these distortions are the natural outcome of the functional-philosophical attitude.

The novel departure and a highly commendable one is the chapter on the history of psychology. Other authors might place the emphasis on different periods but the presence of an historical setting would improve most modern books on psychology. Perhaps due to habituation, one is disappointed to find no references at the ends of the chapters before he finally discovers them at the end of the book. On the other hand it is very pleasant to meet again with authors who will take the time and trouble to head their chapters with appropriate quotations.

Hobart College FORREST L. DIMMICK

Education as the Psychologist Sees It. By W. B. PILLSBURY. New York, The Macmillan Co., 1925, pp. ix, 342.

The volume affords a concise survey of the field of educational psychology. Dispensing with the usual introduction of elementary neurology, the author briefly discusses individual differences and their instinctive basis and then undertakes a consideration of habits, attention, learning, emotion, will, fatigue, and formal discipline. A short chapter has been appended, apparently as an afterthought, on "Tests of Accomplishment in School Subjects."

The chapter on "The Ends of Education" (Chapter VI) serves a helpful purpose in that it presents with clarity what the author conceives to be the aims of education. If there is anything concerning which teachers are uncertain, it is the objectives of their own profession. A discussion which serves to dispel such vagueness is worthy of attention.

The author emphasizes the selective function of education. He says in part (p. 83): "To gather together even half of the men of the upper five per cent of the intelligence of the nation in some two hundred or more institutions and know where to look for them when they are needed is in itself a highly important function. That alone would be worth to the nation what is spent upon the universities, were these men not improved at all by the time spent in the institutions."

The illustrations used in the text are familiar ones from Darwin, Swift, Thorndike and others, and are not in every instance attributed to their sources. At the end of each chapter is appended a list of questions, which are more or less thought-provoking and useful. The references have been selected with great care and their number limited with what the beginning student will no doubt acclaim as praiseworthy restraint.

The frequent application of theory to school problems gives the book a somewhat unusual practical value. It should therefore appeal to the teacher and prove useful as an introductory text.

Harvard University C. E. LAUTERBACH

The Theory of Ethics. By Arthur Kenyon Rogers. New York, The Macmillan Co., 1922, pp. 197.

Ethical theorizing, according to Professor Rogers, does not supplant experimental groping toward a satisfying life but it should decrease the blindness of the groping. Guidance of conduct is possible because feelings of satisfaction are indications that we are heading the right way. If when we consider an end we have a feeling of satisfaction, then we must judge by the only criterion we have that the end in question is a valuable one; and if our satisfaction continues while we pursue the end, then our judgment has been a valid one. The 'ought' distinguishing moral value arises when in addition to a feeling of satisfaction there is a feeling of repugnance for a rejected alternative. Apparently the primary factor in such repugnance is our perception that one alternative represents a judgment that is inadequate to the realities of the situation. The account of the genesis of the 'ought' is the weakest point in the book, which contains much keen analysis.

University of Chicago C. M. Perry

Reason and Morals. By Israel Levine. Glasgow. Macklehose, Jackson & Co., 1924, pp. xi, 177.

Psychology, through the insight covered by voluntarism, Freudianism, and behaviorism, has rendered necessary some re-casting of ethics. The point of departure chosen for the present venture is the 'reality-principle,' as used by Freud. Levine seeks to show that in general the moral is to be identified with the reasonable. Reason is the instrument through which such conditions of real life as lie at the base of our coöperative enterprise are discerned and utilized. Through the coördination of the pleasure-principle with objective social demands Freud's insight is capitalized; and through the emphasis upon reason as the medium of adaptation contact is kept with the idealistic tradition in ethics. It is not probable that all the relatives of either contracting party will be entirely pleased with the union; but the end proposed is valid and the analysis is well sustained throughout.

University of Chicago T. V. Smith

The Problem of Immortality. By R. A. Tsanoff. New York, Macmillan Co., 1924, pp. 418.

The book is a scholarly treatment of the problem of immortality from the standpoint of the philosopher. He traces the history of the problem from primative thought in its various philosophical and religious phases through the modern period. The book is essentially a philosophical treatment and may, therefore, be disappointing to one primarily interested in the psychology of the belief in immortality. The last chapter of the work consists of an able discussion of the philosophy of value, personality and destiny in a modern metaphysical setting.

Harvard University Hudson Hoagland

The Unconscious: An Introduction to Freudian Psychology. By Israel Levine. New York, The Macmillan Co., 1923, pp. 209.

Within the covers of this little book the author makes the modest attempt to discuss the pre-Freudian treatment of the unconscious by Leibnitz, Schopenhauer, Maine de Brian, Hartmann, Fechner, Nietzsche, and Butler; to outline Freudian doctrine; to present a defense of the unconscious as a psychological hypothesis and of the necessity for writing a psychology in purely psychological (non-physiological) terms; and, in addition to all this, to describe the implications ot psychoanalysis for education, crowd psychology, ethics, aesthetics, and philosophy, with general emphasis on the light cast by psychoanalysis on all fields of human endeavor and interest.

Although the author emphasizes the contrast between Freud's scientific conceptions and the metaphysical character of all previous discussion of the subject of the unconscious, he has thought it unnecessary to adduce any evidence of the scientific nature of Freud's work. He assumes the validity of all the Freudian concepts in the discussion and considers any question settled when he has found an appropriate quotation from Freud.

Harvard University E. C. Gilbert

Psychoanalysis: Its Theories and Practical Application. By A. A. Brill. Philadelphia, W. B. Saunders Company, 3d ed., 1922, pp. 468.

If one wishes to know what psychoanalysis is about and seeks to learn the Freudian principles and practices, set forth within the compass of a single volume, Brill's book very well suits the demands. This edition, the third, has been thoroughly revised and enlarged. Brill feels that despite the tremendous success of psychoanalysis, and "disregarding the foolish ranting hurled at psychoanalysis now and then by ignorant individuals," there is too much misunderstanding among those who take the subject seriously. To correct this 'grave' situation he has added two new chapters, one on 'Homosexuality' and the other on 'Paraphrenia.' Many new cases and illustrations are added to make clearer the precise applications of the Freudian theories.

Cornell University F. Lovell Bixby

How Much English Grammar? By Martin J. Stormzand and M. V. O'Shea. Baltimore, Warwick and York, Inc., 1924, pp. 224.

If we would keep our textbooks fresh and virile, we must, in the opinion of the reviewer, from time to time take stock of the actual needs in the fields concerned. For in certain instances, perhaps, there is too little relation between textbooks, classroom instruction, and current practice. But, on the other hand, we must not drift so far with the current as to lose the richness of the past.

The authors have undertaken a comparatively exhaustive inquiry with a view to learning what grammatical constructions are commonly used by American students and adults in their written expression. Ten thousand sentences were selected from material of certain grades of current usage. Upon the basis of the frequency of usage and frequency of error, as indicated by this investigation, this little volume points out in detail what should be emphasized and what may safety be eliminated in English grammar today.

The form of presentation is simple, condensed, and intelligible; and the book should be of considerable value and interest to those who are concerned in planning or teaching language and grammar courses in elementary and high schools.

Cornell University H. D. Powers

The Intelligence of College Students. By Andrew Hamilton MacPhail. Baltimore, Warwick and York, 1924, pp. 176.

This book presents a survey of present methods of selecting college freshmen and tests their predictive value as found in a large number of colleges and universities and at Brown University in particular. Numerous facts and conclusions valuable to the college administrator are found in the book. A bibliography of 294 references is given at the close for those who wish to pursue the subject further.

Cornell University J. P. Guilford

NOTES

DR. DAVIES ON "MECHANISM, MEANING AND TELEOLOGY IN BEHAVIOR"

Dr. Davies, in his article on "Mechanism, meaning and teleology in behavior,"[1] discusses the topics with reference to my own point of view as expressed in my paper, "Behaviorism and behavior."[2] Some of his criticisms are anticipated in my book on human behavior[3] which, however, had not appeared in time to be available to Davies.

Before turning to the details of his criticism I wish to meet some of the general questions which Davies has raised against my materialistic determinism, and against the restrictions which I seem to place upon what he regards as the 'realities' in life and mind. He regrets that I do not "admit to the world of existence . . . anything that falls outside . . . the 'movement continuum'." "One would have thought," he continues, "that questions of reality were sometime in their discussion questions of fact, and it is the question of fact that I find assumed in a good deal of behavioristic discussion" (p. 8). I can see no scientific problem in this quotation. The chemist in his laboratory never asks himself or any one else: Is this which I am observing 'real' or not 'real'? Is it a 'fact' or not a 'fact'? Again when I observe and record an infant's movements it never occurs to me to ask: Are the stimuli which are acting 'real' or are they 'facts'? Even when I record my own responses to a stimulus and then (using my own responses as stimuli) record the responses to my responses (even to the third or fourth degree) the question of 'factuality' or 'reality' never arises. Of course both the chemist and myself may doubt whether our records or measurements are reliable or whether our sense organs are functioning normally, but we may determine the degree of reliability by the degree of correlation which exists between various individuals making the same observations. Davies' facts and realities are not the observable objects or relations between objects. They are verbal responses characteristic of individuals who have been raised under what we may call a philosophic environment. From the scientific point of view they are merely stimuli produced by movements of the speech mechanisms which may or may not be investigated with respect to their own sensorimotor antecedents. Philosophers have no sense organs which the physicists do not have. They cannot see a 'reality' which the physicists are unable to see. The 'facts' and the 'realities' of the philosophers are merely names for the way in which objects, movements, stimuli, responses, etc., are classified by the philosophers. The scientists may accept or reject this classification, or substitute one of their own, but whatever the classification the objects, movements, stimuli, responses, etc.,remain unchanged.

That, according to Davies, I should logically accept the syllogism: "Everything that is real is physical; secondary qualities, if they cannot be ignored, are real; therefore, secondary qualities are physical" (p. 9), does not disturb me particularly. The syllogism is merely a series of verbal responses for Dr. Davies and a series of visual stimuli for me. The scientific importance of the syllogism depends upon whether (as a verbal stimulus) it leads scientists to discover new properties or new relationships between properties. Having such a simple test I fail to be impressed when Davies informs me that "a very considerable portion of the time and energy of a good many of the leading minds in this country and in England, during the last twenty years, have been given to the discussion of just such questions. In fact, the modern form of the realistic controversy may be said to center upon the status of the secondary qualities" (p. 9).

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[1] *Op. cit.*, this JOURNAL, 37, 1926, 2-24.

[2] *Op. cit.*, *Psychol. Rev.*, 31, 1924, 32-50; 118-149.

[3] *A theoretical basis of human behavior*, 1925.

It occurs to me that for many years philosophers and "a good many leading minds" spent their energies arguing about the number of angels that could stand on the point of a needle. When physicists or chemists discover primary or secondary qualities, or when they accept the classification as a useful working hypothesis, it will be time enough to introduce the syllogism into the study of human behavior. It remains to be seen whether my statement "human behavior is a form of motion" is a more effective stimulus for analyzing out the elements of human behavior and achievement than the stimulus "you must not fail to look for reality" which, it seems to me, represents in a condensed form Davies' caution against extravagant behavioristic methods.

We pass to a consideration of consciousness which Davies maintains is not adequately considered. He says, "Whether 'consciousness' could or could not sustain a rigid examination as to its factual existence is not now being considered; what I am saying is that the importance and even necessity of making such an examination has been obscured by reason of the position that behaviorists and others have taken with respect to questions that have no necessary relation to this one. Mechanism, as I have pointed out, is one of these; the 'movement continuum' is another" (p. 8).

When I am most active in adjusting myself to my environment there seems to be no need for me to 'remain conscious,' to 'apprehend clearly,' to 'develop a consciousness of the *meaning* of what I am doing,' etc. It is only *after* a series of adjustments have been made that I may enter upon an entirely different type of activity, that of *classifying* the movements or partial movements that make up the adjustments. In this I may (a) use the traditional mental classification, or (b) classify the adjustments as forms of movements, reflexes, habits, etc. The two classifications I regard as mutually exclusive and the first one (a) as scientifically unproductive.

Whether mechanism or the 'movement continuum' has any necessary relation to consciousness will of course depend upon what we are to include under the term. That there are many things that might be (and have been) included, I do not deny, but at present I feel no more secure in trying to decide on the content of the term (consciousness) than does Davies himself, who thinks it should be 'dropped altogether.' I have gone somewhat further and 'dropped' all terms of this type, even the 'mental order' which Davies proposes as a substitute for consciousness. Davies, of course, does not approve of this, for he says, "what we mean by an image is the apprehension of a material object when that object is outside the range of our perceptual, that is, receptor-effector system. Does the behaviorist have to be in physical contact with the object in order to describe it or react to it? Or can he describe and react to it in its physical absence? If he can, how is this possible unless he apprehends it in some non-physical fashion?" (p. 8).

The term 'apprehension' in the domain of science can only be a verbal stimulus which releases a series of more or less circumscribed responses. That it releases a different set for every psychologist who is asked to report what he means by the term can only be a justification for dropping it. Its vagueness does not demonstrate the existence of non-physical relationships. It is only when imagery is forced into such categories as 'apprehension' that we get the apparently absurd condition of reacting to a non-physical condition. If I am asked at this moment to describe Mr. X whom I have not seen for five years I believe I can make my description accurate enough so that Mr. X might be selected from a group of say ten persons. The fact that I have not seen Mr. X for five years does not mean that I am now "apprehending him in some non-physical fashion." It only means that I am now reacting to the verbal stimulus, 'describe Mr. X,' in the same way that at one time I did react to Mr. X in the flesh. That I am unable now to fill in every physical and physiological process which has occurred during the last five years, does not require me to assume that it cannot be done or that any non-physical conditions have been operative. Of course I have learned to speak about the association of ideas, the mental processes of recognition and recall, as have other psychologists, but how these ideas, recognitions, recalls, can actually bring about the movements in my fingers by which the description of Mr. X is written, I have not learned. Futhermore, when I try to describe along traditional lines, what I have been taught to call an image or a mental process, my own descriptions are just as variable as those of others. The more critically I observe my own so-called mental states, the more they differ from each other and from those of other psychologists.

We may now consider the specific questions which Davies raises.

Mechanism in Behavior. The following quotations deal directly with the problem. "The advantage of approaching the problems of behavior from the standpoint of biology, rather than from the standpoint of physics, is that we are not thereby committed inevitably to the theory of mechanism. It is true that one of the aims of science is prediction and that predicting implies uniformities of some sort, but this may be granted without begging the question as to the kind of uniformities that are involved in human and animal behavior" (p. 10). "One would have to be very forgetful of the present-day situation of biology to say that the acceptance of a biological point of view commits the psychologist to the position of materialistic determinism" (p. 11). This quotation frankly proposes a limited determinism, which, of course, is indeterminism. Recent statements by biologists, such as C. M. Child[4] and R. S. Lillie,[5] place protoplasmic processes definitely in the series of physico-chemical processes. This would make biological mechanism one with physical mechanism. I would not deny that we may always have a limited *predictability* in human behavior, but this does not mean limited *determinism.* We also have limited predictability in physics and chemistry. Thus the meteorologist is unable to predict the weather accurately, but he does not ascribe his errors to a non-material cause. He merely has not yet isolated and measured the essential *mechanical* causes of the weather. This does not, however, lead him to indeterminism. but actually leads to a more rigid mechanical determinism.

Where it is possible to use statistical methods in biology we have the same type of evidence for materialistic determinism. Thus in vital statistics and life insurance it is possible to predict within a rather narrow range what percentage of the population will die, say, between the ages of forty and fifty; the relative proportions of the deaths from heart, respiratory, or intestinal causes; the number of automobile accidents; etc. If we take such social statistics as school enrollment, theatre or church attendance, number and kinds of books bought, library attendance and kinds of books called for, we again find a degree of predictability which points to mechanical determinism, not to non-mechanical causes. Even such measurements as intelligence tests, educational tests, tests of mechanical or musical ability, all point to mechanical causation when the number of measurements are large enough to establish norms. Of course, since human behavior is more complex than that of the weather or the amoeba, we would expect less accuracy in the prediction of the behavior of man.

The 'be careful' attitude which Davies recommends is, of course, the proper attitude for science; but for science, 'be careful' *now* means, "assume natural (mechanical) causes until compelled to do otherwise." For traditional psychology and philosophy, however, 'be careful' still means, "do *not* assume natural (mechanical) causes *until* there has been a *complete* demonstration that they are adequate." The normal response to such hair-splitting is, "Well, what difference does it make?" As I see it, the difference revolves about the practical effectiveness of a working hypothesis of the 'Thou shalt' type as compared with one of the 'Thou shalt *not*' type. That is to say, actions and investigations which result from the 'Thou shalt' form of stimulus can be measured and bear directly on the problem under investigation; action which follows upon the 'Thou shalt *not*' type of stimulation, while it may be measured is usually irrelevant to the problem being considered. I have adopted the working hypothesis, "human behavior is a form of motion" because it is of the 'Thou shalt' type. I justify this rather premature hypothesis because I think it is time to investigate human behavior and human achievement by methods that will lead to something which can be made the basis of social science. I do not, of course, subscribe to any 'psychology for itself alone' sentiment.

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[4] C. M. Child, *Foundations of Physiology,* 1924.

[5] R. S. Lillie, *Protoplasmic Action and Nervous Action,* 1923.

Psychology should forge ahead of even the biologists in placing human behavior upon a scientific (and this means mechanistic) foundation so that social science may derive from the investigations of human behavior the same sort of support that biology now receives from biochemistry, and biochemistry in turn receives from physics. In this way we have the possibility of a mechanical series ranging from the electrons to the League of Nations. If mechanical monism has made physics, chemistry, biology what they are, perhaps it is worth while trying it in psychology and eliminating even the attenuated dualism which still lingers and which Davies urges us to retain.

Meaning in Behavior. The central problem of meaning as Davies sees it, is 'complication.' "Complication in human experience and behavior is," he says, "only another name for development and, if we so take it, it may not be difficult for all of us to agree that a primary condition of any profitable discussion is that the problem be broken up into a number of constituent questions, and that each be given a careful definition" (p. 12).

When he introduces 'development' as an aspect of human behavior, I think he is touching on a fundamental principle of organization which passes beyond any limited psychological range and participates in all scientific investigation. Grant this, then, is it not better to classify this phase of human behavior with the other scientific fields rather than create a new continent for such an ambiguous term as 'meaning'? As Davies points, out 'development' refers to some form of interaction between the organism and its environment, but even the biological conceptions of the terms are restricted to the field of organic relationships and we have failed to consider the possibility of human behavior as belonging to a wider category which includes all changes, inorganic as well as organic. If the terms organism and environment are extended beyond their biological limits we may construct a series of the following order: Let the term organism include the whole solar system (of which our sun is the center) then the term environment would include all the planets, stars, and everything else beyond the spatial limits of the solar system. If we start with the earth as the organism the rest of the solar system and everything outside of it is the environment. Starting with a human individual as the organism—everything outside of the individual is the environment. Starting with the nervous system of the individual as the organism—the other organs, tissues, fluids, the earth, sun, stars, etc., are the environment. With a single nerve cell as the organism—the other nerve cells, organs, the earth, etc., are the environment. With the nucleus of the nerve cell as the organism—the cytoplasm, protoplasts, cell membrane, axone, dendrites, other nerve cells, etc., are the environment. Using a chromosome of the nucleus of the nerve cell as an organism—the other chromosomes, nucleoplasts, etc., are the environment. Using a chromomere as an organism—the other chromomeres, chromosomes, etc., are the environment. With one of the chemical compounds in the chromomere as an organism—the other compounds, chromomeres, etc., are the environment. With one of the molecules of the chemical compound as the organism—the other molecules, etc., are the environment. With one of the atoms as the organism—the other atoms, molecules, etc., are the environment. With one of the electrons of the atom as the organism, the environment becomes (in a 'house that Jack built order') all the other electrons, atoms, molecules, compounds, chromomeres, chromosomes, nucleus, cytoplasm, cell membrane, nerve cell, nervous system, bodily structures and organs, the human individual, the earth, moon, sun, solar system, stars, and the universe. This is about as far as we can go at present.

The distinction between organism and environment is always an arbitrary distinction. We may include the atmosphere of the earth as part of the earth or as part of its environment, depending on the nature of the discussion. The important thing to realize is that the distinction is arbitrary and in no way affects the balanced mechanical interactions between the parts of the system. One changes the other, and the other changes the one. The question which Davies raises, reduces itself to, "Shall we mark off particular limits in this series of reciprocal interactions and say that within these limits we will refer to the interactions by the term 'meaning'?" As an expedient, science requires a specialization of labor but it does not require different names for the same thing. The term 'meaning' is not used very generally as a scientific term. In fact its implications are obviously non-scientific. While I agree with Davies that 'complication' in human behavior is an important topic for investigation, it seems to me that the physical, chemical, anatomical, and physiological terms and methods which we already have, will lead to a social science more rapidly than any attempt at revising and re-defining the subjective classification which had its origin, not in science, but in poetry, mythology, and an egocentric theory of human behavior.

The more specific problem of meaning Davies states as follows: "the problem of meaning arises when the environment presents a complexity that goes beyond that which is characteristic of the situations in which the fundamental reaction-patterns have evolved. From this point of view, behavior is said to have meaning when the adjustment has some obvious and statable relation to this complexity" (p. 13).

I agree with Davies that behavior does have "some obvious and statable relation" to the complexity which arises when new stimuli act upon old reaction-patterns. But this relationship I include under the category of learning and habit formation instead of adding it to the muddle of definitions which have already been assigned to the term 'meaning.' Such an act as crossing a crowded street, Davies maintains, "refers to a situation which lies outside the neuromuscular changes to which the mechanistic behaviorist would reduce all behavior whatever, and" Davies continues, "the neglect of this feature in his descriptions obscures for the behaviorist an important problem in his explanation of how behavior is possible" (p. 14).

This is not the place to review experiments on learning, the variation of response, or the conditioned-reflex, of which crossing a street is an example. To convince oneself of this it is only necessary to watch a benevolent policeman helping an elderly country woman, who but for the assistance in crossing, would stand rooted to the sidewalk 'neurally blocked.' The illustration is far too complicated for a behavior analysis and since Davies himself makes no attempt at analysis in terms of his category of 'meaning' I wish only to assert that in learning to cross a street, I see nothing essentially different from learning to dance. Instead of 'obscuring an important problem' (new responses to new situations) as Davies maintains, the behavioristic method is responsible for what light exists.

"I have questioned," says Davies, "if an adequate analysis of perceptual behavior has been made by behaviorism, and the conclusion seems to be warranted that an element has been omitted that, for the purpose of reference, I have called 'meaning' " (p. 17). To this I would answer that for the philosopher the term 'perceptual' is 'loaded' in such a way that it already includes a non-mechanistic factor, and I do not believe that any analysis would be acceptable to him unless it did include a non-mechanistic element. I merely maintain that when I see (perceive) an object and respond to it, I find nothing which seems to require a category of 'meaning.' For instance, I never find myself observing, that the pattern of white and black circles before me, *mean* a cup of coffee, or *mean* a sleepless night, or *mean* the proximity of a speech. I merely *respond* to the stimulus by drinking, or not drinking, placing my napkin on the table, pushing back my chair, or in the many other ways in which I have responded to a cup of coffee. Of course I may afterwards review the action and analyze out the stimuli, anatomical structures, physiological processes, the change in the environment which my movements have produced, and then classify these steps. That it is as yet impossible to make a classification in terms of a complete regression of the physical antecedents of every stage in the responses, to whatever my infantile sensorimotor equipment may have been, I will also admit, but this implies only a lack of technical ability to do so. It does not imply that there is a *meaning* element which somehow or other has eluded and will always elude a scientific analysis.

Before I am willing to accept Davies' conclusion that "some kind of a criterion is necessary to determine the subject matter of behavioristic psychology" I should like to know more about the specific criterion that is established. So far as 'meaning' is a topic in current systematic psychology it seems to me to have reached the same level as 'consciousness.' To drop it entirely, seems the best alternative.

Teleology in Behavior. That I have not made clear what I mean by 'social' behavior appears from the following:

"Weiss skillfully avoids the question between the leading schools of social psychologists, and between them and the sociologists, as to what constitutes 'social' behavior, but he does so at the cost of giving us merely a negative statement of what he means by 'biosocial equivalence' " (p. 17).

In the more complete account in *Theoretical Basis of Human Behavior*, I have defined human behavior as those movements of the individual which produce changes in the environment (stimuli) which release responses (in others) that indicate the individual's social status. This is, of course, an arbitrary differentiation in the interest of the study of human behavior and is designed to exclude the more purely physiological processes of digestion, respiration, regulation, circulation, sleep, etc. Positively I characterize these movement complexes as the educational, vocational, administrative, recreational, and personal responses of the individual. I name them *social* behavior because the conditions under which they are learned include stimuli which operate *only* when there is a sensorimotor exchange between two or more individuals. That this behavior goes beyond the limits of heredity in the sense that 'the lone child on a desert island' would never acquire the conventional responses or learn to produce the stimuli, is obvious. The origin of 'social' stimuli is through a historic and progressive series of sensorimotor interactions between many individuals. Thus, oral speech, whatever may have been the original conditions under which it actually did develop, now represents a condition in which some sound made by individual A becomes a *substitute* stimulus for some object which acted on the eyes of A but not on the eyes of B. This substitute stimulus (usually verbal) releases in B the appropriate response to the stimulus which only acted on A. By sensorimotor interaction between individuals I mean merely that B's responses are appropriate responses *to what A saw*; B has used A's eyes instead of his own. Thus when A reports (in substance) to B, "I just saw a man-eating tiger headed in the direction of your hut," B does not respond to the cadences, rhythms, vocality, and tonality of A's voice, but he responds as if his own eyes had been stimulated by the man-eating tiger.

A discrimination between (a) biophysical stimuli, (b) biophysical reactions, (c) biosocial stimuli, (d) biosocial responses, should help in comparing them with each other.

(a) A biophysical stimulus is any form of energy which releases function in a sense organ; light, sound, pressure, warm, cold, etc.

(b) A biophysical reaction begins when sensory tissue is stimulated and a nervous process is transmitted over afferent, central, efferent neurons, to some motor organ or gland cell. The biophysical reaction ends with the contraction of a muscle or the secretion of a gland. All biosocial stimuli and responses are biophysical stimuli and reactions, but in addition they are *substitutes* for stimuli and reactions which have occurred in the past.

(c) The biosocial stimulus is one that has a social history. It is a residual effect of the interaction of two or more individuals, and has its origin in social interaction. Thus the statement, 'human behavior is a form of motion' is a biosocial *stimulus*. As it appears on this page it is a pattern of reflected light waves which act upon the eyes of whoever reads this paragraph. It is a substitute stimulus for the original stimuli which acted on many individuals while they were responding to (observing) the actions of other organisms, and a substitute for the recording responses made to the original stimuli. In what way the stimulus will affect the reader, will depend upon the social history of the reader. If he has not learned to read English, it will have practically no effect. If he has learned to read English the effect will depend upon what books he has read, his education, his verbal habits, the modifications which have occurred in his sensorimotor system. How much all of this depends on social interaction we need not attempt to analyze. I merely wish to add that a given biosocial stimulus may assume a number of different physical forms as, oral, written, printed, engraved, code, shorthand, etc.

(d) The biosocial response is a series of motor or contractile effects which produce the biosocial stimulus. The statement 'human behavior is a form of motion' is also a biosocial *response*. It has sensory, cerebral, and motor antecedents just as the biophysical reaction, but the motor component has produced a change in the environment (the light pattern of the printed statement) and this change is a 'socialized' stimulus for others. Obviously the statement could not have been produced without the interaction and coöperation of many individuals, living and dead. It has a social history in that it resembles other responses produced by other individuals. It also has an ontogentic history in that there was a *first* appearance of the response in the life of the one who first produced it, and its first appearance had sensorimotor antecedents. That it is difficult to establish these resemblances and antecedents goes without saying, but this is what the study of human behavior and of human achievement is for.

The dual character of biosocial stimuli and responses is better brought out in the following illustration. "Will you take dinner with us?" for me, is a biosocial *stimulus*, but for my friend who extended the invitation it is a biosocial *response*. As a biosocial stimulus or a response its particular biophysical form, whether oral, in writing, by telegraph, or by telephone; or whether I respond to the stimulus orally, in writing, by telegraph, or telephone, is unimportant. The physical forms of stimuli or of responses are *biosocially equivalent*, because the behavior of my host and myself at the dinner will be the same whether he invited me over the telephone and I accepted by mail; whether he invited me by telegraph and I accepted by long distance telephone; or whatever the combination may have been. It is in the sense that stimuli and responses which are biophysically different may be *biosocially equivalent*.

Perhaps I have stressed the difference between biophysical and biosocial *ad nauseam*, but I wish to clear myself of the charge of 'camouflage' which Davies makes. If the statement, "Will you take dinner with us?" releases the *same* response, whether the statement reaches the ear as a complicated sound wave produced by the muscles of the vocal mechanism, or reaches the eye as a light pattern produced by the entirely different muscles of the writing hand, then I have made my point of the biosocial equivalence of two stimuli which are biophysically non-equivalent (incommensurate). It remains only for me to show that this can be done *mechanically* (biologically) to meet Davies' criticism. This I have tried to do by indicating that the biosocial stimulus is a *substitute* stimulus for some other form of stimulation. Thus the social antecendents of "Will you take dinner with us?" were probably first developed as pantomime. The verbal forms are merely substitutes for this pantomime. I indicate specifically in my book,[6] the mechanical principles by which the word 'orange' may become, in a social sense, the substitute for an actual orange, and generally how the process of 'substitution' may be resolved into mechanical antecedents.

From the following quotation it seems to me that Davies arbitrarily places all social interaction into a teleological category. "But not to press the point, one does begin to wonder, in the light of the recognized discontinuity between biophysical and biosocial behavior that is positively affirmed in Weiss' denial that these two classes of phenomena are different aspects of sensorimotor organization, what mechanical explanation of the biosocial can be given that escapes the charge of being essentially teleological" (p. 18).

If Davies insists that all social or biological reactions are teleological (by definition) then, of course, mechanism is automatically excluded. The final criterion of teleology which Davies proposes is not stated clearly enough. He says, "Ultimately, teleology rests upon an affirmative answer to the question: does the behaving individual contribute factors to the conditions under which behavior takes place so that, if all other factors remain the same, the behavior would, in this case, be different?" (p. 20).

My answer to the question is, yes, the individual does contribute something, but this something is mechanistic. In the classical 'burnt child' illustration, the second time the child responds to the candle it responds differently than it does the first time, because after the first response the sensorimotor mechanism of the child has become so modified by the first response that the child is no longer the same. The child's contribution is this modified sensorimotor system. However we find the same thing in geology. Does not a stream modify its own channel through the mechanical action of the water on its own shores and stream bed and *contribute* this modified channel to the effects which next year's flood will produce? Knowing how the child responded to the first sight of the candle, is it not easier to predict how it will respond the second time, than it is to predict how this spring's freshet will modify next year's stream bed? To differentiate the action of the 'child' from the action of the 'stream' and to say that one is teleological and the other mechanical, seems to me to be merely another attempt to preserve philosophical distinctions where science finds none.

I have already indicated that the statistical treatment of human behavior points toward mechanical causation in social laws. Davies' 'indeterminism' means only 'low predictive value,' and this prevails in what is regarded as strictly physical (the weather, earthquakes, tidal waves, etc.) quite as much as in social or biological occurrences. Even the most complicated forms of behavior may be regarded as one stage in a strictly mechanical series. For instance, from my postulates[7] on 'Stability and Form' and 'Organization' it follows that human behavior is one of many forms of motion occurring in an electron-proton aggregate. Variability in duration is one phase of this variety. In its simplest forms human behavior is made up of movements which have survived in the reciprocal interaction between the organism and the environment. In man these movement complexes and interactions have reached a maximum variability. That some of these complexes should assume the form of what I call sensorimotor interchangeability between individuals, is only one of the many probabilities which might be expected from the law of like and unlike signs. That the still more complex forms of this sensorimotor interchangeability that I have called *compound* multicellular organization increase the stability of those movements which remove the limitations of disease and death; extend the sensory range and enlarge the environment; extend muscular energy and precision through machinery; correct faulty inheritance through education; reduce the time and energy devoted to the food and shelter activities; eliminate wasteful competition between individuals; is again only the mechanical operation, within specific electron-proton aggregates, of the law of like and unlike signs.

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[6] *A theoretical basis of human behavior*, Chapter XIII.

[7] *Op. cit.*, 390-391.

We may express all this in such a statement as 'the behavior of man is being modified in the direction of producing an optimum relationship between (a) the time devoted to the production of wealth (in the economic sense) and (b) the time available for recreative or leisure activities (productive or unproductive). The most effective method in the production of wealth is through coöperation and the specialization of labor, but such movements require the individual to adapt himself to a tempo, a distribution of his time, and a rate of output, which is set by the *group* or by machinery. This tempo and rate is always too fast or too slow for any given individual. Recreation on the other hand is made up of movements whose tempo and rate is regulated by the sensorimotor condition of the individual rather than by social practice or machinery. There is thus an antagonistic relation between 'labor and leisure' which is constantly approaching an 'optimum ratio' which varies from generation to generation, from nation to nation, from infancy to old age. That we should expect just this type of behavior from the operation of the law of like and unlike signs would justify the philosopher in classing behavior as teleological because it is in conformity with this 'optimum ratio' or *final* tendency, but for the same reason the scientist is justified in calling it mechanical. This paradox leaves the questions (a) shall we extend the philosophical conception of teleology *downward* into the sciences from which every attempt is being made to eject it, or (b) shall we extend the scientific conception of mechanism *upward* into social interaction. When we have actually studied how biosocial stimuli and responses develop, I do not think we shall need the teleological category. That I propose to throw out this category first rather than last is a personal response arising from the observation that practically no advance was made in physics, chemistry, and biology, until teleology was thrown out.

In defining human behavior as a form of motion I, of course, leave no room for what Davies calls uniformities of some sort or other which do not commit the psychologist to the position of materialistic determinism. But why should this spectre of vitalism be a constant source of inhibition when not even a half-dozen psychologists will agree on what is to be included under any one of these non-physical uniformities? That we can continue to describe human behavior in the old subjective terminology I do not deny; and that it is possible to improve this so that it will more adequately describe human behavior, is also granted—but it is not being done. Every critic of mechanism in human behavior instead of improving the subjective terminology which already exists merely adds a few more categories. Davies' wishes, McDougall's purposive strivings, the drives, meanings, desires, various dynamic and teleological factors which have sprung into existence during the last twenty years should be a warning to one scientifically trained. My assumption is only a working hypothesis to be used as long as it works. An attitude of doubt, caution, vagueness, is not a good working hypothesis for science. I think it must be clear to all of us working in the systematic field that the degree of controversy between structuralists, functionalists, configurationists, behaviorists, etc., is maximally articulate and minimally fruitful. I have tried to focus the discussion on the fundamental assumptions underlying psychology and the study of human behavior. To this end I have formulated a set of postulates[8] which are short and at the same time as inclusive as I have been able to make them. Of course I realize their shortcomings, but as a beginning they may serve as an outline into which substitutions, alterations, and extensions, can be introduced. With a set of postulates for each of the divisions of psychology, or for individual psychologists, we can place before each other and our students something tangible upon which the 'survival of the fittest' may operate with greater expedition. *

Ohio State University ALBERT P. WEISS

AN ANSWER TO DR. BORING

In the April, 1925, number of the JOURNAL, appears a note by E. G. Boring entitled "Scientific Induction and Statistics," in which articles by Murchison and Gilbert,[1] and by Murchison and Pooler[2] are attacked as if in a casual reply to Kelley.[3] The two articles in question were written by graduate students of mine, and the data contained therein have never up to now been checked by me. Since I did check the conclusions in the two articles in question before signing my name as joint author, I shall attempt a brief reply which will add perspective to the principles involved.

(1) The two articles in question by Gilbert, Pooler, and Murchison are but two chapters in a very large book to appear soon. When this material first began to be published in the *Journal of Criminal Law and Criminology*, all of this was explained very carefully. In every succeeding article, reference is made to this first publication.

(2) The method I have followed in presenting my data in all my publications on criminal intelligence was decided upon after mature deliberation and consultation with able statisticians. The method makes possible complete fusion with future data, and is the only general method which makes such future amalgamation possible. The method consists in giving simple arrays of quantitative measurements, with the discussion strictly limited by the data presented. All conclusions are headed 'tentative,' and are to be revised in the light of future accumulations of data. I have in my possession two letters from world famous statisticians congratulating me on this simple, lucid, and useful method.

(3) The method of presenting averages, deviations, errors, and probabilities is a method that is not useful in presenting a large number of relatively small groups of quantitative measurements. I am dealing with not more than 7000 cases in all, and these cases have been classified into a very large number of small groups. I discussed this matter at much length in the *Journal of Criminal Law and Criminology*,[4] and actually presented without comment ten pages of closely printed lists of averages, deviations, errors, correlations, and regression coefficients. It was obvious to me that my groups were not large enough for such methods to be used with profit, so I presented such figures without comment and proceeded to use the method of simple arrays of measurements.

(4) Boring announces that "the thing to do is to compute the average scores and their probable errors and then to compute the constants that

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[8] *Op. cit.*, Chapter XVII.

*[A rejoinder by Dr. A. E. Davies will appear in the October number of the JOURNAL —Ed.]

[1] C. Murchison and R. Gilbert, Some marital concomitants of negro men criminals, *Ped. Sem.*, 32, 1925, 652-656.

[2] C. Murchison and Priscilla Pooler, Length of incarceration and mental test scores of negro men criminals, *ibid.*, 32, 1925, 657-658.

[3] T. L. Kelley, Principles and technique of mental measurements, this JOURNAL, 34, 1923, 408-432.

[4] C. Murchison, American white criminal intelligence, *J. Crimin. Law & Crim.*, 15, 1924, 239-316, 435-494.

bear upon the 'significance' of the difference." This is the method I discarded as worthless in the very beginning. It is a serious matter for a man in one field of psychology to assume to teach a man in another field of psychology how to play his game. Such helpfulness is meritorious, and should be appreciated if it is valid and can be trusted. Personally, I am seeking advice all the time from all sources that can be trusted. Can I trust Boring's advice in this matter? He computes for my inspection the averages, differences, errors, etc. which he says I should have computed. Whether I should or not I considered and decided against years ago. But can I trust Boring? Even though I realize that he does not know even in an elementary way the problems I am dealing with, can I have confidence in what he does know? On the basis of what data was he able to compute the above averages, differences, errors, etc.? The raw original data have never been published and have never gone out of my possession. How did Boring make his computations? He simply assumed that all the measurements in a class-interval are compacted at the centre of that interval. Such assumption is statistically proper in the treatment of large groups, but is sheer nonsense for the analysis of small groups. Furthermore, his figures are all wrong. As an example: he computes the average score of married whites as 80.78, when the true average is 74.3. His computed average deviates from the true average twice the probable error of the average.

(5) Is Boring competent to engage in this type of criticism? On the first page of his Note, he makes the following statement: "We come certainly to induction when we wish to assert that all A's tend to be greater than all B's, that is to say, any group of A's will on the average be greater than any group of B's." Boring here links with the words "that is to say" what he supposes to be two logically identical statements. To pose as a teacher of logic, and blunder like that! On the second page of his Note, in discussing my method of presentation of data, he makes the following statement: "I wondered why two pairs of such similar distributions were printed side by side if not for the sake of demonstrating a difference." Boring's assumption that it must be for the sake of demonstrating a difference is most amazing!

(6) Does Boring, in his own research work, observe the principles he is attempting to demonstrate to me? On the second page of his Note, in discussing Gilbert's cautious and valid tentative conclusion "on the average, married negro criminals seem somewhat less intelligent than the unmarried," he offers the following substitution: "The adequate statement would be that criminals were thus and thus in that particular prison at that time, and that they do not, when statistical insight is had into the case, even seem to be thus and thus in general." The 'statistical insight' that Boring mentions is an animal I have never met. But wholly apart from that, to what extent does Boring in his own research publication state his conclusions wholly in terms of his data in their time and place? After his investigation of appetite,[5] based only on *three* subjects, he stated the following conclusion among others: "The most constant and characteristic feature of appetite is an oral, deglutitory attitude." Boring here makes a universal conclusion on the basis of data from three subjects. After investigating the question of introspection in dementia precox,[6] using only *eight* subjects, he makes the following general conclusion: "Persons with dementia precox can, under experimental conditions and without prolonged special training, give reports indicating the general trend of consciousness." He should have said 'some persons,' but did not mean that at all. After investigating the sensitivity of the alimentary canal,[7] using only *seven* subjects, he made the following general conclusion: "The esophagus is

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[5] E. G. Boring and Amy Luce, The psychological basis of appetite, this JOURNAL, 28 1917, 443-453.

[6] Introspection in Dementia Precox, this JOURNAL, 24, 1913, 145-170.

[7] Sensations of the alimentary canal, *ibid.*, 26, 1915, 1-57.

sensitive to warm and to cold stimulation throughout its length." In making the conclusions from this investigation of seven subjects, Boring formulates more than *sixty universal generalizations!* All of Boring's scientific contributions would need either to be thrown overboard or entirely revamped if he were to follow the principle which he is trying to teach me.

(7) Kelley is the one statistician who has taken the trouble to discuss Boring at some length. What does Kelley think of Boring's ideas in the general field of statistics? In discussing a systematic statement by Boring, Kelley says: "The procedure described is such as I believe a competent scientist never resorts to. . . . 'Absurd' hardly characterizes this procedure. Is it not rather a matter of the honesty of the investigator?"[8] "As a consequence of the foregoing I contend that the limitations which Boring ascribes to much of statistical procedure are unwarranted."[9] "Our conclusion therefore is that none of the measures advocated by Boring is free of those things which he inveighs against."[10]

Yet Boring assumes that he has found something which gives him an opportunity to show that both he and Kelley are right![11] It is exceedingly probable that Kelley is right.

Clark University CARL MURCHISON

THE 'VISUAL PERCEPTION' OF MOVEMENT *vs.* THE 'APPREHENSION' OF MOVEMENT FROM VISUAL STIMULI

Once more the problem of visual movement has been attacked,[1] and this time with the stated purpose of checking up the results of earlier investigations (75).[2] Inasmuch as we come in for a fair share of the discussion, it will not be amiss to consider the new work in the light of our own experiments. When an author presents a set of results in terms of which he proposes to criticise an earlier investigation—especially when that criticism is adverse—we have a right to demand that his experimental setting and procedure be comparable with that of the work criticised.

The relevance, then, of Higginson's objections, which are based upon his inability to correlate temporal interval or 'grey flash' with optimal movement, depends entirely upon the completeness with which he duplicated our conditions—the necessary conditions for such a correlation. In some ways Higginson's experiment is similar to ours; but the differences are so fundamental as to preclude the possibility that he could duplicate our results.

(*a*) In the first place, Higginson's stimulus is entirely different; and the difference unquestionably introduces factors that were not present in our work. He admits that the irradiation of the light about the slits might have been reduced (75, note 17). Such irradiation of a light, as it is turned on or off, very readily gives the impression of movement. Just how Higginson's substitution of light directly transmitted through a slit for an uniformly illuminated field in which dark rectangles were set gives "a greater control over the extra-organic factors than has been obtained in most of the previous investigations" (75) is not apparent. Certainly, it precludes any direct comparison of his results with our own.

(*b*) The *Aufgabe* under which Higginson's *O*s worked differs materially from ours. We called for 'description' or 'characterization,' as the case might be, and demanded complete assurance (*319f.*). This task was adequately performed after a fair amount of practice. All *O*s attained to stability of report; and no reports were acceptable until they became stable. In contrast, Higginson's directions to "observe carefully whatever comes and report," and to "indicate by the number 1, 2, 3, or 4 the degree of assurance" (78) were, he admits (79), "ambiguous" to his *O*s; and he endeavored to correct the ambiguity by further directions as to what they should and should not report. The *instability* of the reports under these instructions is a point of which Higginson makes a great deal (79, note 21; 83, 105, 111). The disagreement of two groups of *O*s under two so different sets of instructions is only to be expected.

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[8] Kelley, *op. cit.*, 411. [9] *Ibid.*, 411. [10] *Ibid.*, 418.

[11] Boring, Scientific induction and statistics, this JOURNAL, 37, 1926, 304.

[1] G. V. Higginson, The visual apprehension of movement under successive retina excitations, this JOURNAL, 37, 1926, 63-115.

[2] Page references in roman type are to Higginson's article; those in italics are to our paper, An experimental study of visual movement and the Phi phenomenon, this JOURNAL, 31, 1920, 317-332.

(*c*) Higginson's bases of interpretation and use of terms are so different from ours that an agreement of interpretation would be impossible even if the results were identical. Higginson's *O*s 'apprehended' movement; and although in his title he calls this movement 'visual,' it turns out that it may be carried auditorily, tactually, or kinaesthetically (98, 103, 110). This sort of apprehension of movement from visual stimuli is not what is ordinarily meant by the perception of *visual* movement. Moreover, Higginson says: "It must not be assumed that 'full-movement' always meant that a line was apprehended to move, as a line, across the field; *although this is the usual meaning of the term in previous studies.*[3] The *O*s instead reported at times that the experience *meant* that one line moved from one position to another; but that the actual movement of the line across the field intermediate to the two perceived positions was not seen" (79). But we defined "optimal movement" as "the movement of a line all the way from one position to the other" (*318*); and this movement had to be perceived visually, *i.e.* seen, and could not be simply *meant*.

Higginson's attempt to apply his criteria to our results has led him into a number of detailed criticisms that are purely artificial and that lose all force when considered in their proper setting. Not one of them will bear scrutiny. We give a few examples. (1) "It is strange that he [Dimmick] does not cite figures or other testimony to the alleged change of optimal over into unimembral and bimembral movement as he passes temporally in either direction from the optimal interval" (101). It would be strange had we not done so! Tables I and III of our report (*321f.*) furnish the desired data. We were not particularly concerned with partial movements (see the statement of our problem, *317*); but we have given the essential facts concerning them that came out of our work. (2) "We must examine certain of his [Dimmick's] 'movement' reports which seem to us ambiguous" (105). Higginson quotes 11 of our reports, and shows that he can interpret them otherwise than we did. But he uses his own definition of the 'apprehension' of 'full-movement'—although he knows that it is different from our 'perception' of 'optimal movement.' Our reports are, we think, entirely straight-forward—if they are read with a proper regard for our definitions. (3) "As we know, Dimmick stands for a close correlation between 'process' configuration and type of movement" (107). Higginson copies some reports from our paper and remarks that "these reports are cited by Dimmick as representative of the alleged correlation" (108). Higginson has the cart before the horse. We did not "stand for a close correlation" until we had examined our results. Nor is it true that "these reports are cited by Dimmick as representative of the alleged correlation." Instead, they are *samples* which show "*the nature of the visual experience which filled the interval*" (*323*). (4) "If, under the process instruction, *O*s generally observed only 'process,' there would seem to be neither logical nor experiential justification for claiming that the process configuration could possibly mean anything more than just what it was" (109). Here and in the remainder of the paragraph Higginson is not clear regarding the distinction between 'description of process' and 'statement of meaning.' His remarks about the numerical interpretation seem also to imply a misunderstanding. The values he quotes are obviously parallel, although he has chosen the poorest but one of the 16 sets given. What can he mean in this connection by the statement, "What the organism apprehends at one moment is not to be identified with its earlier and later experiences" (109)? Further, what does his table of differences "which should not be overlooked" show? It appears to show nothing more than that we have dealt with a finite number of cases. (5) "If the field were filled *uniformly* with a grey (to condition full-movement) . . . .;" "the reader gains the impression that a plain but brief grey was being described;" "we have little or no description of the temporal aspect" (110). Again we appeal to our original exposition. We never characterized the 'grey flash' as uniform; we called it *multiform*. We described the temporal aspect as *brief*. What further descriptive terms are there that are applicable to this duration? The integration of the two attributes gives a "shimmery," "unsteady," "live," liquid," "flame-like" grey.

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[3] The italics are ours.

Such criticism is no criticism at all. The ambiguities, discrepancies, etc. that Higginson remarks do not exist in our presentation of results and conclusions. On the other hand, he seems to have shut his eyes to the aspects of his own results which, in spite of the different conditions of his experimental setting, confirm the results of our earlier study.

Summary. Our reply, then, to Higginson's criticisms is categorical. (1) His experimental setting is entirely different from ours, and therefore can offer no basis for criticism of our results. (2) His problem, as indicated by his use of the term 'apprehension,' is also different. (3) His objections to our presentation of results are based upon misinterpretations of, and disregard for, our explicit statements.

Hobart College FORREST L. DIMMICK

MEETING OF THE MID-WESTERN EXPERIMENTAL PSYCHOLOGISTS

After a lapse of twenty years or more a group of mid-western experimental psychologists were invited to an informal conference at Northwestern University, Evanston, Illinois, on Friday and Saturday, May 7 and 8. The meeting place was appropriate because several decades ago, meetings were frequently held at Evanston and Chicago which were attended by such distinguished psychologists as Angell, Baird, Colvin, Max Meyer, Scott, and Watson. Experimental psychology certainly showed its vigor when after this interval more than 100 people interested in psychological research would come together and discuss current problems.

The sessions were opened Friday evening by an address from Jastrow, who surveyed the present field of experimental psychology and incidentally lamented the appearance before the public of fake psychologists and pseudo-psychologies. He indicated a parallel phenomenon in the history of medical science and practice and felt sure that as soon as the public became more educated in the matter they would distinguish the true from the false. He outlined the development of mental hygiene as a distinct American contribution and made some reference to the increasing need for the control of the emotional life which required a more intensive experimental study of human feelings.

On Saturday morning and afternoon the sessions were held in the lounge room of the Patten gymnasium. Gilliland and Morgan (Northwestern) presided at the morning sessions and E. S. Robinson (Chicago) in the afternoon. Both sessions were attended by about 75 to 100 persons. There were a number of prepared reports in the morning and one in the afternoon. Vaughn (Chicago) gave the results of an experiment on learning under negative incentive following the work of Rexroad and others on the effect of punishment in the learning of human individuals. He has found in general that, while there is a large individual variation especially in emotional attitude, toward the experiment, on the whole the rate of movement is slower and attention is keener in the series with threatened punishment. The effect of the threats has not been fully investigated but it is apparently not pronounced except for a short period of time. Positive incentive in terms of reward was less effective in learning than negative incentive, in the way of punishment, but the positive incentive was better than no incentive at all.

H. M. Johnson (Mellon Institute) gave an account of some experiments on restlessness during sleep. Ten *O*s slept on beds which were supported by four very sensitive pendulums in each case. At 5 min. intervals throughout the sleeping time of approximately 7¾ hours, records were automatically taken of the amount of motion produced by the sleeping person. The sudden movement of an arm was sufficient to be recorded. In general the most active *O* stirred once in every 1¾ min. and the least active *O* stirred on the average of once every 16 min. The mean for both groups was 11.7 min. Further investigations were made on the question of mental capacity before and after sleep. Two forms of substitution tests were used one-half before hour and one-half hour after sleeping. In one series the evening performance was on the whole 10% better than the morning performance and in another series 6% better. These results confirmed other reports made in industrial investigations. They are explained in one of three ways: (1) that the toxic effects of fatigue may indeed act as a stimulant before retiring; (2) that in the morning the habit of relaxation continues into the waking period; (3) that there is a large individual variation and a scattering of activity through the night which must be taken into account. Some individuals moved most during the first quarter and less during the next two quarters, while the activity of others was distributed in various patterns during the night.

S. L. Stevens (Northwestern) attempted to correlate objective response with conscious behavior on the theory of what he called *metabolic functionalism*. The experiment used a compound reaction device with five handles numbered from 1 to 5 and two pedals lettered A and B. The instructions required a quick response to a complex pattern. Any experimental series could be interrupted by *E* or *O* for an introspective report. Three problems were investigated: (1) the process surrounding the perceptual clearing up of the stimulus; (2) the conscious adjustment to the stimulus in terms of meaning; (3) the function of attention as a conscious process. Changes of bodily expression, especially the shifting of eyes, as a cue to attention were noticed by *E*. No definite results were at hand but the data so far indicated that conscious behavior is a unique phenomenon and that it is more of a 'doing' than of a 'having.'

C. R. Griffith (Illinois) outlined his experiments in connection with athletics. They included investigations of the memory span for such materials as football signals and the various characteristic performances of each of 9 baseball players while 'at bat.' The rate of memorizing is also being investigated. Other researches go into the problem of motor adjustment under situations analogous to each one of the sports, some of which were in the form of serial reactions. It has been found that many men incapable of "making a basket" in basketball do better when the motor mechanism is first automatized under blindfold conditions. On the whole these tests so far devised tally with the coaches' estimates to the extent of indicating the highest 10% and the lowest 10% in the varsity squads. On the side of pure psychology these investigations show that motor learning may mean not merely the acquisition of motor skill but an acquisition of motor dissociation so that one part of the body will react quite independently of another. Again, in terms of the more recent discussion of *Gestalt*, the problem of integrating perceptual and motor responses in a single pattern was brought up. An expert football player in an uncanny way would be able to judge that an opening in a line was about to be made. He would be able successfully to perceive a situation in the process of its formation.

Max Meyer (Missouri) discussed a paper on the application of psychology to industry. He objected to the traditional use of reaction times which involves single jerky movements not commonly found in the industries. On the other hand he objected to too great a complexity of apparatus which presented conditions too closely analogous to the situation to be investigated. Between these extremes lies the device which he has perfected on the basis of the well known 'Cartisian diver.' There are two divers in two separate jars, each one controlled by a separate lever in the right and left hands. The problem was to keep the diver midway between the top and bottom of the jar in rhythm with the sounding of bells. A time clock would indicate when mistakes had been made and the aim of the *O* is to keep on pumping in alternate directions in rhythm with the bells by varying the intensity of the strokes. It is inferred that a leak in the system makes it necessary to keep up a certain amount of pressure.

Hull (Wisconsin) began the afternoon session with a discussion of some experiments performed under hypnosis. (1) Post-hypnotic amnesia was studied with nonsense material and by means of several of the classical methods. Individuals showed a 100% recognition of material from one trance state to another but could not recall any of the material in the waking state. Relearning the material during the waking period, however showed 50% greater efficiency than would have been possible without previous learning. (2) The strength of post-hypnotic suggestion in which an *O* is told to do something in the waking state was also studied. The suggestion was made that he would keep breathing slowly when he read odd pages of the book and faster when he came to the even pages. The rate of disappearance of this suggestion was carefully studied and so far results show that the lasting effect is greater than the waning effect during the first few hours.

When the informal laboratory reports were called for it was found that there was not enough time to cover all laboratories represented. The order of report was then determined by chance and the first lot fell to Ohio State. Renshaw discussed human posture both phasic and tonic, and especially the reciprocal relationship between these. He briefly outlined the segmental postures like the accommodation of the eyes and he discussed individual differences in maintaining postures in long and tedious mental tasks. The hydrogen ion concentration of the saliva was used as the best indicator of vascular and visceral disturbances as well as an index for general metabolic acidity of tissue. On the basis of these studies he classified individuals into four groups: (1) those who are metabolically quiet during the no-task period and active during work; (2) those who are quiet during the no-task period and quiet during work; (3) those who are active in the no-task period and active during work; (4) and those who are active during the no-task period and quiet during work. Rexroad continuing his work with negative incentive reported investigations bearing on the relation of verbal response to finger response in a multiple reaction device. He encountered several curious results. One concerned a double thumb movement which would necessarily be wrong but which showed a sort of perseveration or automatic response the second time. He also showed that *Os* had a tendency to respond to the word green when visually displayed in place of the color green. Even when the other fingers were tied back 29 out of 32 *Os* still responded to the word green and 11 out of 32 responded to the word green when auditorially presented, even when they knew that this was a wrong response.

Dockeray (Ohio Wesleyan) reported some experiments on the phantom sound with binaural beats through the use of the oscillating circuit. His results are uncertain as yet. In another direction he found that the conduction of sound through an intermediate *O*'s head was impossible except with the sound of the spoken voice.

Ruckmick (Iowa) was requested at this point to discuss experiments on sound localization which had just been completed. He emphasized the desirable factors of the heterodyne circuit which produced relatively pure tones without electric filters. A series of investigations gave unequivocal results for phase differences between the lowest audible tone and a tone of approximately 8,000 cycles. He outlined also a published investigation which showed that intensity is a deciding factor in the middle range of tone values.

Reymert (Wittenberg College) reported on some experiments investigating kinaesthetic sets with ball tossing at targets by means of the worse arm. At the same time the psychometric function as related to the attensiometer was calculated. This research also used the Whipple target, the rifle aiming response and muscular strength and rhythm. He outlined further some experiments correlating quantitative scores with qualitative introspective reports. Materials so far used comprise the Seashore musical tests, the Healy pictorial tests and other means. Experiments were also directed on the problem of meaning with geometrical designs using three-year old children as *O*s. A preponderance of kinaesthetic cues was found.

Wheeler (Kansas) showed some tentative results on neuromuscular rhythm. If an *O* showed a decided decrease in performance on the 20th circuit in the maze he would almost always show similar decreases on the 40th, 60th, etc., turn. But so far there have been two groups of *O*s: those who show (1) high steeples or crests at certain periods; (2) those who show relatively low steeples. Heron has been continuing some experiments of Hunter on the conditioned reflex with the tilting of the learning frames in which white rats were subjects and, also, on the problem of orientation in the horizontal plane. So far he has results which show that the white rat does not keep on the same side on which he has turned from his successful performance. He is also experimenting with a frame constructed of 4 tubes in the shape of a cross which can be tilted up or down so a rat going up one tube for food will be compelled to go down the same tube at another time. So far the results are not well established as regards the effect of tilting in this way.

At this point there was an intermission during which refreshments were served. The session was then continued by Jacobson (Minnesota) who showed that there were no effects in skilled performance after the extirpation of the frontal lobes, and in a few cases of the cerebellum, in some of the lower animals including rats and chimpanzees. Metfessel (Iowa) who reported the natural fluctuations of the voice in the (a) vibrato, (b) the tremulo, (c) the emotional changes with laughter and grief, (d) the trill and, (e) fast fluctuations of pitch and intensity superimposed on any of these. Through a refined method of amplification photographic records of these changes of rate are recorded. Travis outlined experiments in neuromuscular action-currents with a 40,000 cycle oscillator and two stages of audio-frequency amplification. This wave is received through an 8-tube superheterodyne radio set and finally recorded through a phoneloscope. The speed of conduction from each hemisphere to the two arms is the problem under investigation as well as the action of the recti in abdominal breathing.

In the evening over 70 persons attended the banquet at the Orrington Hotel. President Scott (Northwestern) was master of ceremonies. He gave an historical account of the development of the campus and of meetings formerly held at Northwestern and outlined the future of experimental psychology. It is already impossible for any one man to know the whole field thoroughly and the demand for young psychologists appears at present to be from year to year greater than the supply. He noted incidentally that pure psychology must guard itself against two tendencies: (1) that of moral misinterpretation on the part of the public, and (2) the encroachments made by industry on our personnel. Already two or three distinguished psychologists have become vice-presidents of large corporations and even universities are making administrative demands on professional psychologists. The presidents of six of the larger midwestern universities have been drawn from the ranks of psychology. Carr (Chicago) was the next speaker. He outlined the reasons for the disproportion between the strength of stimulus and intensity of sensation in the Weber-Fechner law. He was inclined to put the difference in terms of a mechanical loss. After briefly outlining the various historical interpretations in terms of loss in nerve conduction at the synapses or at the neural center or in the brain or in the outside media, he came to the conclusion that there never was a one to one relationship between cause and effect in any physical phenomenon without loss of some kind of energy, or a transformation of energy into other forms.

Weiss (Ohio State) outlined the experimental program at his laboratory which concerns itself chiefly with the biosociological approaches to genetic psychology in three ways; (1) how does the child get to be an adult? *i.e.* what are the mental and physical conditions surrounding each stage of development, (2) how does a child get to be a child? *i.e.* what are the inherent forces that develop within from inherited traits, and (3) how does a child get his environment? *i.e.* to what extent does the environment influence him and to what extent do he and others influence his environment.

Several invitations were received for next year's meeting and Gilliland was authorized to act as chairman of the committee[1] which will make the necessary decisions and arrangements. At the meetings and banquet there were representatives from psychological laboratories at the following institutions: The universities of Chicago, Illinois, Iowa, Kansas, Minnesota, Missouri, Northwestern, Ohio State, Ohio Wesleyan, Wisconsin; Chicago Normal College, Lake Forest, Wittenberg, and Mellon Institute. Many psychologists independently commented on the amount of free discussion that accompanied each paper and informal report and they expressed a keen desire to have this conference become an annual event.

University of Iowa C. R. RUCKMICK

THE TWENTY-THIRD ANNUAL MEETING OF EXPERIMENTAL PSYCHOLOGISTS

The twenty-third annual meeting of Experimental Psychologists was held in the psychological laboratory of the University of Pennsylvania on April 5-7. Professor Samuel W. Fernberger acted as host and chairman. Eleven laboratories were represented. Members of the faculty and graduate students were in attendance from the University of Chicago, Columbia University, Cornell University, Harvard University, Hobart College, Princeton University, Wittenberg College, Wellesley College, Wesleyan University, and Yale University.

The opening meeting on the afternoon of April 5 was, in commemoration of the 125 anniversary of the birth of Gustav Theodor Fechner, devoted to a discussion of psychophysics. The other meetings were given over to reports from the various laboratories of work that had recently been completed or that was still in progress.

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[1] The personnel of the committee consists of Professors A. R. Gilliland (Northwestern) chairman, C. R. Griffith (Illinois), C. Hull (Wisconsin), E. S. Robinson (Chicago), A. P. Weiss (Ohio State), and C. A. Ruckmick (Iowa).

At the close of the evening meeting on April 6 the following resolution, proposed by Professor E. B. Titchener, was unanimously adopted: "Resolved, that this meeting deplores the increasing practice of collecting administrative or supposedly scientific data by way of questionaries; and that the meeting deplores especially the practice under which graduate students undertake research by sending questionaries to professional psychologists."

K. M. D.

The Journal of Philosophical Studies

We have just received the first number of the *Journal of Philosophical Studies* (Vol. 1, No. 1, dated January 1926) which is published quarterly by the British Institute of Philosophical Studies.

The *Journal* is edited by Sydney E. Hooper with the assistance of William Brown, M. Ginsberg, G. Dawes Hicks, L. T. Hobhouse, Julian S. Huxley, F. B. Jevons, Harold J. Laski, A. D. Lindsay, J. H. Muirhead, Bertrand Russell, G. F. Stout, A. E. Taylor, J. Arthur Thomson, and Clement C. J. Webb. It was founded as a means of keeping the widely extending membership in touch with the activities of the Institute, and also in the hope that the object of the Institute—the advancement of philosophical studies, "the supreme desideratum of our age"—would in some small measure be furthered.

As explained in the prefatory editorial, the *Journal*—since only a small minority of its readers have been professionally trained in philosophy—will "avoid being too severely technical" and will "aim at making philosophy intelligible to the well-informed student." Besides articles of general interest, it will publish original contributions which may be of special interest only to the expert, summaries of different fields of philosophy, surveys of philosophical literature in the various languages, reviews of books and important articles, and notes and news of the Institute.

This number, after the Editorial, contains the following articles: S. Alexander, Art and Science; G. Dawes Hicks, The Metaphysical Systems of F. H. Bradley and James Ward; Morris Ginsburg, Emotion and Instinct; J. Arthur Thomson, Different Kinds of Evolution; John S. Mackenzie, The Present Outlook in Social Philosophy; and F. B. Jevons, The Purpose of Philosophy; abstracts of Bertrand Russell's lecture on Perception and T. H. Pear's lecture on The Definition and Scope of Psychology; surveys of recent English Books, of The Doctrines of M. Émile Meyerson, and of Philosophy in Russia; and departments devoted to New Books, Periodicals, Discussion, and Institute Notes. The number closes with a list of officers of the Institute and of the original members of the Council.

K. M. D.

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AN EXPERIMENTAL INVESTIGATION OF THE DETERMINANTS OF APPARENT VISUAL MOVEMENT

By Harry R. DeSilva, Harvard University

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It is a well-known fact that a perception of movement may be experienced between two stationary objects if the interval of their appearance is regulated within very brief limits of time.[1] In this study an endeavor has been made to discover the principal determinants or conditioners of this phenomenon of visual movement, by using a tachistoscope in which certain objective factors in the stimulus-situation could be varied, and by carefully controlling other conditions under which the experience of movement arises.

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[1] The best summary of work done upon the perception of apparent movement is contained in an article by H. Helson, Psychology of *Gestalt*, this Journal, 36, 1925, 498-514. Other relevant information is to be found in his doctoral dissertation on *The Configurational Theory of Perception* in the Harvard University Library.

Other articles are: M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-278. K. Koffka, Perception, An Introduction to *Gestalt-theorie*, *Psychol. Bull.*, 19, 1922, 548-550; *Beiträge zur Psychologie der Gestalt* (containing articles also by F. Kenkel and A. Korte), 1919; with P. Cermak, Untersuchungen über Bewegungs und Verschmelzungsphänomene, *Psychol. Forsch.*, 1, 1921, 66-129. E. Lindemann, Experimentelle Untersuchungen über das Entstehen und Vergehen von Gestalten, *Psychol. Forsch.*, 2, 1922, 5-60. F. L. Dimmick, this Journal, 31, 1920, 332-333; F. L. Dimmick and G. H. Scahill, *ibid.*, 36, 1925, 412-417. G. D. Higginson, The Visual Apprehension of Movement under Successive Retinal Excitations, *ibid.*, 37, 1926, 63-115 (excellent bibliography).

This investigation has attempted to examine only some of these variable factors which give rise to and govern the experience of movement; the possible determinants appear to be so numerous and so difficult to describe and control that we are apparently still a long way from any final understanding of the phenomenon. Among the many variables, attention has been devoted chiefly to a study of the factors of repetition, fatigue, background, position, form of stimulus, meaningfulness of stimulus, encompassment of stimulus, fixation point and predispositions of various sorts.

Apparatus. A modification of the Dodge[2] tachistoscope (depending upon the principle that a glass plate is transparent when the illuminated object lies behind it, while the same plate functions as a mirror when the illuminated object lies in front) was constructed for this investigation. The accompanying diagram (Fig. I) indicates the arrangement of the apparatus.

The body of the tachistoscope was made of wood and painted black within and without except for white passageways leading from the stimuli to the source of illumination. Three 75-watt, Type-C Mazda, frosted, nitrogen-filled lamps (B), placed opposite the ends of the three parallel tubes furnished the illumination which was reflected indirectly upon the exposure fields. Aubert diaphragms (AD) were set in the tubes opposite the lights in order to equalize the illumination. Ground glass plates (W) placed in the passageways helped to render the illumination more even. The projections seen on the sides of the passageways in the diagram indicate screens containing square openings which served to confine the illumination to its proper path. Cards (S1, S2, S3) were inserted in slides through slits cut in the solid board top covering the entire tachistoscope. The optical distance of every stimulus from the eyes was 15½ in. and the size of the stimuli was such that with central fixation the physical stimulus pattern fell upon the retina within the area of central (foveal) vision.

Attached to the tachistoscope at the open end was a frame with brass grooves in which a falling shutter (steel) operated. The shutter (FS) fell from a constant height by its own weight when released by a catch. Its fall was checked by a padded platform on three spiral springs. The shutter was raised by means of a cord passed over a ball-bearing pulley.

The duration of the exposure was regulated by means of adjustable cardboard windows on the shutter. Exposure intervals were computed, regulated and tested from time to time. Contacts made at the beginning and during the fall actuated a Johns Hopkins (Dunlap) chronoscope,[3] thus giving the time of fall, which was found to be remarkably constant. The whole interval of fall varied no more than 10 to 15σ, making the *Zwischenzeit* vary little more than 2σ.

The *Zwischenzeit* was illuminated and was made standard at 60σ for all experiments. The exposure time of both the first and second stimuli was 65σ.

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[2] R. Dodge, *Psychol. Bull.*, 4, 1907, 10-13.

[3] K. Dunlap, *J. Exper. Psychol.*, 2, 1917, 49-52.

The tachistoscope was located in a window between two dark rooms. *O* remained in the darkness during the whole period.

Stimuli. Lines drawn on oblong cards were used for stimuli. The length of a radial line was 1 cm., of a diametral line 2 cm.; the width of lines was 1 mm. The size of the total visual field was 7 x 7 cm. Throughout the experiment the fixation point was always made to coincide with the junction of the two lines.

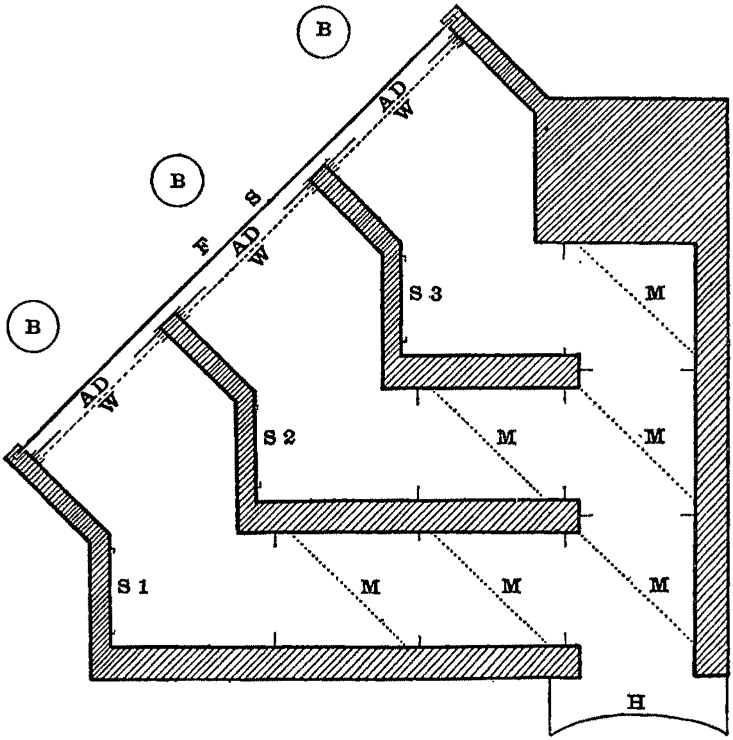

FIG. I. TACHISTOSCOPE. The diagram gives an overhead cross-section view of the tachistoscope. B: electric lamp bulbs. FS: falling shutter. AD: Aubert diaphragms. W: glazed windows. S1, S2, S3: stimuli. M: transparent glass plates. H: hood for *O*. The short lines projecting from the side walls represent large pieces of cardboard with square openings in the center. These screens serve to limit the visual field of the *O*, and to confine the illumination to its proper path.

Observers. The regular *Os*, three in number, were untrained in systematic introspection. They were A. R. Stone (ST), T. Frank (FR), first year graduate students in psychology, and J. P. Bethel (BE), graduate student in English. The *Os* were kept ignorant of the nature of the experiment and requested not to talk among themselves or to other people concerning it. They reported regularly three hours a week during the collegiate year from the last of September to the middle of March.

I. NATURE AND DEVELOPMENT OF THE PERCEPTION OF MOVEMENT

The data of the experiment reported upon in this section were gathered from fourteen *O*s during their first hour of observation of the phenomenon of visual apparent movement. The individual differences in the adaptation of the *O*s proved to be interesting, and the protocols derived from this experiment contain much relevant information concerning the nature and development of the perception of movement and its dependency upon certain determinants, of both an objective and subjective character.

Observers. Besides the regular *O*s, eleven casual *O*s contributed one hour apiece for this experiment. They were D. K. Adams, C. Goldthwait, H. Gurnee, H. Hoagland, E. D. Hutchinson, R. A. McFarland, J. M. McGinnis, C. C. Pratt, M. Upton, W. Vaughan, K. E. Zener. With the exception of Dr. Pratt and Mr. Goldthwait all *O*s were relatively untrained in experimental work.

Procedure. At the outset the *O*s were charged with the following simple instructions: "Fixate the dot before you. You are going to be shown something. Please observe it carefully and report what happens. Under no circumstances allow your gaze to wander from the fixation point. You will find you will be able to see what happens without taking your eyes off the fixation point, so guard against it."

Every effort was made to avoid the influence of suggestion, but occasionally throughout the investigation *E* attempted to follow up and elicit more complete information where data seemed scanty, and where *O* appeared to be unduly parsimonious. For instance, when the phrase 'flops down' was used two or three times by an *O*, it was incorporated into a casual question concerning the 'flopping down.'

The work was arranged as far as possible so as not to fatigue *O* and he was cautioned to notify *E* of the first moment of weariness. Effort was made to avoid 'pressing' *O* too closely for a precise or exact description. In general, *O* was left to report the experience as it occurred to him. Reports were given orally by the *O*s and written down by *E*. After the experiment was over the casual *O*s were questioned informally about the experiences during the whole hour and encouraged to relate everything that occurred to them regardless of whether they considered such information relevant or irrelevant.

Stimuli. The stimuli consisted of a radial arrangement (No. 1 in Fig. II) making an angle of 70° to the right, a diametral arrangement (No. 2, Fig. II) of the same pattern, and a pictorial arrangement of an encounter of an old woman with a dog.

Protocols. Because of lack of space the introspections of the *O*s are omitted and only a summary of the results from introspections is given.

Summary. (1) Usually at first the *O*s are confused and respond as follows: "The second line flashed in," "dropped in," "appeared from above," "it's too fast, I don't know," "horizontal appears, vertical appears," "diagonal thrown in," "flopped in," "fades in," "horizontal vibrates and then seems to fade." The apparent movement is obviously artificial under tachistoscopic conditions; moreover, the distractions of the walls of the tachistoscope and the slight flashing of light tend to render the report of *O* artifictitious—at least in the beginning.

(2) On continued observation the *O*s often eventually fall of their own accord, into report in transitive terms connotative of continuity, using the word 'movement' seemingly because it is the most natural and simple and obvious, and perhaps also because there is an experimental setting and sanction for the relationship between lines in space and time. There is a certain change in the attitude of every *O* above and beyond what would be expected from mere habituation. With most *O*s we might describe this change as a loss of 'criticalness', of 'uncertainty', of 'indeterminateness', and a general narrowing of the field of attention.

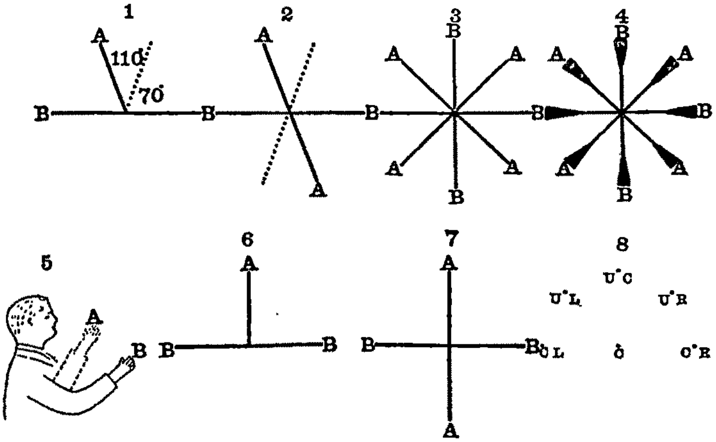

FIG. II. STIMULI. The eight diagrams show the form and geometrical relationships of the stimuli, and reference in the text is repeatedly made to this figure. The first seven diagrams represent successive exposures of a pair of stimuli: the initial position is marked 'A' and the succeeding position is marked 'B'. Diagrams 1 and 6 are for 'radial' movement; diagrams 2, 3, 4, and 7 are for 'diametral' movement. Diagram 4 is the 'windmill' figure. Diagram 8 shows the positions and corresponding symbols of the 6 fixation points used (Section VI) when fixation point was varied.

(3) There is a gradual development of the 'formedness' or 'togetherness' of the apparent movement with practice and training. The *O*s become more receptive of the movement and less attentive to other artificial disturbances. During a single hour even there is an obvious development in the ease of perceiving the movement of the lines, rather than mere perception of succession. As an *O* grows habituated to the artificial laboratory conditions and regards the successive appearance of

the bare lines as a meaningful, eventful situation the movement becomes thereby more nearly univocal and more distinct. In those cases where the meaningful situation was used, the configurational effect of the addition of meaning was patent. In every case where meaningful familiar objects were shown, the *O*s reported 'movement' either explicitly or by synonym, without the least hesitation, but in the case of bare lines the movement was not so clear. Thus it would seem that by clothing bare figures and lines with purposefulness or meaning we thereby render them better qualified to evoke movement experiences.

(4) At first, certain *O*s (proponents of *Gestalttheorie* might call them analytical *O*s) did not perceive the movement and continued for some while to speak of lines 'appearing' and 'flashing' without reference to phenomenological continuity. When, however, such *O*s were shown meaningful stimuli under the same conditions, they always said "it did so and so" or something to the same effect, implying that it moved. After seeing meaningful objects move under the same conditions and classifying the process as movement, the *O*s usually did not hesitate thereafter in applying the term 'movement' to the successive appearance of lines.

(5) The ends of the lines carry the movement. That part of the line nearest the axis of movement is comparatively unimpressive and unnoticed. Moreover, in the case of diametral movement the upper phase of the movement seems to be the one to which the naïve *O* naturally directs his attention. The lower phase of the movement is sometimes noticed, but the movement of the upper end of the line is the most prominent and impressive.

(6) The *O*s being questioned from time to time as to the positiveness and certainty of their judgments in the case of the equivocal situation, report that their conclusions are reliable and valid, and not 'forced' or 'arbitrary'. A quick judgment was usually made. When a judgment was slow the *O*s usually admitted uncertainty. They were repeatedly encouraged to report 'don't know' rather than give a reflective judgment.

(7) On several occasions *O*s reported that they were sure of the motion but not of the order of appearance of the lines or of any lines at all. This experience which came involuntarily and irregularly we take to correspond to the 'pure' *phi*-phenomenon[4] in which there is no synthesis of content, no founding of elements and no sensational attributes. With untrained *O*s it could not be obtained by attitude or 'trying'. It was something that appeared of its own accord at very irregular intervals.

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[4] *Cf.* M. Wertheimer, *op. cit.*, 215, 227. Also, V. Benussi, Versuche zur Analyse taktil erwekte Scheinbewegung, *Arch. f. d. ges. Psychol.*, 36, 1917, 65 ff. Benussi's *O*s were unable to experience 'pure' *phi* movement.

(8) Some of the *O*s occasionally reported movement in the reverse direction from which it actually occurred. In other words they saw first the line which actually appeared second.[5] It was noted that the movement was reversed mostly in a counterclockwise direction. The confusion of the sequence of lines, the reversal of the direction of the movement, and the corresponding loss of compulsoriness of the direction of the movement, occurred only in the early stages of training and under conditions of fatigue.

(9) In experiments of this kind involving uncontrolled attitudes it is always important to guard against artifacts. An attempt was made to avoid 'forcing' an analytical, process attitude upon an *O*. Only two *O*s ventured free rising analytical reports as to the nature[6] of the movement experience and we did not venture to ask anything further concerning the exact nature of the perception except in the case of these two men.

ZE characterized the experience of movement, that is to say, what was taking place in the arc of movement, as a "change in brightness, change of intensity. Something seems to happen there. Not black and not white. Like heat rising on a hot day from the p avement. But we can usually see heat waves better. This thing is much faster and more elusive." Upon being asked if he had anything more to say about the nature of the movement he remarked that "it was very difficult to see and much more difficult to describe."

CP adopted an analytical attitude of his own accord and gives us a much completer report: "I can only describe the movement by analogy. The pure movement has an appearance more of an imaginal type. Something more or less imaginary. Something more like the trail of a match in the dark. I can't follow the movement of the line through space. I can get something in terms of imagery, a fuzzy sort of thing that comes in the arc of the movement. It is different from the blackness of the line. It isn't as black and as 'clear-cut'." Later on: "a kind of blurring aftereffect." CP also remarked upon the extraordinary difficulty of ever being able properly to get at it descriptively. He seemed to think of the actual stimulus as being almost beyond the ability of the sense-organs and the perceptive process to grasp.

(10) At first nearly all the *O*s seemed to set themselves to experience the movement analytically. With the passage of

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[5] This phenomenon corresponds to Korte's reverse movement (*Zsch. f. Psychol.*, 72, 1915, 204). By repeating Korte's conditions, that is to say, making the second member wider and more 'intense' than the first, and by turning the attention to the second member, the direction of the movement was found to be reversed. Moreover, the movement was quite positive. Dimmick (*op. cit.*, 31, 1920, 331) reported that his *O*s were unable to get positive movement perceptions under such conditions.

[6] Dimmick (*op. cit.*, 331-332) states: "The temporal attribute is beaten up with the grey quality into an integration of the first order. All *O*s describe the experience as a 'flashing', 'flickering', 'unsteady', 'shimmery', 'flame-like', 'liquid', 'live' grey." Whether the description of the Cornell study is ultimate or representative of the immediate bare experience is to be questioned.

time and the recurrence of the stimulus-situation the general perceptual pattern or configuration seems to change, to become more unified and to yield a more nearly univocal and synthetic experience. The change of set seems to be due as much to non-stimulus determinants as to stimulus determinants. To *E* this indicates that the general perceptual pattern or set of *O* is of even greater significance and relevance in the study of movement phenomena than it is in the study of more static experiences. The 'analytical' and 'totalizing' attitudes seem to be quite variable. *O*'s attitude toward movement phenomenon varies in degree as well as in time. Consequently, greater efforts than usual must be made to control attitude in investigating the experience of movement lest we merely build up a system of artifacts which will be of no use to psychology.

II. INFLUENCE OF REPETITION, FATIGUE, BACKGROUND, FORM OF STIMULUS AND MEANINGFULNESS OF STIMULUS

This experiment is an outgrowth of the work reported upon in the preceding section and consists in a more careful and systematic examination of the factors of repetition, fatigue, background, form of stimulus and meaningfulness of stimulus, in an attempt to discover and describe their influence as determinants of the phenomenon of movement. Moreover, the experiment is intended to throw light on the development and decay of the configuration or pattern of movement, in so far as it is manifested in a change of clearness during repetition.

Instructions. "You will be shown a certain movement a number of times. At the end of every 5 exposures when you hear the signal 'now' make a judgment on the basis of the clearness of the movement experience according to the following scale: Report: 1, if the movement has maximal clearness; 2, if the movement is clear but not quite maximally clear; 3, if the movement is moderately clear; 4, if the movement tends to be vague and uncertain yet fairly definite; 5, if the movement is positively unclear, uncertain, and you are not even sure which way it goes."

Later during the preliminary trials when *O* was presented with a different arrangement, additional instructions were given, namely: "Let your judgment be a comparative one, that is to say in each series let 1 stand for the most clear movement, 5 for the most unclear movement. 1 and 5 of course need not correspond to each other exactly in any two arrangements, but if you note any marked differences in the maximal clearness between different series please report them. Should you judge 1 at the beginning of a series and later on experience what seems to you to be a clearer perception of the movement, report the fact so that we may correct the scale. However, do not confuse such a case with a case in which the movement is a 1, maximally clear, becomes a less clear 2, and once again becomes a 1 maximally clear."

The *O*s were encouraged to ask questions concerning the instructions. In no case did any *O* experience any noticeable difficulty in carrying out the instructions.

Procedure. The variations of stimuli and background as they pertain respectively to the data shown in Figs. III and IV are as follows:

1 a. Movement of a black radius (No. 1 in Fig. II) and a black diameter (No. 2, Fig. II) on white backgrounds. The arrangements were given in both the 70° acute angle position and in the 110° obtuse angle position (as indicated by oblique black and dotted lines in Nos. 1 and 2, Fig. II). The results represented in the curves of 1a are for ST.

1 b and 1 c. Same as above for BE and FR respectively.

2. Movement of two 'crossed' diameters (No. 3, Fig. II) on a white background.

3. Movement of a 'windmill' (No. 4, Fig. II) on a white background.

4. Movement of a man saluting on a white background (No. 5, Fig. II).

5. Movement of a white diameter on a black background. *Cf.* 1 a above for angle arrangement.

6. Movement of a black diameter on a grey background. Same.

7. Movement of a 'black crochet thread' diameter[7] on a white background. Same.

8. Movement of a 'white crochet thread' on a black background. Same.

Each angle arrangement was repeated in every case until *O* reported 5, or very unclear movement.

The results as graphically represented in 1 a to 8, Figs. III and IV, show the rise and decay of the perception of movement as based on the 5-point judgment scale. The dotted line represents 3 the average or midpoint on the judgment scale. Deviations of the curves above and below the line correspond to changes in relative clearness of the perception of movement.

Summary. (1) There is an increase in the clearness of visual perception of movement up to the 15th or 20th exposure, although the maximum may be reached as early as 10 or as late as 50. This increase in clearness, of course, is subjective and seems to consist of an increase in the perceptibility of the perseveration of the objective stimulus pattern. The duration of the immediate present of the perception of movement seems to increase at first and then with continued repetition to decrease.[8]

(2) The period of greatest clearness of the perception of movement lasts sometimes for only 2 or 3 exposures, sometimes

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[7] The crochet thread diameters were constructed by passing crochet thread (the same width as the ink lines) through two small holes punched in the card, and fastening the loose ends on the back.

[8] It is interesting to compare this conclusion with the work of V. Benussi, Gesetze der inadäquaten Gestaltauffassung, *Arch. f. d. ges. Psychol.*, 32, 1914, 407-408, on the Müller-Lyer illusion, to the effect that voluntary exercise of a synthetic attitude is accompanied by fatigue of an analytic attitude and vice versa. Also that persistence in an attitude tends either to improve or destroy the illusion, according as one or other attitude becomes more and more dominant.

In the beginning of a series of movements under the experimental conditions described above a synthetic attitude seemed to predominate. The movement configuration improved until the onset of fatigue, whereupon the synthetic attitude seemed to break down with a resultant disintegration of the perceptual pattern of movement.

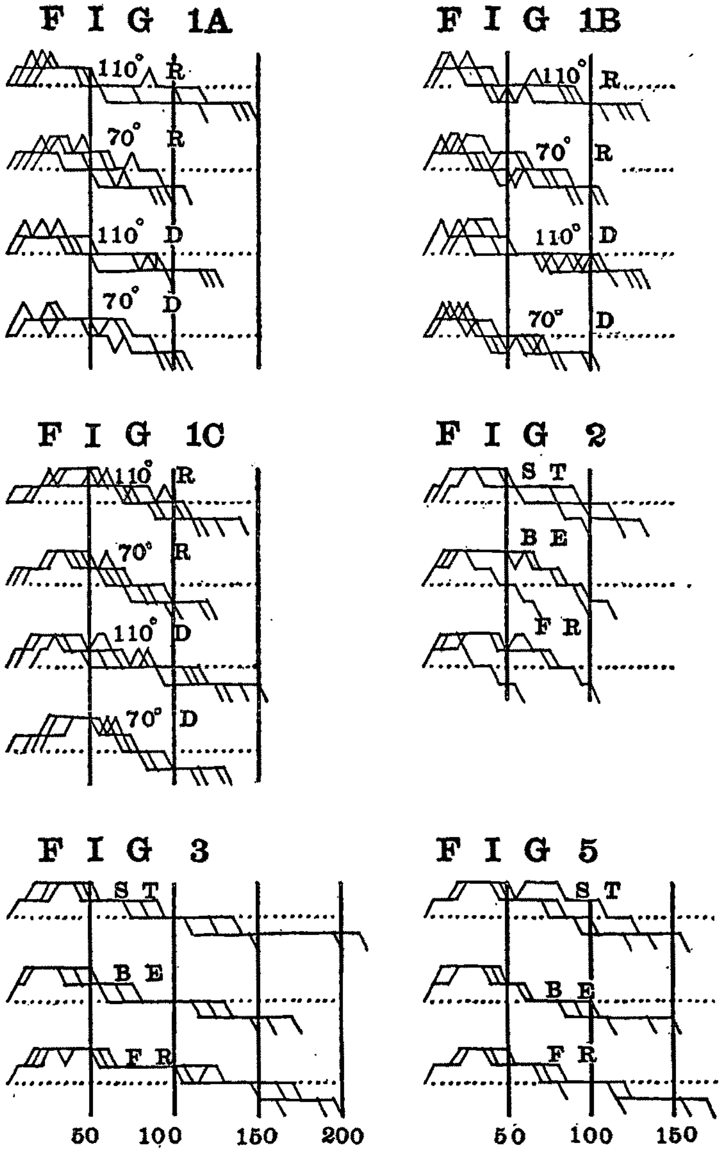

FIG 1A

FIG 1B

FIG 1C

FIG 2

FIG 3

FIG 5

FIG. III. DECAY OF SEEN MOVEMENT WITH SUCCESSIVE EXPOSURES. See "Procedure" in text and Fig. II for illustrations of stimuli. The exposures were continuous and judgments were made every 5 exposures. Abscissae indicate exposures. Ordinates indicate judgments of the perception on the basis of 5 degrees of clearness. The highest points on the curves represent the judgment of 'very clear'; next level 'clear'; next level (corresponding to the dotted line) 'moderately clear'; next level 'vague'; lowest level 'unclear.'

for 60 or 70 exposures. Even when *O* strives to maintain a constant attitude, there is a very obvious irregular variation in the clearness of perceiving the movement. On many occasions the clearness of the experience of movement increased after it had once started on a decline.

FIG 4

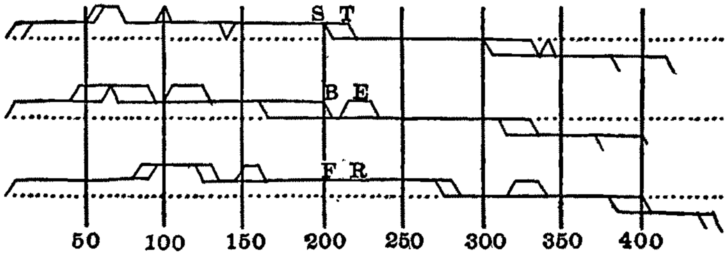

FIG 6

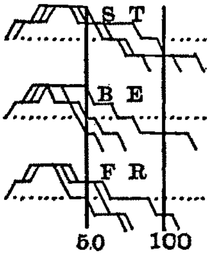

FIG 7

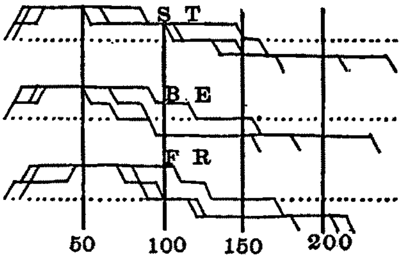

FIG 8

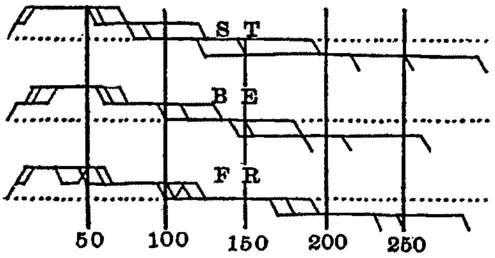

Fig. IV. Decay of Seen Movement with Successive Exposures. See legend to preceding Fig. III.

(3) After movement has become thoroughly unclear one may, by persistence of attention and voluntary effort, cause a revival of perceptibility which may last for 50 or 60 exposures. At about this point the eye-strain and fatigue become so great that the experiment has to be halted.

(4) The movement of a radius seems to persist as long as the movement of a diameter, but the former is never as definite as the latter. Radial movement, because of its 'bareness', simplicity and univocality, permits itself to be analyzed and experienced as 'succession' with greater ease than does diametral movement. It is much more difficult to maintain a constant passive attitude in observing the movement of a radius than in observing the movement of a diameter. Moreover, the diametral movement seems to be more impressive. There is an indication that diametral movement is in general more meaningful.

(5) The movement of two diameters fatigues more slowly than the movement of one diameter. This observation coincides with the results reported below in Section VII,[9] in which the movement of two diameters in the equivocal situation (where an object may move in either of two directions) invariably was found to be the more stable movement arrangement.

(6) The movement of a meaningful animate object is less fatiguing and more durable and insistent than the movement of a less meaningful inanimate object. In the case of the movement of the man saluting, the 'reality' and 'animateness' of the situation seems to hold and centralize the attention of the *O* with the result that the movement persists for a long time. The movement of the man's arm in saluting is a movement of 'something' and compels the attention, thus relieving the *O* from putting forth as much effort, and perhaps also causing him to disregard his fatigue. The movement in the meaningful situation is more real, is psychologically 'filled in' so to speak, and accordingly loses much of the mechanical and artificial character of the movement of 'bare' lines.

(7) The movement of the 'windmill' figure (No. 4, Fig. II) is more durable than the movement of lines and less durable than the movement of a more meaningful object. This difference may be accounted for on the basis of the relative meaningfulness involved. The arrangement with the thickened lines is more meaningful (like a windmill or a cross) than the 'bare' lines, but less interesting and meaningful than the arrangement of the man saluting. Moreover, the lines in the 'windmill' figure which carry the movement, are filled in at the ends. Anything that one does to direct the attention to the ends of the lines by filling them in, or by surrounding the arrangement by a circle,[10] tends to increase the clearness and durability of the movement. The lines thickened at the ends are more impressive than the uniform lines, and when repeated, decay much less

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[9] 494 ff.

[10] Cf. Section IV below, 486 ff.

readily. The windmill movement has more 'body' to it. It can be perceived with less strain because it is more impressive.

(8) A white line on a black background produces a clearer and more durable movement than that produced by a black line on a white background. The white on black is more impressive. The movement is more striking and more deliberate. At first the movement of a white line on black was reported as 'comparatively slow.' With repetition, the movement was reported as 'faster.' As the eyes grew more fatigued the movement became faster and less definite. The movement of white on black is the clearest of all movements. In the case of black on white or of black on grey, with fatigue the black seems to blend with the background, but the white never blends on black even when the movement grows less clear.

(9) The movement of a black line on a grey background is the least durable of all movements. It is never so distinct as the movement on the black background and seems to blend with the background very quickly. The blending or fading takes place more suddenly with a grey background. The grey background proved very easy upon the eyes, producing no noticeable fatigue in spite of the fact that the movement became unclear so quickly.

The influence of background on movement as reported in this section is corroborated in other data reported in the section immediately following.

(10) The use of a solid crochet thread diameter rather than an ink line has the general effect of rendering the movement experience more definite and durable. The movement is more impressive and resists fatigue longer than in the case of an ink line.[11] A solid white object on black evokes the clearest movement. It easily arrests the attention and has a 'resiliency,' as one *O* expressed it.

(11) In 1 a, 1 b and 1 c, Fig. III, movement in a counterclockwise direction is seen to last longer than movement in a clockwise direction, indicating the presence of a predisposition in the *O*s for counterclockwise movement.

III. Influence of Background

Although attention focuses consciousness upon one specific aspect of a total situation, the margin of consciousness and the background of the stimulus-situation exercise a considerable influence. All perceptual experiences have these two distinguishable aspects: a focal impressive structure with very definite features, and a general background from which the first raises itself. The distinction between the figure which appears and the less structured level may be denoted by the words 'figure' and 'ground'.[12] In this section we are concerned with the effects of variations in background on the perception of movement.

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[11] A. Gelb and R. Granit (Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle, *Zsch. f. Psychol.*, 93, 1923, 103-106) found that the limens for figures were greater than limens for grounds, and that the higher values for lines of the figure showed that they resist change more than ground. In the above case, a thread is more *aufdringlich* than a line, actuates more increments of experience, partakes more of the nature of figure, and accordingly, its resistance to change is greater than in the case of a line.

A careful study of the protocols of the *O*s in all experiments indicates that most of the experiences of movement emerge from a total situation and are influenced by the vehicle of movement and the background against which it moves. It is the contention of *E* that the color, consistency and obtrusiveness of the object which moves, and the brightness and impressiveness of the background are interacting factors which must be controlled in an investigation of movement phenomena. Even the 'phi-ness' of *phi*-phenomena seems to vary with the objective conditions of the stimulus and background as well as with the subjective attitude of the *O*.

Stimuli. The following stimuli were used:

(1) Black diametral arrangement on white background (No. 2, Fig. II).

(2) White diametral arrangement on black background (same).

(3) Black diametral arrangement on grey background (same).

(4) Black crochet thread diametral arrangement on white background (same).

(5) White crochet thread diametral arrangement on black background (same).

Procedure. The first arrangement was exposed a few times. Immediately after the last exposure of a series (*i.e.* as quickly as the cards could be changed, an interval ranging between 3 to 7 sec.), the second arrangement was exposed, and the *O* requested to "note all the differences he could between the movement in the first series as compared with the movement in the second series." Several changes of arrangements were made and the *O* given every opportunity to check up and verify any difference he noticed.

During a preliminary series of comparisons, all three *O*s reported something or other concerning a difference in the speed, in the clearness, and in the completeness of the movement. One *O* (BE) reported a difference in 'haziness' in the arc of the movement. Inasmuch as the report of these four differentiae were consistent, they were incorporated into the following *Aufgabe*: "You will compare and report upon the difference, if any, in the speed, the clearness, the completeness of the movement (whether you can follow it easily from beginning to end), and in the 'haziness' of the arc of the movement."

None of the *O*s experienced any particular difficulty in making judgments on the basis of these criteria. The same procedure of comparison was applied to the stimuli listed above, as follows:

Part 1. (1) White line on black background
(2) Black line on white background

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[12] The importance of this division within perception is pointed out by E. Rubin, *Visuell wahrgenommen Figuren*, 1921, vii, 244, who found a characteristic set of properties for figure and another for ground. Koffka, *Psychol. Bull.*, 19, 1923, 567-569, generalizes this distinction by calling the ground a 'level' upon which the figure appears.

Part 2. (1) Black line on white background
(2) Black line on grey background
Part 3. (1) Black line on grey background
(2) White line on black background
Part 4. (1) White crochet thread on black background
(2) Black crochet thread on white background
Part 5. (1) White crochet thread on black background
(2) White line on black background
Part 6. (1) Black crochet thread on white background
(2) Black line on white background

Parts 1, 2, 3, 4, 5 and 6 of Table I summarize the reports of the *O*s.

TABLE I. INFLUENCE OF BACKGROUND ON PERCEPTION OF MOVEMENT. Each part of the table shows the results of a comparison of two stimuli, referred to as '1' and '2,' with respect to four criteria of differences

BASIS OF JUDGMENT	*Observer*		
	ST	BE	FR
PART 1.	1: WHITE LINE ON BLACK GROUND. 2: BLACK LINE ON WHITE GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 more distinct	1 clearer	1 clearer
Completeness of movement	1 whole motion through arc	1 follows through better	1 movement completer
Arc of movement	2 hazier	2 hazier	2 hazier
PART 2.	1: BLACK LINE ON WHITE GROUND. 2: BLACK LINE ON GREY GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 clearer	1 clearer	1 clearer
Completeness of movement	Unable to make judgment	Unable to make judgment	Unable to make judgment
Arc of movement	1 hazier	1 hazier	1 hazier
PART 3.	1: BLACK LINE ON GREY GROUND. 2: WHITE LINE ON BLACK GROUND.		
Speed	1 faster	1 faster	1 faster
Clearness	2 clearer	2 much clearer	2 clearer
Completeness of movement	2 follows all the way	2 completer	2 completer
Arc of movement	1 much hazier	1 hazier	1 much hazier
PART 4.	1: WHITE THREAD ON BLACK GROUND. 2: BLACK THREAD ON WHITE GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 greater contrast	1 stands out	1 clearer
Completeness of movement	1 completer	1 follows through better	1 more definite
Arc of movement	1 no haziness	2 hazier	2 hazier
PART 5.	1: WHITE THREAD ON BLACK GROUND. 2: WHITE LINE ON BLACK GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 clearer	1 clearer	1 clearer
Completeness of movement	1 easier to follow	1 easier to follow	1 easier to follow
Arc of movement	2 hazier	2 hazier	2 hazier
PART 6.	1: BLACK THREAD ON WHITE GROUND. 2: BLACK LINE ON WHITE GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 clearer	1 clearer	1 clearer
Completeness of movement	1 follows through easier	1 completer	1 easier to follow through
Arc of movement	2 hazier	2 hazier	2 hazier

Summary. (1) Movement of white on a black background is clearer and easier to follow than movement of black on a white background.[13] A white line against a black background hesitates at first and then moves deliberately across the field. It is much more difficult to follow the movement of black on a white ground than to follow the movement of white on a black ground. An inverse relationship exists between speed and clearness. The faster the movement, the less clear the phenomenon. On the other hand, if the movement is slower it is more distinct and easier to follow. The movement of white on black is quite impressive and compels the attention more than movement of black on white.

(2) The movement of a black line on a grey ground is not quite as clear and distinct and as easy to follow as the movement of a black line on a white ground. There is, however, comparatively little difference. The grey ground is optically more restful than the white ground, but the *O*s were unable to come to a consistent agreement as to whether the movement was more complete in the one case than in the other. The movement on a white ground tends to be vague, but the movement on a grey ground is more vague. On grey the movement is reported faster than on white, an occurrence which is to be explained by the fact that the black blends a little better with the grey than it does with the white, and also by the fact that because the movement cannot be followed through completely it is judged to be more rapid. The conclusion seems to be, that when a moving object stands out distinctly against a ground we tend to judge it as moving slower than when it blends more completely with the ground.

(3) 'Thingifying' or 'objectifying' a line (*e.g.* thread) diminishes the speed of the movement and at the same time renders the movement clearer and more distinct and easier to follow. Moreover, the haziness in the arc of the movement is reduced. The solidity of an object makes it more real, giving it an *aufdringlich*, 'thing' character which sets it off more sharply against the ground. The movement of an ink-line partakes more of the nature of a 'flash.' The movement of a crochet thread approximates to the actual movement of a real object. There is something very substantial and durable and indubitable

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[13] An unpublished experiment of L. Hartman, reported by Koffka, *op. cit.*, 565 ff., is interesting in this connection. Hartman worked with a Schumann tachistoscope with rotating disc (containing windows) instead of a falling shutter as used in this investigation. He discovered that the time of fusion as measured by the critical speed (that speed which is just capable of bringing about complete fusion after the last bit of flicker has disappeared) of a black cross on a white ground was greater than the time of fusion for a white cross on a black ground. This is used by Koffka to point out the close relation between 'formedness' and 'vividness.'

about the movement of a solid line. The perceptual experience attending the movement of a thread is 'realer' and is better 'structured' than the movement of a line.

(4) The 'haziness' in the arc of the movement is most evident on a grey background and least evident on a black background. There seems to be a considerable qualitative or attributive difference between a movement experience evoked on a black ground as compared with one produced on a white ground. On black there is less of the attribute of 'greyness' and more of the attribute of 'protensity.' Moreover, it is the observation of *E* that there is very little 'flickering' or 'shimmering'[14] apparent in the case of movement of white on black, although something of the sort of thing described by the above words is noticeable in the case of movement of black on white.

IV. INFLUENCE OF ENCOMPASSMENT

By drawing a line about an object we tend to mark it off from the rest of the total visual field. Our attention is drawn to it more easily and it stands out more clearly in consciousness. Even the space inside of a line or structure is more impressive and actually seems whiter.[15]

The experiment reported in this section concerns the influence of circumscribing the objective movement arrangement with a line.

Procedure. The following angle arrangements were used in this experiment:

Arrangement 1. Same as No. 2 in Fig. II.

Arrangement 2. Same angle arrangement, bounded by a circular line whose diameter was equal to the length of the lines in the angle arrangement.

Arrangement 3. Same arrangement as in 1, except bounded by a circle whose diameter was one half again as large as the length of the lines in the angle arrangement.

The instructions for Arrangements 1 and 2 were: "You will be shown a movement of a line. Immediately after a series of such movements you will be shown movement of a line within a circle. Compare the speed, clearness, completeness of movement, and haziness of the arc of the movement, in the case of movement of the line which is circumscribed, with movement of the line which is not circumscribed."

The *Os* were later given the same instructions for a comparison of Arrangements 1 and 3.

Because of lack of space it is necessary to omit the protocols.

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[14] Descriptive terms used by Dimmick's *Os*, *op. cit.*, 332.

[15] F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmungen: Einige Beobachtungen über die Zusammenfassung von Gesichtseindrücken zu Einheiten, *Zsch. f. Psychol.*, 23, 1900, 1-32, reported that the white space inside a figure is 'whiter' and more 'live' than that outside of the figure.

On practically all commercial moving picture curtains there is a dark border surrounding the limits of the frame of the picture for much the same reason.

Summary. (1) The protocols indicate that movement is more clearly perceived when surrounded by some fixed contiguous frame of reference. The ends of the lines carry the movement, and any addition to the total situation that tends to narrow the perceptual field and to accentuate the peripheral phase of rotary movement facilitates the general perception of the movement.[16] The effect produced is related to the effect produced by *Nebenreize* or *Nebeneindrücke* as reported in investigations of optical illusions; also, to 'pregnance of configuration'[17] as reported by configurationists. Under the conditions of this investigation, the movement of a line within a circle does not carry the movement over to the circle itself; that is to say, the line slides around inside of the circle. The circle itself does not move.

(2) Encompassing the angle arrangement by a larger circle, which is not actually contiguous to any part of the object which moves, also produces a clearer perceptual configuration of movement.[18] For reasons which seem indescribable, circumscription of the objective pattern of movement tends to bring the perceptual pattern of movement subjectively nearer and to render it more clear and complete. The effect is somewhat similar to that which one gets in framing a picture. A frame tends to compel the attention to the picture and causes it to stand out more definitely in consciousness. The limitation or bounding of the field in which something moves seems to assist the organism in limiting the field of consciousness, resulting in a higher degree of concentration upon the ground within the boundary, and, in the case of movement, producing a clearer and more impressive experience of movement.

V. INFLUENCE OF POSITION

The object of this experiment was to ascertain the part played by the factor of position as a determinant in rotary movement. Everyday experience leads us to expect that it is just as easy to perceive rotary movement in one phase of its total revolution as to perceive it in another phase. By varying the position of the lines in the angle arrangements and placing the movement experience under more critical descriptive conditions, lateral movement (*i.e.* on the sides) was contrasted with transverse or crosswise movement (*i.e.* above and below). More specifically, this procedure involved a study of the compulsoriness of the direction taken by movement starting from all positions in the circular field.

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[16] This seems to accord with the finding of T. Lipps, *Leitfaden der Psychologie*, 1909, 193, that when elements are used in a group they make less claim on perceptual activity (*Auffassungstätigkeit*), especially when they are in close union with other members of the group.

[17] W. Fuchs, Die totalisierende Gestaltauffassung, *Zsch. f. Psychol.*, 86, 1921, 8-16, shows that well known objects do not complete themselves unless they are 'pregnant' configurations. For instance, a straight line is not complete whereas a circle is. The head of a dog is not supplied in a drawing which lacks it. A line, forming a part of a good or 'pregnant' configuration like a star, is filled in providing it forms a necessary part of the configuration.

[18] Rubin, *op. cit.*, 130, throws some light on this question of 'structural clearness' in reference to contours.

Procedure. Diametral arrangements as shown in the charts of Fig. V were presented to the *O*s in the order shown and in the reverse order. The method of limits was employed because it was the most convenient.

The *O*s were instructed: "Keep your eyes fixated steadily upon the point and observe and report the direction of the movement. Maintain, if possible, a neutral receptive attitude and in no way attempt to control the direction of the movement."

According to the results of the experiment, as presented in the Fig. V, rotary movement takes place more readily laterally than transversely.

Summary. (1) The results portrayed in the form of curves prove that the compulsoriness of rotary movement is not uniform at every position. The data represented in Fig. V demonstrate a decided preference on the part of the *O*s for lateral movement over transverse movement. This assumption was further corroborated through confrontation. The *O*s were shown the facts and asked for an opinion. Every *O* admitted that lateral movement was more compulsory than transverse movement. Furthermore, the *O*s noticed an inhibiting tendency in the vicinity of the vertical plane; that is to say, the direction of the movement tended to be reversed if the initial position of the line which moved was changed from right to left, or from left to right, of the vertical plane.

(2) Any explanation of this tendency seems to involve the question of general spatial level,[19] and of empathy or *Einfühlung*. Civilized man stands upright, is always aware to some degree of his vertical position and constantly and automatically adjusts for deviations therefrom. Likewise, as shown in sculpture and painting, we seem to have a tendency to judge figures and objects automatically with respect to their vertical position. The vertical position is the position of equilibrium. A line slightly off the vertical is potentially not in a position of equilibrium and by empathy we expect it to fall and regain a more stable position. In diametral movement the attention is nearly always directed upon that part of the line above the axis, and it

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[19] Koffka, *op. cit.*, 572, points out that "the spatial level (*Raumlage*) has a marked tendency to remain constant, together with common directions of 'above', 'below', 'right' and 'left'." Again, *op. cit.*, 577: "We possess a general spatial level within which we are anchored." Cf. G. E. Müller, Ueber das Aubertsche Phänomen, *Zsch. f. Sinnesphysiol.*, 49, 1916, 109-244. Müller has made extensive investigations of the phenomenon.

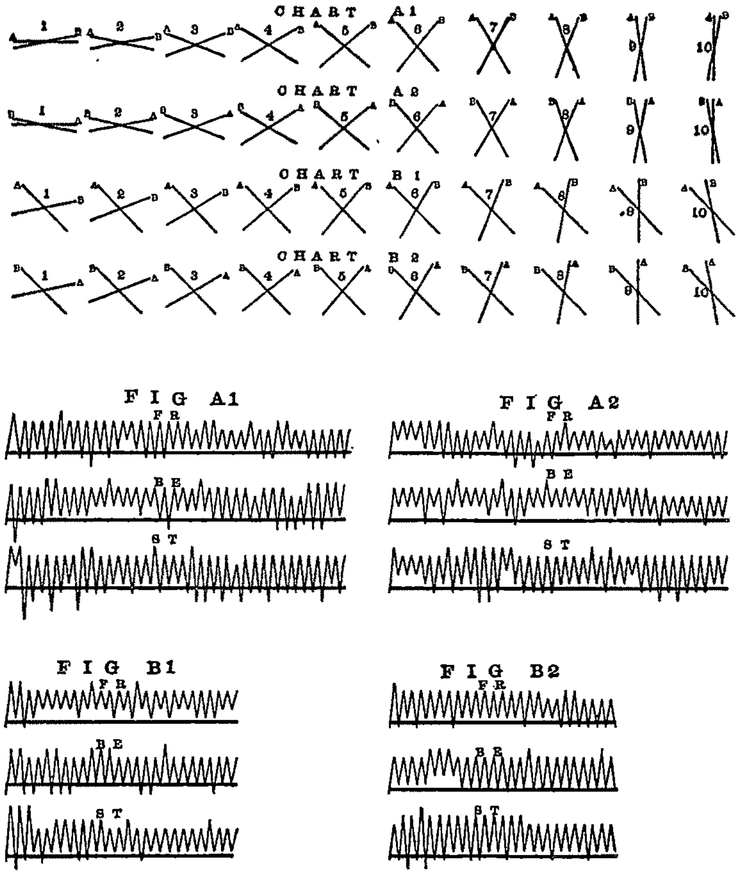

Fig. V. Predominance of Lateral Movement over Transverse Movement.

The ordinates in Figs. A1, A2, B1, and B2 represent series of angle arrangements from 1 to 10 as illustrated respectively in Charts A1, A2, B1 and B2. The heavy horizontal line indicates the midpoint of the series, that is to say, angle arrangements 6 to 10 stand above the line while arrangements 1 to 5 stand below the line.

The abscissae represent exposures. Five exposures of each angle arrangement were given, beginning with No. 5 and proceeding upward until the limiting series was reached, then downward until the lower limiting series was reached, *etc.* The limiting series was that angle arrangement in which the movement, if clockwise, became counterclockwise, or, if counterclockwise, became clockwise.

In Charts A1 and B1 the arrangement of the series is such that the lateral movement of line A to line B is counterclockwise. In Figs. A1 and B1 the ascending lines indicate counterclockwise movement, the descending lines clockwise movement. The case is exactly the reverse with Figs. A2 and B2, that is to say, the lateral movement of line A to line B is clockwise, and the ascending lines in the Figs. A2 and B2 represent clockwise movement.

If position exerted no influence on the direction taken by the movement in the various series of angle arrangements, the limiting series on the ascending and descending lines of the Figs. might be expected to be equidistant from the heavy line. The uneven distribution of the curves above the heavy line is indicative of the significant influence of position.

is the upper part of the line which tends to fall when it is not in a state of vertical equilibrium.

VI. INFLUENCE OF FIXATION POINT

One of the constants in all the experiments was the fixation point. Invariably it was made to coincide with the junction of the horizontal and upright lines. A new *O* in tachistoscopic observation always has difficulty in holding his gaze steadily and continuously upon the fixation point. Even with *O* trained in tachistoscopic observation one may question whether the artificial conditions attending the production of apparent movement may not cause him to vary his gaze ever so little. The only remedy for error of this kind, under the conditions of tachistoscopic experimentation, is to train the *O* to notice and report lapses and wanderings of fixation and attention. It is the belief of *E* that a trained *O* can maintain a relatively constant fixation when he is fresh, but that after a certain amount of repetition with resultant fatigue, a slight relaxing of attention is probable in spite of the best of good faith. If there is this occasional wandering of fixation to some other point or points in the stimulus field, we should endeavor to control it or at the very least determine what effect it has upon the movement experience. The data accumulated in this section were derived from a study of the stimulus and non-stimulus determinants of movement arising out of a variation of the objective fixation point.

Procedure and Results. Five fixation points besides the center point were used (see No. 8 in Fig. II), viz., center-left, upper-left, upper-center, upper-right, center-right. All of these fixation points were located along the circumference of a circle with a radius of the same length as the radial lines in the angle arrangements. The center-left and center-right fixation points were on a line with the center fixation point. The upper-center fixation point was directly above the center point, and the upper-right and upper-left points were at an angle of 45° with the horizontal.

A radial arrangement (No. 6, Fig. II) was adopted, consisting of a horizontal line 2 cm. long whose mid-point was coincident with a vertical line 1 cm. long.

The instructions were: "Throughout this experiment focus your gaze on the fixation point no matter where it appears. Try to keep your attention upon the point but notice and report the direction of the movement. Make every effort to hold your gaze upon the fixation point and report any lapses."

If at first the movement was clockwise down to the right then the upper-left fixation point was introduced. With the best efforts to fixate the new point, usually no change of direction was reported in the beginning. In this case 10 exposures of a left oblique radial angle arrangement (No. 1, Fig. II) were given. The direction of the movement was compulsorily counterclockwise and remained counterclockwise when the vertical radial arrangement was again inserted.

At this stage, the upper-right fixation point was inserted in an effort to reverse the direction of the movement. If reversal took place, the upper-left fixation point was again inserted and alternated regularly with the upper-right fixation point, until the direction of the movement changed automatically with the change of the fixation point. If the reversal did not occur, a right oblique angle arrangement (dotted line of No. 1, Fig. II) was introduced intermittently to bring about a clockwise set.

Repetition of the above procedure was continued until eventually the upper-right and upper-left fixation points came to be in themselves sufficient to reverse the direction of the movement in the equivocal situation.[20] Occasionally, after the first day, an oblique arrangement had to be introduced to reverse the direction of the movement. However, having once trained an *O* to attend properly, the fixation point proved adequate to vary the direction of the movement.

The same procedure was followed for the center-right and center-left fixation points.

Finally, after the *O*s were well trained in the use of variable fixation points, they were asked to compare the effectiveness of the upper-right and upper-left fixation points with the effectiveness of the center-right and center-left fixation points, in rendering the direction of the movement compulsory. In every case the *O*s reported that the center-right and the center-left fixation points were more effective in bringing about an immediate and clear reversal of movement than the upper-right and upper-left fixation points. The reason obviously seems to be that the movement is more clearly perceived when one fixates the point where the movement is completed. Upon confrontation with the data the *O*s corroborated the above supposition.

As a further check upon this conjecture the vertical angle arrangement was presented in reverse order, the horizontal line first and the vertical radius second (cf. No. 6, Fig. II). The same general procedure used for the first radial arrangement was followed, and an endeavor made to determine the relative effectiveness of the five circumferential fixation points shown in diagram 8, Fig. II, in producing clear movement from the equivocal arrangement above. Invariably, the upper-center fixation point was declared to be the most effective in producing clear movement. In the latter case such phrases as "snaps into position", "comes up with a crash", were used in describing the movement. When the center-left and center-right fixation points were utilized it was easy to perceive the start of the movement, but the completion of the movement was extremely vague. Fixation upon the upper-left and upper-right fixation points enabled the *O*s to grasp the whole of the movement fairly well but the end of the movement was found to be vague. Clear perception of the conclusion of the movement seems to be an important factor in 'structuring' of the movement pattern. Fixation upon the point of final rest seems to correspond to what Wertheimer calls the *Schwerpunkt,* and proves to be the most effective of the circumferential points in the production of clear movement. The beginning of the movement is *aufdringlich,* that is to say, it compels the attention naturally, but if the *O* is not able to perceive clearly the fading out of the movement, which is not so spontaneously obtrusive, he is unable to grasp the movement so well as a totality.

This general principle holds only in the case of rotary movement through arcs of 90° or less. It does not hold so clearly in the case of rotary movement through an arc of more than 90°. Movement through obtuse angles is more clearly perceived if the intermediate fixation points (upper-right and upper-left) are used, because if the gaze is directed upon the center-right fixation point in an obtuse angle arrangement the central end of the left oblique line often slides out sideways to the right instead of remaining in place and acting as an axis.

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[20] Koffka, *op. cit.*, 578, refers to a change of this kind as a change of points of anchorage (*Verankerungspunkte*).

A comparison of the effectiveness of the center fixation point with the peripheral fixation points is in favor of the center point except in the case where the peripheral point is located at, or very near to, the point where the movement is completed. Movement is carried by the outer ends of lines and the peripheral position where the movement is completed seems to be the most effective point of regard for the perception of clearest movement. The center fixation point is more effective than a point at the beginning or at intermediate positions on the peripheral arc of movement. There is, after all, no great difference in the relative effectiveness of the center fixation point and the peripheral point which coincides with the completion of the movement, but the advantage seems to be with the peripheral point.[21]

No effort was made to test out, with reference to linear direct movement, the principle that movement is most clearly perceived when one fixates the point where the movement is completed. However, the facts of this experiment indicate that the principle might hold equally well in the case of any other kind of movement.

Summary. It was found that change of the *O*'s fixation point varies significantly the gross perception of movement, *i.e.* movement seems to appear better where there is the greatest concentration of attention, but, owing to the fact that the starting position of a presentation of movement is the most impressive position for the human organism, the most favorable position of fixation for perceiving a presentation of movement as a whole is at or near the position of completion of the movement.

VII. Influence of Form of Stimulus on Direction of Movement

Although movement perceived under tachistoscopic conditions for optimal time is practically compulsory, the direction taken by the movement is not always compulsory. There are equivocal situations in which the direction of movement seems to be wholly a matter of chance. For instance, in the vertical radial angle arrangement (No. 6, Fig. II) the line 'A' appearing first and followed by line 'BB,' sometimes revolves clockwise, and sometimes counterclockwise, to 'BB.'

This arrangement was presented to the *O*s at the beginning of an experimental hour on 12 different occasions in order to determine if possible their natural preference for one direction over the other direction. The only instructions were: "Report the direction taken by the movement."

	ST	BE	FR
Clockwise movement	9	4	5
Counterclockwise movement	3	8	7

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[21] This conclusion corroborates the statement of Wertheimer (*op. cit.*, 207) to the effect that movements are not due to processes at the periphery of attention, but that movements appear better where there is the greatest concentration of attention. However, these data controvert the statement made by Wertheimer that "the nearer attention is to the apex of the angle the better the movement" (*op. cit.*, 207).

These figures indicate the number of times the direction of the movement was reported clockwise or counterclockwise, without the *O*s being influenced in any way (as far as one could tell without attempting to control the directional pre-perception) by a rotary movement perception contiguous in time, that might set up a directional predisposition.

The results are equivocal. No definite tendency, either clockwise or counterclockwise, is apparent.

A variation of the above experiment consisted in the use of a vertical diametral arrangement (No. 7, Fig. II).

The procedure was the same and the results are presented below.

	ST	BE	FR
Clockwise movement	5	3	1
Counterclockwise movement	7	9	11

A very definite counterclockwise tendency is exhibited in a movement evoked by the diametral arrangement, a finding which corroborates results in Section II.[22] No definite *Aufgabe* was used in this preliminary experiment and the varied results are probably due to the variation of uncontrolled attitudes. It is interesting to note that movement of diameters is less subject to this variation than movement of radii, a fact which supports similar conclusions obtained under different conditions, as reported below in Section IX.[23]

Using these preliminary experiments as a starting point the following more systematic investigation was formulated in an effort to determine the influence of any permanent directional predispositions.

Procedure. The instructions were: "Observe the movement reporting the direction it takes, or, in case of 'splitting' report, 'split.' Maintain a neutral receptive attitude if you can, neither facilitating nor hindering the movement experience. Report any occasion in which you find yourself disobeying directions." The *O*s experienced little difficulty in adopting the receptive attitude and reported few cases of disobedience.

The results are shown in Tables II and III, where the nature of the stimuli is also sufficiently indicated. The vertical stimulus was presented first in Parts 1, 2, and 5, and the horizontal stimulus first in Parts 3 and 4.

The seen movement was clockwise, counterwise, or 'splitting,' *i.e.* the line split for the equivocal stimulus and moved in both directions, or at least there was movement and the direction of movement was equivocal. Where the two columns of Tables II and III do not total 25, it is to be understood that the remainder of the 25 cases were 'splitting' movements.

A clockwise or counterclockwise predisposition was established by a continuous preliminary series of 10 exposures in which the angle was reduced by 10° in the direction in which predisposition was desired. Thus a clockwise predisposition was set up by preceding stimulus No. 6, Fig. II, by a similar stimulus in which the vertical line made an angle of 80° at the right and 100° at the left (instead of 90° in both cases) with the horizontal line. Movement through an acute angle, even at 80°, is compulsory as against the complementary obtuse angle.

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[22] 481 ff. [23] 497 ff.

TABLE II. MOVEMENT FOR AN EQUIVOCAL STIMULUS AS DEPENDENT UPON PREDISPOSITION. For nature of equivocal stimuli see 6, 7, and 3 of Fig. II. Predisposition established by 10 preliminary exposures, 10° more acute than equivocal angle (movement is compulsory through an acute angle as against an obtuse). C = clockwise seen movement. CC = counterclockwise movement. 25 cases in each set. Where the C and CC columns do not total 25, the remainder were 'splitting' movements (both directions). The 10 pairs of figures in each block of the table represents 10 experiments of 25 cases each under the same conditions, with predisposition established anew in each of the 10 experiments.

Observer	ST						BE						FR					
Stimulus No. See Fig. II	No. 6		No. 7		No. 3		No. 6		No. 7		No. 3		No. 6		No. 7		No. 3	
Direction of seen movement	CC	C	CC	C	CC	C	CC	C	CC	C	CC	C	CC	C	CC	C	CC	C
Part 1 Clockwise predisposition Vert. stim. precedes horiz.	13	12	0	25	0	25	5	10	5	20	0	25	16	5	0	25	0	25
	18	7	0	25	0	25	7	13	5	15	0	25	10	10	0	25	0	25
	10	15	5	20	0	25	10	12	13	7	0	25	8	5	23	2	0	25
	11	14	3	22	0	25	2	18	3	15	0	25	6	2	0	25	0	25
	7	18	0	25	0	25	10	10	7	10	0	25	0	2	0	25	0	25
	23	2	0	25	0	25	2	15	10	10	0	25	0	18	0	25	0	25
	10	15	0	25	0	25	10	13	2	20	0	25	0	15	0	25	0	25
	16	9	0	25	0	25	8	14	0	24	0	25	0	17	0	25	0	25
	13	3	2	23	0	25	15	10	13	5	0	25	2	14	2	20	0	25
	11	14	1	24	0	25	8	15	0	25	0	25	10	5	0	25	0	25
Part 2 Counterclockwise predisposition Vert. stim. precedes horiz.	10	15	25	0	25	0	12	5	25	0	25	0	25	0	25	0	25	0
	8	17	14	11	25	0	20	0	21	0	25	0	15	0	25	0	25	0
	11	14	18	7	25	0	15	0	12	8	25	0	20	0	25	0	25	0
	12	6	10	15	25	0	17	0	23	0	25	0	3	0	20	0	25	0
	7	13	5	20	25	0	15	0	25	0	25	0	22	0	25	0	25	0
	8	15	25	0	25	0	18	2	20	0	25	0	15	5	20	0	25	0
	7	5	7	18	25	0	16	6	25	0	25	0	21	0	22	0	25	0
	8	5	25	0	25	0	12	8	18	5	25	0	20	0	25	0	25	0
	10	6	17	8	25	0	16	0	15	10	25	0	24	0	21	0	25	0
	17	5	14	11	25	0	10	4	19		25	0	25	0	17	0	25	0
Part 3 Clockwise predisposition Horiz. stim. precedes vert.	11	14	12	13	0	25	12	8	13	12	0	25	0	20	0	25	0	25
	10	15	15	10	0	25	15	5	8	7	0	25	0	15	0	25	0	25
	8	17	0	25	0	25	20	0	12	5	0	25	0	3	0	25	0	25
	10	15	15	10	0	25	18	2	20	0	0	25	0	20	0	25	0	25
	5	20	5	20	0	25	0	5	0	25	0	25	0	10	0	25	0	25
	7	8	0	25	0	25	0	4	0	25	0	25	0	13	0	25	0	25
	8	15	9	16	0	25	0	2	0	25	0	25	0	5	0	25	0	25
	4	18	0	25	0	25	5	13	2	18	0	25	0	12	0	25	0	25
	0	20	7	18	0	25	2	20	7	15	0	25	0	17	0	25	0	25
	11	14	2	23	0	25	0	17	10	10	0	25	0	20	0	25	0	25
Part 4 Counterclockwise predisposition Horiz. stim. precedes vert.	20	5	13	12	25	0	18	3	15	10	25	0	5	0	25	0	25	0
	10	15	25	0	25	0	20	0	25	0	25	0	2	0	25	0	25	0
	18	7	25	0	25	0	18	2	5	8	25	0	2	0	25	0	25	0
	14	11	25	0	25	0	20	0	25	0	25	0	10	0	25	0	25	0
	23	2	25	0	25	0	3	0	25	0	25	0	2	0	25	0	25	0
	20	5	25	0	25	0	4	1	23	0	25	0	17	0	14	0	25	0
	12	13	25	0	25	0	2	0	20	0	25	0	10	0	20	0	25	0
	10	15	25	0	25	0	12	0	18	2	25	0	14	0	18	0	25	0
	19	6	25	0	25	0	16	2	17	0	25	0	13	2	25	0	25	0
	17	8	25	0	25	0	19	0	20	0	25	0	20	0	20	0	25	0

TABLE III. MOVEMENT FOR AN EQUIVOCAL STIMULUS AS DEPENDENT UPON PREDISPOSITION. Same as Table II, *q. v.*, except that stimulus is two paddles of No. 4, Fig. II, arranged as in No. 7, Fig. II.

Observer	ST		BE		FR	
Direction of seen movement	CC	C	CC	C	CC	C
Part 5 Clockwise predisposition Vert. stim. precedes horiz.	0	25	0	25	0	25
	0	25	0	25	0	25
	0	25	0	25	0	25
	0	25	0	25	0	25
	0	25	0	25	0	25
	0	25	0	25	0	25
	0	25	0	25	0	25
	0	25	0	25	0	25

Summary. (1) The tendency for a line to move counterclockwise in the equivocal situation (where the first appearing line may move in either of two directions) is much more marked than the tendency for it to move clockwise. Even a set of 10 exposures of a clockwise movement does not seem to have any considerable influence in facilitating the clockwise movement in the repetition of an equivocal angle arrangement where the direction taken by the movement is in no way controlled by the objective stimulus.

(2) The direction of the movement of a vertical radius to a horizontal diameter, or of a horizontal diameter to a vertical radius, was found to be the least compulsory and the least determinate of all movements. This particular arrangement is so simple that apparently the slightest variation in attitude, eye-movement or fixation reverses the direction of the movement or else evokes a 'splitting' phenomenon.

(3) The direction of movement of one diameter is more compulsory than the movement of one radius. Two movements, one on each side of the axis, of course are involved in the case of diametral movement, but nevertheless, splitting and uncertainty sometimes occur. The movement of this arrangement is much more determinate and the directional tendency much more compulsory, yet attitude and variation of attention are able to exercise an influence in spite of the best efforts on the part of the *O*.

(4) The direction of movement of two diameters at right angles to each other is more compulsory than the movement of one diameter. In no case was there a variation from the direction initiated by the predisposition. The *O*s remarked on the fact that the movement of two diameters had a 'compulsory' character to it. Later on, after the regular experiment was concluded, the *O*s were given these angle arrangements and asked to alternate voluntarily the direction of the movement. After a considerable amount of practice, they were able to predetermine the direction of the movement, but only with the greatest difficulty and strain. Voluntary predetermination of direction was found to be much more difficult in the case of movement of two diameters than in the case of movement of one diameter.

(5) Table III brings out the fact that thickening the ends of the lines stabilizes the directional tendency of movement. Increasing the relative size of the ends of the lines seems to result in discounting the effects of attitudes, fatigue and variation of attention, and makes it more difficult for the *O* to control and vary the direction of movement. As was observed in an earlier section, the ends of rotating lines carry the movement and therefore thickening of the ends of the lines makes the

movement more definite, clear-cut, 'intense', and probably more meaningful due to the similarity to a windmill. Once started, such a movement seems to 'stamp in' the perceptual directional configuration.

(6) It would seem therefore, that the stimulus pattern is closely correlated with the subjective configuration, and that the form, complexity and meaningfulness of the stimulus may and often do determine the quality as well as the compulsoriness of the direction of the movement perception. One *O* reported that, when he set himself very conscientiously to his task, 'splitting' movements tended to be eliminated. The reversals of direction of movement came suddenly, but the degree of compulsoriness seemed to vary from time to time with change of attitude.

VIII. INFLUENCE OF SPLITTING

The use of an equivocal situation (No. 6 in Fig. II), in which the movement may go in either a clockwise or counterclockwise direction, sometimes gives the unique experience where the movement splits and goes fanwise in two directions. The object of the experiment of this section was to analyze and describe this experience.

Procedure. Radial and diametral arrangements as represented in Nos. 6 and 7, Fig. II, were presented to the *O*s repeatedly. The instructions were as follows: "Designate the direction of the movement, or, in case of splitting, report 'split'." Whenever an *O* reported 'split' he was requested to explain as well as he could the nature of the splitting movement.

Owing to limits of space it is necessary to omit the protocols, but the following summary is based on explicit reports.

Summary. (1) Splitting movements are due to a variety of factors, both subjective and objective (*subjective:* variation of attitude, wandering of attention, variation of focus and distribution of attention, 'trying' to break up the existent directional tendency, fatigue, relaxation of visual accommodation; *objective:* reduction of the complexity and meaningfulness of the vehicle of movement, and, of course, an equivocal situation).

(2) This examination of splitting movements brings to light a multiplicity of determinants. The description of the splitting movement by the *O*s bears a remarkable resemblance to 'bare,' 'objectless' movement, that ultimate unit of visual experience denominated by Wertheimer as the 'pure' *phi*-phenomenon. However, unlike the 'pure' *phi*-phenomenon of Wertheimer, splitting perceptions were found to depend upon proper experimental conditions and also to be controlled to a considerable extent by *Aufgaben.* Subjectively, the splitting movement seemed to be brought to the fore when the image of the first line of the arrangement persisted in consciousness and when attention was taken from the 'reality' of the vehicle of movement and the ground against which it moved.

(3) To throw more light on this question, *E* set himself to observe the movement of acute angles, under varying conditions of visual accommodation. He believes that he developed the ability, after much practice, to experience the movement while accommodating to far vision, and to compare it with the experience of the movement when accommodating to near vision. In the case of accommodation to distance, the movement appeared vague and 'flashy.' A phenomenological description of the movement under such conditions would approximate to the description of Wertheimer's 'pure' *phi*-phenomenon, as movement divorced from anything that moved.[24] Such a statement hardly makes sense, but phenomenologically something of the sort is what one occasionally experiences.

While accommodating for the object which moved and forcing the attention upon the existential situation, *E* was unable to experience anything which subjectively corresponded to 'pure' *phi*-movement. However, by accommodating to distance vision, turning the attention away from the existential situation, and assuming a passive receptive attitude, he was able to experience something resembling pure 'flashiness' or 'flashy filminess' with little other accompanying context.

In sum therefore, splitting movements are vague movements, perhaps partly unlocalized in space, dependent largely upon and controlled to a considerable extent by fatigue and other subjective factors. Moreover, it seems to *E*, that there is a remarkable similarity between the description of the splitting movements observed in this experiment and the 'pure' *phi*-phenomenon observed and reported by Wertheimer's *O*s. The results indicate that further investigation might yield further determinants which would enable us to predict and control all 'pure' *phi*-movements.

Because of its unique nature it is suggested that 'splitting movement' be considered as a separate kind of movement in any future classification of varieties of visual movement.

IX. INFLUENCE OF VOLUNTARY PREDISPOSITIONS ON THE DIRECTION TAKEN BY THE MOVEMENT

It was the purpose of the experiment of this section to investigate the dependence of the perception of the direction of movement upon conditions not determined by the stimulus, that is to say, the extent to which the *O* has control over the direction taken by apparent movement.

Procedure A. A left oblique angle arrangement (No. 1, Fig. II) was shown, and *O* requested to alternate the movement, now counterclockwise, now clockwise, now counterclockwise, now clockwise, etc. He was directed to use any method whatsoever in his effort to control the direction of the movement. If *O* was unable to make the movement carry across the obtuse angle to the right, an oblique angle arrangement with an acute angle to the right (dotted line, No. 1, Fig. II) was introduced in order to facilitate the establishment of a clockwise directional tendency. As these two angle arrangements at first evoked compulsory movement in opposite directions, they were exhibited to *O* in whatever order he chose in order to assist him in his effort to overcome the directional compulsoriness of the stimulus, and thereby to aid him to achieve voluntary control over the direction of the movement across an obtuse angle. After a surprisingly short time, all *O*s found themselves able to alternate at will the direction of the movement, now over the obtuse angle, now over the acute angle, etc.

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[24] Wertheimer, *op. cit.*, 213.

Procedure B. Besides the radial arrangements, the *O*s were shown diametral arrangements (No. 2, Fig. II), double diameter arrangements (No. 3, Fig. II) and 'windmill' arrangements (No. 4, Fig. II).

The instructions were: "Compare and report the differences, if any, in the ease or facility with which you find yourself able to control the direction of the movement in the different arrangements."

The following summary is based upon explicit protocols.

Summary. (1) Voluntary control of the direction taken by rotary movement is easiest in the case of radial and diametral arrangements. The movement of two diameters, and of two diameters thickened at the ends, is much more difficult to control. The 'bareness' and simplicity of the movement of a radius or of a diameter lends itself more readily to change of direction. The movement of two diameters and of a 'windmill' is much more complicated and involves many more phases of movement. The 'windmill' movement has more of a 'body' or 'thing' character to it. The bare lines have more of a 'film'[25] character and are not so attention-compelling. The 'windmill' movement is not so much 'a movement' as 'the movement of a windmill,' or 'the movement of thick lines.' Bare lines seem unmeaningful in much the same way that nonsense syllables are unmeaningful. We are aware of the lines and aware that they move, but it is not exactly the same as if something real moved. In the case of the 'windmill' arrangement the movement is indubitably 'realer.' The movement attracts the attention more naturally just as a real word may attract the attention more effectively than a nonsense syllable. The nature of the influence of meaningfulness on the phenomenon of apparent movement is very difficult to describe, yet there is something here quite obvious and quite real to the *O*. It resists verbal analysis and is very difficult to approach, because it concerns the core of psychological 'meaning,' which presents as yet much mystery to experimental psychology.

(2) The *O*s reported voluntary predetermination of movement to be very fatiguing. An actual physical strain is involved.

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[25] Resembling the *Flächenfarbe* described by D. Katz, Die Erscheinungsweisen der Farben, *Zsch. f. Psychol.*, Erg. Bd. 7, 1911, 6-12.

All the forces of *O* seem focussed upon the problem and the fatigue is a muscular fatigue of the entire organism. This voluntary alternation of movement at first seems to involve the 'will' or voluntary power of *O* in a more complete degree than is necessary in ordinary receptive observation. It involves a conscious psychosomatic straining on the part of *O*. This experience and the experience engendered by the ordinary receptive neutral attitude seem to be just as wide apart attitudinally, as the experience of a man who is creating something (book, poem, painting), and the experience of a man who is merely reading a book or absorbing casual information of any sort.

(3) Kinesthesis in eyeballs, face, neck and head as accompaniments of movements seems to vary considerably among different *O*s. These kinesthetic components of voluntary effort are among the conditioning factors of movement, but they are not constant nor found present with all *O*s, and therefore are not necessarily to be regarded as the outstanding conditioning factors.

(4) Our regular *O*s and four other casual *O*s who served in this experiment found counterclockwise movement more compulsory than clockwise movement. All *O*s were right-eyed and right-handed.

(5) The question of right-handedness and right-eyedness seemed an interesting cue and two left-handed students were induced to observe. Experimentation with equivocal situations demonstrated a preference for movement in a clockwise direction. In other words, the directional compulsoriness of the movement in the case of left-handed *O*s was found to be exactly the reverse of that in right-handed *O*s, a discovery which indicates that the directional compulsoriness of clockwise movement or of counterclockwise movement may be due to 'handedness' or 'eyedness.'[26] These facts also accord with some of the findings of Dallenbach[27] and others[28] in reference to 'position' as a determinant of consciousness. Their results indicate that "specific preferential position seems to be a matter of right- or

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[26] B. S. Parsons, *Lefthandedness*, 1924, 1-85, summarizes the data on the subject, including experiments of his own, and concludes that handedness is dependent on eyedness.

[27] K. M. Dallenbach, Position vs. Intensity as a Determinant of Clearness, this JOURNAL, 34, 1923, 282-286.

[28] L. G. Meads, Form vs. Intensity as a Determinant of Attention, this JOURNAL, 26, 1915, 150-151; J. N. Curtis and W. S. Foster, Size vs. Intensity as a Determinant of Attention, *ibid.*, 28, 1917, 293-296; A. M. Bowman, Size vs. Intensity as a Determinant of Attention *ibid*, 31, 1920, 87-90; Dewey and Dallenbach, *ibid*, 35, 1924, 121-125; Burke and Dallenbach, Position vs. Intensity as a Determinant of Attention of Left-handed Observers, *ibid*, 35, 1924, 267-269.

left-handedness; the positions to the left and above have a distinct attentional advantage for right-handed *O*s and contrariwise the positions to the right and below have an attentional advantage for left-handed *O*s."[29]

In sum, the part played by attitudinal variables in the direction taken by movement is very significant. However, *E* maintains that the direction of movement in an equivocal situation is not merely a matter of chance or ignorance, but rather depends upon certain determinate or determinable conditions, many of which were mentioned in this Section and in Section VII.

CONCLUSIONS

Some determinants of the attributive characteristics (such as duration, greyness, clearness, rapidity, completeness) of the phenomenon of apparent movement may be listed as follows:

A. Determinants not in the stimulus:

(1) *Anayltic vs. synthetic attitude.* Laboratory 'atmosphere' is favorable for an analytic attitude. A synthetic attitude develops with habituation and training. Decay of one attitude results in growth of the other (473, 479).

(2) *Passive-receptive vs. active-striving attitude.* When *O*s are habituated to the experiment and a synthetic attitude has developed, a passive, receptive attitude has been found to favor a uniform distribution of attention over the stimulus field and hence a more nearly univocal perception of movement. An active, striving attitude under conditions of training tends to merge into an analytic attitude and to bring about a perception of succession rather than of movement (479, 498).

(3) *Attentional preference for rotary movement in certain directions.* Rotary movement seems to be initiated more readily in a counterclockwise direction from the vertical position, at least in the case of right-eyed *O*s (498).

(4) *Position.* There seems to be an inertia of the organism against carrying rotary movement across the vertical axis. In other words, rotary apparent movement takes place more readily laterally than transversely (487 ff.).

B. Determinants in the stimulus:

(1) *Distractions and artificiality of tachistoscopic technique.* Except in the case of the well-practiced *O*, it is found that an even distribution of attention and a uniform maintenance of gaze upon the fixation point is rendered particularly difficult in tachistoscopic observation owing to the diminutive size of the stimulus field, to light changes and to noises (489).

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[29] Burke and Dallenbach, *op. cit.*, 269.

(2) *Background.* Relative variation of stimulus and background influences the clearness, speed and haziness of the perception of movement. Movement of white on black gives the clearest and sharpest perception, black on white the next, and black on grey the poorest (481).

(3) *Fixation point.* A peripheral position at or near the completion of rotary movement (which takes place within the limits of the fovea) is the most effective point for fixation (490 f.).

(4) *Form of the vehicle of movement.* In general, an increase in the complexity of the object favors good movement. The perception of movement of a solid object is clearer than the perception of movement of a unidimensional object. Lines which are thickened at the ends revolve more easily and definitely than lines of uniform width. The direction of the movement of a double-diameter (No. 3, Fig. II) is less subject to voluntary change than the direction of movement of a single diameter (No. 7, Fig. II) (480, 494).

(5) *Circumscription.* Surrounding the stimulus field by a line increases the clearness of the perception of movement (486).

(6) *Meaningfulness.* In general, a meaningful, animate pattern gives a clearer perception of movement than an unmeaningful, inanimate pattern (474, 480).

(7) *Splitting movement.* Perception of an equivocal situation (No. 6, Fig. II) often gives rise to a 'splitting' movement, which presumably corresponds to Wertheimer's 'pure' *phi*. Splitting movement seems to be dependent upon definite conditions and is not a matter of chance (495 f.).

(8) *Repetition.* With repetition of the stimulus, the movement pattern at first improves and then, with the development of fatigue, gradually decays. The decay of the movement pattern of animate objects is more gradual than the decay of the movement pattern of inanimate objects, and of objects structurally more simple (479 f.).

A hitherto neglected factor in the phenomenon of apparent movement is the basis for the directional tendency, *i.e.* the cause of the direction of the movement in an equivocal situation. (The 'equivocal situation' is a figure in which an object may go in either of two directions; cf. No. 6, Fig. II.) The direction taken by the movement in such an arrangement has been found to depend upon some of the determinants listed above.

It is a theoretical implication of this investigation that the so-called *phi*-phenomenon is not an invariable and uncontrollable experience. The perception of movement is a complex, variable experience, not necessarily analyzable into sensational units, but certainly analyzable into conditions responsible for its phenomenological structure. It is partly controllable, and is subject to a multiplicity of determinants, some of which have been discovered in the course of this investigation and are described in this article.

The position of the *Gestalt* psychologists implies that some sort of constancy exists between the physical configuration and the phenomenal structure. The results of this investigation accord roughly with such a conception, while at the same time they emphasize the tremendous importance of subjective determinants. Even the univocality or discreteness of the perception of movement seems to be a relative affair, depending not only upon determinants in the stimulus, but also depending to a considerable extent upon vague 'natural' or 'artificial' attitudinal variables which call for further study.

MESCAL VISIONS AND EIDETIC VISION

By Heinrich Klüver, University of Minnesota

It has been stated[1] that the discovery of the eidetic disposition ranks first among "the true advances of psychology in the last decade." Unfortunately, the investigator of eidetic phenomena who is not eidetically gifted is unable to observe the behavior of *Anschauungsbilder* for himself. Consequently the interpretation of certain statements repeated again and again by his eidetic subjects involves some difficulties. Now, according to W. Jaensch (13) *Anhalonium Lewinii* provides the possibility "bei Nichteidetikern Anschauungsbilder . . . . zu erzeugen." So the writer took mescal buttons (peyote)—kindly provided by Professor S. W. Fernberger—with the object of producing experimentally phenomena that are, or are comparable to, eidetic images. The question was whether some of the characteristics of eidetic images as determined by experimental work in and outside of Marburg can also be found in mescal vision.[2] In the following account attention is paid both to the similarities and to some striking differences between mescal vision and eidetic vision. At the same time observations not bearing directly on eidetic problems will be discussed.

For purposes of investigation peyote has been used quite frequently. The peyote cult of many Indian tribes (see bibliography) gave impetus to a number of researches upon the physiological effects of the drug. Psychologists were, aside from obtaining descriptions of mescal visions, interested in sensorimotor changes, in the influence upon higher thought processes, and in changes of personality. Very infrequently, however, mescal experiments have been considered as means to elucidate more general questions of the psychology and pathology of perception. Here Knauer and Maloney (16) must be mentioned for their attempt to throw light upon "the mechanism of visual hallucinations" by means of experiments of this kind.

The following experiment was conducted in the Psychological Laboratory of the University of Minnesota on Oct. 18, 1925. I wish to express my thanks to Mr. C. Jacobsen for his help in experimenting and in taking down my observations. Practically the whole time between 11 a. m., (Oct. 18), and 2:30 a. m., Oct. 19, was spent in one of the rooms of the Psychological

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[1] O. Klemm, *Arch. f. d. ges. Psychol.*, 47, 1924, 223.

[2] Compare especially: Jaensch, E. R. (12). See also: (15).

Laboratory. After that I went home and slept restfully from about 3 a. m. to 11 a. m., Oct. 19. (As to dreams during this time, I recall only the fact that I was discussing a rather abstract philosophical problem with somebody.) The buttons were not chewed and swallowed in the Indian fashion, but 43 grm. were ground to powder. At 11 a. m. I put 13 grm. of this powder in a glass of water and drank it. Even taken in this way it was very nauseating. At 11:40 a second dose of 10 grm. was taken. I placed portions of teaspoon size on my tongue and washed them down with water. After this second dose I felt rather 'seasick.' At 12:20 I vomited. Soon hereafter the visions began. Aside from a slight frontal headache, dryness in the mouth and an occasional pressure in the stomach, I did not experience any bodily discomfort throughout the rest of the day. Since I was afraid that the 23 grm. of the first two doses would not be effective enough because of the vomiting, I thought of taking a third and even a fourth dose; but because this idea invariably caused nausea I limited myself to the previously described doses.

Description of Mescal Visions

The resulting phenomena were observed with closed eyes (blinders) unless special mention is made of the fact that the eyes were open. It may be remarked that I myself am neither *Eidetiker* nor do I possess vivid and complete optical memory-images of objects.

Concerning the visions obtained in this experiment the following general observations should be noted:

(1) Adequate descriptive terms are difficult to find. Most investigators emphasize the point that the phenomena defy all description. H. Ellis says, "The chief character of the visions is their indescribableness" (6). This is due partly to the fact that forms and colors show certain characteristics not experienced previously in such a way, and partly to the transitoriness of the phenomena.

(2) The phenomena are frequently but not always localized at reading distance. Sometimes they are nearer or farther away. Movements may occur in all directions, but movements of approach and recession do not occur so often.

(3) The center of the visual field is often distinguished from the remainder of the field; there may be greater brightness, a different color or rotation while the remainder of the field is motionless (or vice versa). In the case of designs the center is often aesthetically the dominating part. Frequently, the field of vision is similar to the interior of a cone the vertex of which is lying in the center of the field directly before the eyes (or vice versa).

(4) The visions cannot be influenced by 'thinking' and 'will.'

(5) Visual memory-images and visionary phenomena exist independently.

(6) All spectral colors can be observed. There is no indication that certain colors occur more frequently than others. Sometimes approximately complementary colors appear simultaneously or successively.[3]

(7) In the majority of cases the arrangement of objects in the optical field is symmetrical.

(8) The observation of the phenomena is accompanied by pleasant feeling tones.

As to the following description, it must be remembered that until 2:30 a. m., Oct. 19, upon closing the eyes visions could always be seen. Taking into consideration the rapid changes it is clear that only comparatively few observations could be recorded.

Protocol transcribed by Mr. Jacobsen.

12:30 noon. Clouds from left to right through optical field. Tail of a pheasant (in center of field) turns into bright yellow star; star into sparks. Moving scintillating screw; 'hundreds' of screws. A sequence of rapidly changing objects in agreeable colors. A rotating wheel (diameter about 1 cm.) in the center of a silvery ground. Suddenly in the wheel a picture of God as represented in old Christian paintings.—Intention to see a homogeneous dark field of vision: red and green shoes appear. Most phenomena much nearer than reading distance.

12:35. The upper part of the body of a man, with a pale face but red cheeks, rising slowly from below. The face is unknown to me.—While I am thinking of a friend (visual memory-image) the head of an Indian appears.

12:45. Wavy spongy ground of greenish black color.—Glowing coals turn into pin heads of brilliant white color.

1:05 p. m. Wonderfully cut deep red jewels constantly changing in size, shape, color and distance from each other. Sometimes covered with silvery or greenish drops. The whole very symmetrical. The design on a background the color of which changes frequently.—Try to see the experimenter, but: blue clouds, a church, the dim face of an old man on background, blue arabesques. The whole seen through a steel veil the meshes of which are constantly changing in size and form.

1:10. Bright yellowish sparks irregularly distributed in the whole field.

1:15. Beads in different colors. Colors always changing: red to violet, green to bright gray, etc. Color so bright that I doubt that the eyes are closed.—Yellow mass like saltwater taffy pierced by two teeth (about 6 cm. in length).

1:20. Eyes open: the pupils alternately dilate and contract in day light; difficulties in focussing objects. Closing the eye: arches in violet. Black quatrefoils with three corners. The fourth corner seems to be lacking. Impossible to 'get' the fourth foil: the quatrefoils begin moving to the left and disappear.—Silvery water pouring downward, suddenly flowing upward.—Landscape as on Japanese pictures: a picture rather than a real landscape.—Sparks having the appearance of exploding shells turn into strange flowers which remind me of poppies in California.

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[3] Compare, however, W. Nägeli, Über selbstbeobachtete Gesichtserscheinungen, *Sitz. Ber. d. K. Bayer. Akad. d. Wissensch.*, 1, 1868.

1:25. Again the quatrefoil. Cannot get the fourth foil.

1:30. Open the eyes for a few seconds, look at the experimenter and close them: positive after-image of the face of the experimenter. His hair turns into hair of a cat, his eyes get a bright yellowish color. Then the head of a cat (natural size); hereafter the whole field filled with yellow eyes. (No change in size of the eyes during the modifications.)—Then a brilliant blue wheel rotating in left upper field. Blue wheel turns into red sparks.

1:35. Spiral rapidly revolving in center of field drawn out away from me.—(Blinders taken off, eyes remain closed): field turns red, red jewel motive appears; jewels now very small (diameter perhaps .3 mm.).—Elephant (size as on picture) turned upside down.

1:45. Pheasant tails filling the whole field.—Red jewel motive.—Colored threads running together in a revolving center, the whole similar to a cobweb.—A number of silvery forms in the midst of the field.—Gorgeous vine leaves colored as in Fall, on violet ground.—Arabesques, ornaments, designs changing kaleidoscopically.—Feeling that eyes are open.

1:55. Scintillating gem (size of a nut) deep red in color in center surrounded by greenish dark ground.—Steel veil of gorgeous bluish-violet color.—Visual memory-image of red candy: field turns green.

2:05. Moving rings in different colors.—In center bright light as coming from an electric bulb. Bulb seems to be incomplete. Then brilliant bluish form running through the field; seems to be incomplete in a certain way. In both cases unable to state what is lacking and how the impression of incompleteness arises.—Green sun (diameter about 3 cm.) in bluish dark field; no motion. Then revolving system of splinters of glass; system illuminated from within.

2:15. Homogeneous dark ground with bright patches in left upper field. Suddenly very intense bright light as if produced by an exploding shrapnel (apparently a few cm. distant from the eyes). Almost simultaneously jerky movement of hand to make sure whether blinders are on.

2:25. Darkish figures revolving around a center in a counter-clockwise direction. (Eyes open): streaks of green and violet on the wall. Then a drawing of a head changing into a mushroom (both of natural size). Then a skeleton (natural size) in lateral view turned about 30° to the left. Head and legs are lacking. Try to convince myself that there are only shadows on the wall, but still see the skeleton (as in X-ray).

2:45. (Eyes closed). Soft deep darkness with moving wheels and stars in extremely pleasant colors.—Nuns in silver dresses (about 3 cm. height) quickly disappearing.—Collection of bluish ink-bottles with labels.—Red, brownish and violet threads running together in center.—Autumn leaves turning into mescal buttons.—Different forms emitting intense greenish light.—Forms in different colors; contours often dark.

3:05. (Eyes open): the walls covered with squares (about 2 x 2 cm): shadowy dark contours. The corners of the squares: red jewels (three-dimensional). This design followed by similar mostly more complicated designs in various colors. Sometimes not sure whether the phenomena are localized at the distance of the walls.

3:20. (Eyes closed): on dark ground a red flag on pole waving to the left.—Designs as on rugs.—From now on the opening of the eyes does not necessarily banish the visionary design. The designs seem to be localized on walls, on the floor, etc. The opening of the eyes does not change the magnitude of the forms.[4]

3:35. (Eyes closed): marvellous explosion of scintillating yellowish light mass in center of the field. (Diameter of exploding mass about 4-5 cm.) Very agreeable.

3:45. (Eyes open): impossible to look at the walls without seeing them covered with visionary phenomena. Various designs, then the upper part of the body of an Egyptian woman (two-dimensional, but natural size). (Eyes closed): a jewel design turns into dark background with symmetrically arranged golden sixes which move from the right to the left.

3:55. Strange animal (length perhaps 10 cm.) rapidly turns into arabesques. —Gold rain falling vertically.

4:05. On stationary background rotating jewels revolving around a center. Then, with a certain jerk, absence of all motion.—Regular and irregular, forms in iridescent colors reminding of radiolaria, sea urchins, and shells etc., in symmetrical or asymmetrical arrangement.—Shells illuminated from within radiating in different colors, moving towards the right, turned about 45° towards the right and somewhat towards me. A little piece in every shell is broken out.—Slow majestic movements along differently shaped curves simultaneous with 'mad' movements.—Feeling there is 'motion *per se*.'—Man in greenish velvet (height about 7-8 cm.) jumping into deep chasm.—Strange animal turns into a piece of wood in horizontal position.

4:15. (Eyes opened): visionary phenomena on the walls. Moving the eyes, I notice that the scotoma which I have in my left eye moves before the designs.

From 4:30 p. m. on various experiments were conducted. The visionary phenomena, however, lasted until retiring and could always be seen in the dark room with open eyes. Arabesques, ornaments, various geometric patterns were dominating. On the following day, Oct. 19, after a period of sleep, there was still a great sensitivity to colors, but visions could not be detected with closed or open eyes. On Oct. 20, 10 a. m., I saw one of the designs with closed eyes at the moment when the dentist began drilling one of my biscuspids.

EXPERIMENTS WITH REFERENCE TO EIDETIC VISION

We began with measurements of the size of after-images under conditions similar to those in eidetic experiments (15). A gray background (43 x 49 cm., Hering's gray paper 24) was placed at a distance of 50 cm. The head was fixed in a headrest. Colored squares (Milton Bradley Company) were used as stimuli.

During the experiment the gray background was covered most of the time with ever-changing designs in various forms and colors. Sometimes the paper assumed a cloudy appearance.

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[4] Compare E. S. Conklin, (4). I do not know whether observations of this kind are in harmony with Conklin's and Jaensch's genetic explanations concerning the approximate constancy of magnitude and deviations from it.

When I tried to touch the slightly pulsating surface my finger seemed to come in contact with it about 20 cm. before the paper. This is a phenomenon surprisingly similar to that experienced by Gelb's patient W (9). The paper squares used to produce after-images were also covered with varying forms and colors. Sometimes the phenomena observed on the gray ground showed the complementary color of the square which I was fixating. In most cases, however, the colors were not complementary or even approximately complementary. The normal marginal contrast was frequently observed. The hues of the after-images seemed to be the same as for me under normal conditions. Frequently, the after-images were more deeply saturated. In most cases form and color of the after-images transformed into the visionary phenomena (16). A violet after-image which seemed to be identical in size with the original stimulus, rapidly turned into a violet circle, then into stars, etc., which became a part of the design that covered the whole paper. Under these circumstances the determination of the size of after-images was very difficult. In some cases the visions prevented the appearance of after-images altogether. In most instances, however, there was a short period during which an after-image with sharp contours could be seen. Thus its size could be determined (vertical measurements from the outside) before it shifted into the texture of the visionary phenomena.

For illustration we include some records:

TABLE I

Color of stimulus	*Size*	*Distance of exposure*	*Time of exposure*	*Size of negative after-image*	
orange	5 cm	50 cm	10 sec.	measured at 50 cm	5.3 cm
orange	5 cm	50 cm	10 sec.	"	5.2 cm
yellow	5 cm	50 cm	10 sec.	"	5.7 cm
orange	5 cm	60 cm	40 sec.	"	5.3 cm
yellow	20 cm	150 cm	10 sec.	measured at 25 cm	3.6 cm
yellow	20 cm	150 cm	40 sec.	"	3.8 cm

The values obtained are about the same as for me under normal conditions. This implies that: (1) Emmert's law seems to hold (slight deviations); (2) there is the frequently observed enlargement in the isodiastatic constellation (Zeman). The approximate constancy of size in various distances as found in eidetic experiments was never observed under mescal or in the normal state. It was, furthermore, impossible to localize the after-image definitely at a given space, as can be done with the *Anschauungsbilder* of certain persons (15). The after-images move with the eyes, while the visionary phenomena seem to be stationary (16). It is to be noted that the changes as to color and form in the after-image, which we observed so frequently, are also found in experiments with *Eidetiker*.

In experiments with simultaneous exposure of two 5 cm. squares (either side by side or 5 cm. from each other), two distinct after-images in the complementary color could be seen. A mixture of these squares was not possible. (Sometimes *Anschauungsbilder* can be mixed under the same conditions.) In experiments with monocular exposure (30 sec.), a faint complementary after-image could be seen with the other eye.

I completely lacked the ability of certain *Eidetiker* to see colors spontaneously. The 'intention', the 'will' to see, *e.g.* blue, had apparently no influence. The gray background showed nothing but brilliant jewel and pearl designs in constant motion. Thus a comparison with eidetic experiments in which it was possible to operate with colors which were produced intentionally, cannot be made.

When the paper squares were exposed behind a reading-glass, the removal of the square resulted in seeing an after-image of the size of the stimulus. Simultaneous removal of square and reading-glass did not change this result.

Pictures of animals (cut out of paper: donkey, tiger, etc.)[5] were exposed individually before the gray background for 15-40 sec. After removal I tried to see the picture with open eyes. In no case was I successful; only visionary phenomena covered the ground. The same was found when silhouettes were taken as stimulus material. With the eyes closed, I observed sometimes certain details in the color of the stimulus. They became, however, very rapidly a part of the visionary designs.

The instruction of the experimenter to 'see' a rabbit on the gray ground could not be carried out, though "brilliant red jewels, crabs turning to snakes, green flames" could be seen. The rabbit could not be seen with closed eyes either. Then I tried other animals, but in vain.

It must be said in general that the drug did not create the ability some *Eidetiker* have to see intentionally 'meaningless' (squares, etc.), or 'meaningful' material (pictures, persons, animals), either with open or closed eyes. In all cases visual memory-images of the objects I intended to visualize were present.

In dealing with monocular and binocular estimation of distance under the influence of mescal, I started from an arrangement similar to that in Jaensch's horopter investigations: head in rest; distance of the three black threads from the root of the nose, 1 m.; distance of the threads from each other, 5 cm; occlusion screen 15 cm. before, and gray background 40 cm. behind the threads; the slit of the occlusion screen at the level of the eyes. In 15 trials the middle thread was placed at either 95, 100 or 105 cm. (5 times at each distance), the other two threads remaining stationary.

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[5] See reproduction of pictures and silhouettes in (15).

TABLE II

	Monocular			Binocular		
Cm.	*Near*	*Same*	*Far*	*Near*	*Same*	*Far*
Mescal state:						
95	—	4	1	5	—	—
100	—	4	1	—	5	—
105	—	5	—	—	—	5
Normal state:						
95	3	2	—	5	—	—
100	—	5	—	—	5	—
105	—	5	—	—	—	5

Comparing the two series, there is no difference between binocular estimations of distance in the mescal and in the normal state. As to monocular estimation, the result is that, in general, in both states no differences of depth can be perceived. (Compare also (26), p. 83.)

It seems, therefore, that mescal could not influence the perception of depth under the above mentioned experimental conditions, in spite of the difficulties of observation: *e.g.* the threads sometimes seemed to move; the gray background was covered with visionary phenomena. It was found, moreover, that in binocular vision 1 or 2 cm. displacements of the right or left thread (98, 99, 101, 102 cm.), the middle and the other thread remaining stationary, were always judged correctly. The threads then were placed at the following distances: middle thread, 95 cm.; right and left thread, 100 cm. The experimenter moved the middle thread slowly backward. It seemed to be the same distance at 99.9 cm. When it was moved from 105 cm. forward, the same distance was reached at 100 cm.

The examination of geometrical-optical illusions (various modifications of Müller-Lyer, Zöllner, Poggendorff, Wundt, Kundt, Hering, etc.) did not reveal any differences between mescal and normal vision so far as the kind of illusion is concerned. With regard to the amount of certain illusions, measurements failed to indicate differences in this respect. Stereograms were normal (8). The filling out of the blind spot was performed under various conditions. Hering's "*Gesetz der identischen Sehrichtungen*" was frequently confirmed. Various contrast effects were observed. To illustrate: at 1:30 a. m., Oct. 19, I suddenly noticed a green shadow in peripheral vision: it was the shadow of my red pen holder. The color of positive after-images seen with closed eyes changed into the color of the visionary phenomena. Positive after-images could not be seen with open eyes if colored papers were exposed in day light. It is well-known that some Eidetiker will get positive *Anschauungsbilder* under these conditions (time of exposure: 1 sec. or less, up to some minutes).

It was possible for me, however, to get positive after-effects of long duration with open eyes if the following arrangement was made: the stimulus material was placed before me in a darkened room and then suddenly illuminated for one second or less. Squares and other geometrical figures, pictures of animals, texts, etc., were used as stimulus material. Certain observations frequently reported by *Eidetiker* were confirmed. Some observations may be recorded. *Stimulus:* a text consisting of 4 lines pasted on cardboard. The words are unknown to me. *After-image:* positive; deep black letters on a paper of extreme brightness; the four lines very distinct; the spatial arrangement of the different words recognizable, but cannot read the text except the words "suffer at all" (correct). The form of the cardboard, and various objects on the table, can be recognized. The brightness on the table is so intense that I have to reassure myself that the light is turned out. *Stimulus:* a text consisting of 6 lines pasted on cardboard. *After-image:* positive; for a moment distinct lines with different words can be seen (unreadable); then the lines turn into rows of black arcades which are alike in form. *Stimulus:* the same. *After-image:* positive; 6 rectangles. *Stimulus:* picture of a tiger: yellow with black stripes and black contour. I do not know what picture is placed before me in the dark. *After-image:* my right hand holding the picture of the tiger; the texture of my coat, the shirt, folds on my thumb quite clear; the picture of the tiger itself in brilliant yellow with deep black contour and stripes. To the left my left lower arm, the hand at the key of the lamp, the lamp itself are seen. As to hue, form, voluminousness, the objects do not seem to differ very much from real objects. Finally the yellow of the tiger turns into a bluish color, the stripes and contour remain black. (During the observation of the after-image I held my hands and lower arms under the table. Probably owing to certain changes in the kinesthetic field, they seemed to be detached from me in a certain way. Thus my 'real' limbs and the limbs in the after-image seemed to be equally 'real' or 'unreal;' a confusing effect). *Stimulus:* contours of circles (colored paper, 2-5 mm. wide), squares, triangles, etc.; certain parts of the contour covered up by white paper. *After-image:* in all cases the form of the original stimulus is retained; no completion of form takes place. (Compare (26), p. 149.) *Stimulus* (monocular exposure): square or picture or candle. *After-image:* seen by the eye to which the material was exposed; opening of the other eye produced blinding effect, and increase in darkness while the stimulated eye still sees a distinct and brilliant after-image;

closing of the stimulated eye produced complete darkness. *Stimulus:* the same; the one eye exposed to the material, the other eye open, but covered by a screen. *After-image:* seen by the eye previously stimulated; no blinding effect in the other eye; closing of the stimulated eye produced complete darkness; no binocular effect of monocular exposure.

It is to be understood that the various after-sensations arising after short illumination in a dark room were soon followed by the appearance of visionary phenomena. They could be seen both with open and with closed eyes. The sudden illumination banished the phenomena in most cases completely for the time of the duration of the after-image.

The Marburg School has found that the application of disturbing stimuli (whistling, various noises, etc.) influence *Anschauungsbilder* in a definite way. Compare (3). The effect of various auditory, olfactory and cutaneous stimuli on mescal visions was tried. After the application of auditory stimuli I did not observe any change in the visions. It did not make any difference whether or not I expected the sound or noise. Upon application of olfactory or cutaneous stimuli the form of the visionary design as well as the saturation of the colors changed suddenly in some cases.[6] In other cases there was no effect at all.

CHANGES

In different Sensory Fields. With regard to color vision, there was a general increase in brightness; most colors seemed to be more deeply saturated. These changes in brightness and saturation seemed especially noticeable in peripheral vision. Again and again, the 'optical attention' was attracted by objects in the peripheral field; *e.g.* a 'flame' in the right lower field (twilight) turned out to be a piece of paper in the waste-basket; the hitherto unnoticed reflection, in one of a friend's front teeth, of a gold inlay in the next tooth was observed. In foveal vision objects seemed to become loose and cloudy. It was strenuous work to fixate objects for a longer time. Sometimes they seemed to lose their solidity. Colors often showed those characteristics usually ascribed to *Flächenfarben* (film colors). Movements of objects were frequently observed. Looking at the radiator in the room, its divisions seemed to move rhythmically. Standing near a corner of the room, the walls, like yellowish clouds, seemed to move towards me. Mach's spiral appeared to be a black steel spring, lying on brilliant white chalk, performing slow contractions. A package of cigarettes on the table moved with a jerk. A black circle pasted on white cardboard: I tried to dip my finger into a deep hole. (Foveal vision! compare the above mentioned observation with the gray paper and Bühler, (2) p. 57.) During the evening, the room being illuminated by electric light, the white paper on which I was writing was yellow; the walls, ordinarily of a dull grayish-yellow, showed a deeply saturated yellow.

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[6] See also (7).

With regard to auditory stimuli, even noises caused by writing, swallowing, etc., seemed to be loud. Tones of my violin had a greater voluminousness than usual. Trying to strum a melody sung by the Indians at their peyote festivals I noticed that I could not keep time. With regard to organic sensations, I seemed to be 'beyond all desires.' I did not feel hungry or thirsty. From the evening of Oct. 18 until noon of Oct. 20, I ate only some slices of bread and some bananas. Even at 2:00 a. m., Oct. 19, I was wide awake; there was hardly any feeling of fatigue throughout the experiment, although there were times when I was extremely drowsy and disinclined to move. In general, frequently repeated gymnastic exercises seemed to be amusing rather than fatiguing. Walking was performed with remarkable ease.

In 'Gegenstands- und Ichbewusstsein.' Some alterations besides the sensory changes are to be mentioned. My state may be characterized, on the one hand, as one of enchanted contentment, in which I was happily observing the visionary world; on the other hand as restless and wide awake, being eager to taste thoroughly the unique experience. There was no conscious intention, however, to be forcibly attentive to the phenomena or to slight sensory impressions. A certain unwillingness to be intellectually active could be noticed throughout the whole day. At the same time there was a dislike to formulate my experience verbally for the sake of an experiment. Even the idea of being subjected to experimentation was unpleasant. To the experimenter I seemed to be very distractible. Introspectively there seemed to be a certain disinclination, rather than inability, to focus my attention on anything for a longer time. There were no difficulties in understanding some pages of a psychological paper, but the whole process was rather disgusting.

The distortion of time frequently recorded by other experimenters was also experienced; but in this experiment it was apparently not so strong as in other cases. After an unpleasant period of 'concentration' it was possible to make fairly accurate time judgments (*Cf.* 7). According to some investigators an enormous over-estimation of time is very characteristic. In my state it seemed most characteristic that the inadequacy of assigning durations was felt so keenly. A question of the experimenter concerning time was considered rather ridiculous. It seemed to me incommensurate to speak about the experienced abundance of phenomena in terms of minutes and hours. I recall that in the past a similar attitude with regard to time has arisen in situations with high emotional pressure, *e.g.* in periods involving danger to life.

One of the experiences (a most surprising one) of this experiment was that owing to certain optical changes in the external objects, movements of external objects, film colors instead of surface colors, etc., visions and external objects became phenomenologically similar. Besides this unification in perceptual respect there are other changes of the '*Gegenstandsbewusstsein*' and '*Ichbewusstsein.*' My body and its organs seemed to be most of the time non-existent or detached from me as a perfectly functioning machine. While speaking I seemed to listen to a speech apparatus. Getting up from my arm chair to move around, I seemed to walk on clouds. In a moment of exhilaration I was inclined to kick through the airy walls of my room with my foot, knowing at the same time that I was in a solid stone building.

In general, the line of demarcation drawn between 'object' and 'subject' in normal state seemed to be changed. The body, the ego, became 'objective' in a certain way,[7] and the objects became 'subjective.' They became subjective not only in the sense that they behaved as visionary phenomena, but also in the sense that they gained certain affective qualities. A movement of the hand of the experimenter, a noise, a turning of the chair, seemed to be harmful and dangerous. Critically, I realized that the movement of a hand at 2 m. distance could not be injurious; but nevertheless the harmful influence of such a movement was felt directly. Somehow, the physical medium of the air, the difference between the bodies did not exist. I frequently felt that the experimenter meant me ill and I wished he would abstain from thoughts which could do me harm.

There is no doubt that these changes in '*Gegenstands- und Ichbewusstsein*' are comparable to those observed in schizophrenia. For the psychology of schizophrenia as developed by Storch, Schilder, and Kronfeld, etc., as well as for genetic psychology (compare Werner), mescal experiments might play a part in throwing light upon the genesis and pathology of the subject-object relation. And mescal seems especially recommendable, since it does not destroy the critical attitude of the observer.

SUGGESTED EXPERIMENTS

In closing, some further mescal experiments may be suggested. It seems desirable to test the effect of mescalin on: (1) a congenitally blind person; (2) a totally or partially color-blind person; (3) various *Eidetiker*, especially on persons of the T-type; (4) persons of the T-type who lost the eidetic disposition after ingestion of calcium lacticum; (5) persons of Kretschmer's body-form types, to investigate systematically the changes in '*Gegenstands- und Ichbewusstsein.*'

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[7] Compare one of H. Ellis' subjects (**7**): "It was as if I had unexpectedly attained an objective knowledge of my own personality."

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GESTALT PSYCHOLOGY AND MOTOR PSYCHOLOGY[1]

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Most American psychologists feel, I think, a cordial gratitude towards *Gestalt* psychology, particularly to the two missionaries, Professors Koffka and Köhler, who have expounded it so delightfully in this country, for the keen introspection and ingenious experiments by which they have revealed to us certain truths about our conscious experience, and reminded us of many more which nearly all of us have long acknowledged, but have perhaps neglected always to emphasize in our writings. We have not, indeed, quite recognized ourselves in the portrait they have drawn of that person whom they call the associational psychologist. We cannot admit that we regard conscious experiences as bundles of sensations, we who in our youth learned thoroughly from William James the law that all things fuse that can fuse and nothing separates except what must. But we do confess that we have sometimes seemed to overlook these laws, and we delight in having them pointed out to the extreme behaviorists, who have wholly ignored them.

My own difference with the *Gestalt* psychologists lies in the following point. I am willing to grant all that they urge against an atomistic conception of consciousness; I recognize the fact that the total conscious state is in no sense a mosaic, but a continuous blended whole. What I miss in all their discussions is any mention of the factor that seems to me to play the leading rôle in the phenomena they so vividly describe: namely, the nature of the motor response. It is this factor which appears to me to form the very essence of that 'thing character' which constitutes a configuration.

Take as an illustration the familiar pair of illusions shown in the accompanying figures. In Fig. 1, circle A looks larger than circle B, although they are equal by measurement. In Fig. 2, the small circle in C looks larger than the large circle in D, although they also are equal. A partial explanation of these illusions consists in saying that A looks larger than B because A is compared with the small circles around it, while B is compared with the large circles around it: the principle of contrast governs. The small circle in C (Fig. 2), on the other hand, is assimilated to the large circle around it and looks larger than the large circle in D, which is assimilated to the small circle within it. The configurationist might say that the patterns in which A and

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[1] Read at the Ithaca meeting of the American Psychological Association, Dec. 28, 1925.

B are found are such as to give emphasis to the circles as separate from one another, so that they are compared with one another and the principle of contrast rules; while in the case of C and D the principle of assimilation holds. But one looks in vain for a deeper-lying principle to explain why one figure should produce emphasis and another assimilation. What, exactly, is the nature of emphasis? We never learn from the configurationist. If, however, we introduce the influence of the motor response suggested by the figures, the matter becomes clearer. In Fig. 1 the circles are so placed that *it would be easy to pick them up separately*. They are therefore perceived as separate things and compared with one another. On the other hand, in C and D (Fig. 2), the concentric circles are so placed that one would naturally pick them up together. They look, in fact,

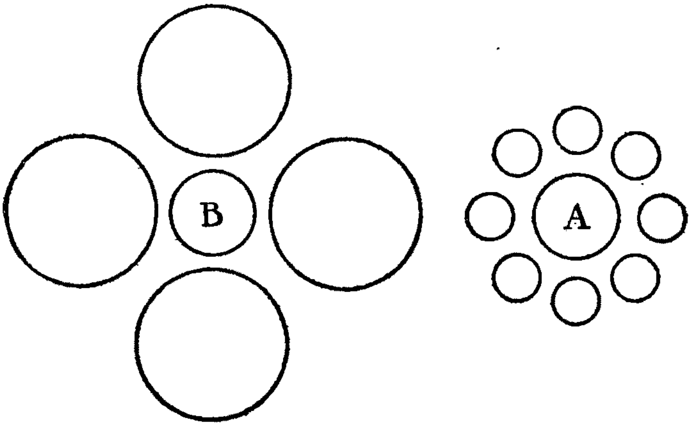

Fig. 1

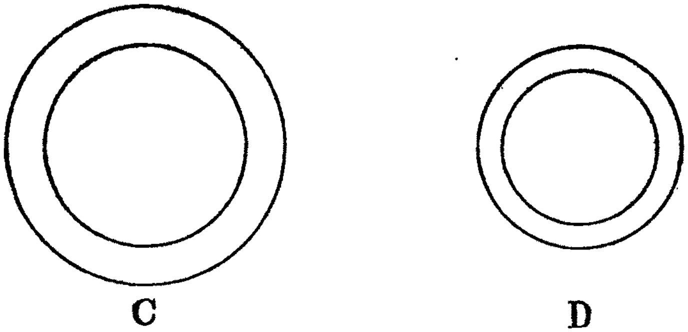

Fig. 2

much like a plate. Hence it is that they are perceived as a single thing, and not being compared with each other, the consciousness of their difference is lessened.

What is the real nature of those patterns which the configurationists call good or natural configurations? They are patterns whose elements are so arranged as to suggest reaction by a single movement to the whole group. Why is it that one portion of a field is perceived as pattern and the remaining portions as background? Köhler says that a small figure on a large ground receives emphasis and is perceived as figure rather than ground because the electric currents in that portion of the retina corresponding to the small figure are more concentrated than those in the surrounding portion. This may be true, but a further explanation lies in the fact that the small figure looks easier to pick up from the background than the background does from the figure, on the basis of our lifelong experience with picking up things. Where, as in the case of the pattern in Fig. 3, the suggestions of movement are equally strong in the two patterns, the white looking as easy to pick up from the black as the black does from the white, either white or black may receive the emphasis. Köhler would say that the concentration

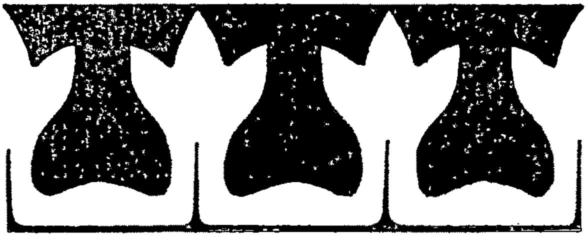

FIG. 3

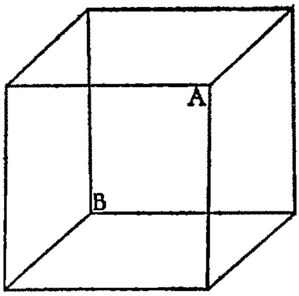

FIG. 4

of currents is equal in the two figures. Now in such a figure there actually occurs an alternation of emphasis, a shift from one to the other, the black and the white alternating as pattern and ground respectively. What determines this shifting? No obvious reason appears for any change in the concentration of currents. But on the motor hypothesis, anything may cause the shift which suggests a change in the motor response from picking up black to picking up white or vice versa. The words 'white' or 'black,' or a change of fixation point due to fatigue, may do it. In the ambiguous cube, Fig. 4, whether a cube resting on the ground or one suspended in air is perceived may depend on whether one fixates the point A or the point B; the point fixated seems to be nearer, that is, suggests a shorter reaching movement, because, other things being equal, in looking at an object we generally look at its nearest point, which of course carries with it that surface of the cube to which it belongs. Further, if a record is kept of the duration of the phases of the cube, we usually find that it is seen longer as a cube resting on the ground than as a hanging cube; this may be explained by the fact that we have a stronger tendency, due to habit, towards sitting down on cubes than towards looking up at them.

These illustrations indicate how the phenomena of ground and figure, of assimilation and emphasis, can be explained by the motor responses suggested, usually from previous experience. So far as I can learn, they are not explained at all on the configuration psychology; they are merely described.

In the second place, a motor psychology can offer an explanation for those relational processes which play so large a part both in configurationism and in actual conscious experience, About ten years ago, in my book, *Movement and Mental Imagery*, I tried to sketch such an explanation, and it still seems to me a serviceable one. It is briefly as follows. The conscious effect of a stimulus pattern is simple when the pattern is reacted to as a whole, complex when details are reacted to. Now the reactions themselves, of whatever nature, give rise to kinaesthetic excitations, and this whether they are movements fully performed or incipient, tentative movements only. These kinaesthetic excitations are a regular accompaniment of such excitations from outside stimuli as are reacted to at all; they influence all conscious processes. Whether they themselves are directly represented in consciousness depends on whether they in their turn are reacted to. When the mass of kinaesthetic excitation is reacted to as a whole, we have in consciousness a simple feeling of relation; when parts of the mass of kinaesthetic processes are reached to separately and thus localized in the body, we experience a complex of kinaesthetic sensations. Often they are not reacted to at all; our reaction habits are formed under the influence of practical needs, and we can get on much of the time very comfortably without being aware of relations as such, and all of the time without analyzing our feelings of relation into kinaesthetic sensation complexes: but whether we react to these kinaesthetic processes or not, they and the movements underlying them profoundly modify our experience. The simple relational feeling of *but*, we may suppose, arises when incompatible movements tend to be aroused; the simple relational feeling of difference when there is a sudden shift of motor excitation; grades of intensity may depend on the felt degree of diffusion of the motor response; feelings of duration, as Wundt has suggested, on strains and relaxations, and so on. If relational experiences have this kinaesthetic origin, it is easy to conceive them as occasioned not by absolute stimuli but by changes in the relation of stimuli, that is, changes in the diffusion, locaion, duration, combinations, and so forth, of the movements, full or tentative, performed by the organism. There is nothing puzzling, for example, about the fact that while intensity A may be indistinguishable from intensity B and intensity B from intensity C, intensities A and C are distinguishable from each other.

The most important evidence for this kinaesthetic theory of the nature of relational processes seems to me to be language. It would be only rarely and accidentally that the same verbal response would be attached to two different experiences if they had no common factor. If then, we use the same words, such as 'more' and 'less,' 'like' and 'different,' to designate quantitative change in any kind of conscious content whatsoever; 'longer' and 'shorter' of both temporal and spatial extents; 'soft' of touches and sounds, and 'motion' of both a tactual and a visual experience, this is most probably because of common factors accompanying the various sensory experiences. What more plausible suggestion as to the physiological origin of these common factors than that they come from our own reactions, muscular and organic, to the external situations which we encounter; from that source of experience which we carry constantly about with us, our own bodies?

WHAT IS EMPIRICAL PSYCHOLOGY?

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The phrase *empirical psychology* is used in at least three major senses in current technical psychological discussion. It is the purpose of the present paper to consider the following questions in regard to this triple usage: (I) What is the definition of each of the three meanings of the phrase? (II) Are these three meanings in any manner related? (III) Is it expedient to continue using the same phrase to denote three meanings?

I. DEFINITION OF THE DIFFERENT MEANINGS

In the first of the three senses to be considered here, the word *empirical* may be taken as roughly synonymous with the word experimental.[1] This usage enjoys the sanction of historicity. Among the sects of ancient physicians the *Empirics* drew their procedures entirely from practice and trial: they tried to exclude all philosophic theory. The *Empirics* opposed both the *Methodic* and the *Dogmatic* schools.[2] Hence it has come to be that one is characterized as *empirical* "who either in medicine or in other branches of science relies solely upon observation and experiment."[3] In this sense the term is antithetical to such words as *rational* and *inferential.* This antithesis is one of method. Indeed, *empirical,* in this first sense, is strictly a methodological conception. Before the middle of the eighteenth century, Christian Wolff dichotomized psychology upon the basis of this anthithesis. He distinguished between a psychology which is in method *empirical* and one which is in method *rational.*[4] In spite of Wolff's failure to live up to this program of separation there are still psychologists who attempt to divide the discipline in this same fashion. For example, Maher says: "The discussion of the former questions—the inquiry into the character of the various mental states and operations is called by different writers *Phenomenal*, *Empirical*, or *Experimental Psychology*; whilst investigation into the nature of mind itself is styled *Rational Psychology* : . . The term *Phenomenal* is applied to the first part of Psychology, because it investigates the various *phenomena* of the mind, the facts of consciousness. It is called *Empirical* or *Experimental* because we have immediate experience of these facts: we can study them in immediate observation. The second part of our subject is marked by the epithet *Rational*, because the truths which are there enunciated are reached not by direct experience, but by reasoning from conclusions established in the earlier part.[5]

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[1] Strictly speaking, *empirical,* in this sense, is a broader term than *experimental* which rightly designates a kind of experience in which the fact observed has been isolated, to some extent, by intentional pre-arrangement of conditions. Cf. William Hamilton, *Lectures on Metaphysics,* 1859, I, 55. Thus the true experimental method is seen to be always an empirical method. Similarly any empirical observation is or was subject to experimentation. The conception of the *Gedankenexperiment* of Ernst Mach is, of course, no exception to this generalization for this method is only by analogy experimental; it is typically rational. Cf. *Erkenntnis und Irrtum,* 1906, 183-200, and for distinctions between this rational method and true experimentation E. Rignano, *The Psychology of Reasoning,* 1923, Chap. IV.

[2] Cf. W. Whewell, *History of the Inductive Sciences,* 1847, III, 425 f.

[3] *New English Dictionary,* Vol. III, Part I, 128.

[4] Christiano Wolfio, *Psychologia Empirica Methodo Scientifica Pertractata qua ea, quae De Anima Humana Indubia Experientiae Fide Constant, Continentur, etc.,* 1732, par. 1-10. Cf. also, *idem, Psychologia Rationalis, etc.,* 1734.

The methodological limitations, therefore, of empirical psychology considered in this first sense are such that it may establish only generalizations that will make each law a uniformity whether of succession or co-existence and which holds true in all instances within our limits of observation, but is not of a nature to afford any assurance that it would hold beyond those limits.[6]

In this first sense of the term, then, the adjective *empirical* may be thought of as characterizing a psychology which is based upon experimentally discovered data.[7]

(2) In the second of the three senses to be considered here the word *empirical* may be taken as roughly synonymous with the phrase, *acquired during individual development.* While current in much psychological writing this meaning is essentially epistemological. In this sense the term is antithetical to nativistic.[8] This meaning of *empirical* is frequently used to-day. Thus, Warren says, "This application of the evolution theory to psychology gave the *a priori* school a nominal victory, since its contention was literally substantiated; but empirical psychology came off with real honors, since its protest against 'innate

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[5] M. Maher, *Psychology: Empirical and Rational*, 1900, 5.

[6] J. S. Mill, *System of Logic*, Book VI, Chap. V, par. I, 1887.

[7] By a curious chain of circumstances (which may well be a parable for any discipline which attempts to be *purely* empirical), this honorable word has come to have, in certain circles, an unfavorable connotation. In medicine the term is often used to describe a charlatan or a quack. This same usage has been extended to other fields. The unfavorable meaning of the word arose from the fact that the ancient Empiric school of medicine degenerated. Its errors, which may well have been due to a failure to correlate observations, came (first in the influential works of Galen) to be synonymous with ignorance and false pretension. Cf. William Hamilton, *loc. cit.*

[8] Cf. J. M. Baldwin, *Dictionary of Philosophy and Psychology*, 1901, I, 321; also R. Müller-Freienfels, *Eislers Handwörterbuch der Philosophie*, 1922, 522 f.

forms' was validated in the wider field of race history."[9] Klemm uses the word in a similar sense, "The typical supporter of empiricism in the theory of spatial perception in Germany is Helmholtz. According to his empirical view our sensations are signs for our consciousness, the significance of which our reason has to learn."[10] Or, as Koffka says, "Even to-day no agreement has been reached between the rival theories of *empiricism* and *nativism*, the first of which emphasizes the influence of the environment, and the second the influences of heredity."[11]

In this second sense, then, the term *empirical* may be thought of as descriptive of a psychology which holds that behavior and experience are wholly developed in the ontogeny of the individual.

(3) The third usage of the phrase *empirical psychology* is not so easily characterized. It has not yet found its way into the lexicons. Its sanction is derived largely from its iterated use by Professor E. B. Titchener. The special sense in which this scholar uses the words *empirical psychology* therefore may best be given in quotation from his own writings.

In an article entitled *Brentano and Wundt: Empirical and Experimental Psychology*, this author shows the antithesis which he believes to exist between the two tendencies presented in his title. In the first place the empirical psychology of Brentano is characterized as "essentially a matter of argument;" while the experimental psychology of Wundt is "essentially a matter of description."[12] Again, we are told: "For the 'empirical' psychologist means to take mind as he finds it; and like the rest of the world, who are not psychologists, he finds it in use; he finds it actively at work in man's intercourse with nature and with his fellow-men, as well as in his discourse with himself."[13] In a later article, in considering the hindrances to the development of an experimental psychology, as he conceives it, Titchener further writes:

"The second unfavorable influence which I have to note is of far greater importance—partly because of its antiquity and perennial vitality, partly because of its seeming innocence and the guise of kinship with experimental psychology which it often assumes, so that it works insidiously from within rather than openly from without. It is the influence of empirical psychology, a mode of psychology that goes back to Aristotle and Thomas Aquinas, and that forms the staple contents of most psychologies down to and including our twentieth-century textbooks. The empirical psychologist professes to take mind as he finds it: I use the term 'mind' without prejudice, for the subject-matter of empirical psychology; in point of fact the definition of that subject-matter has varied greatly: always, however, the empirical psychologist professes to take mind as he finds it. . . . This subject-matter, then, mind in use, the empirical psychologist seeks to reduce to order and arrangement; and empirical psychology thus becomes a rationalization of practice, a 'description and explanation' of mental uses." [14]

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[9] H. C. Warren, *A History of the Association Psychology*, 1921, 119.

[10] O. Klemm, *History of Psychology*, tr. by E. C. Wilm and R. Pintner, 1914, 335.

[11] K. Koffka, *Growth of Mind*, tr. by R. M. Ogden, 1924, 1.

[12] E. B. Titchener, Brentano and Wundt: empirical and experimental psychology, this JOURNAL, 32, 1921, 111.

[13] *Ibid*, 119.

These excerpts suggest the meaning which Titchener has newly attached to the term *Empirical Psychology*. For a complete picture of his view the two articles cited must, of course, be read in their entirety.

If, in summary, a short characterization of the meaning of this phrase, as used by Titchener, be essayed, it may be suggested that an *empirical psychology*, so considered, is a discipline concerned with the presentation of a consistent and reasoned account of the mind in use. Such a psychology, typically, respects the authority of past thought more than the data of present experiment. *Empirical psychology* is therefore, in this sense, antithetical to an hypothetical psychology that is purely descriptive.

II. Relation Between the Different Meanings

Are these three general meanings of the phrase *empirical psychology* related in any manner? Between the first and second meanings considered above there may be some kinship. The hypothesis of nativism, historically, seems to have been, in so far as psychology is concerned, for the most part, although by no means exclusively, supported by those who use some such method as intuition rather than experiment for the discovery of the facts which they consider.[15] To the present writer it seems more than a mere coincidence that the psychology of John Stuart Mill was empirical, in spirit at least, both in the sense that it opposed 'innate principles' and that it depends, at least theoretically, upon scientific experiment for its data.[16]

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[14] *Idem*, Experimental psychology: a retrospect, this JOURNAL, 36, 1925 315 ff. Cf. also K. M. Dallenbach, Review of Woodworth's *Psychology*, *ibid.*, 33, 1922, 434 ff.

[15] Of course this is by no means universally true. In claim, the post-evolutionary psychology of instinct is empirical in method.

As we have shown, the first conception of *empirical* is methodological; the second, epistemological. It may be urged that a proper application of the empirical method to the data of development will show that all mental life is empirically (in the second sense) acquired; but this is a separate question which cannot be considered here.[17]

Between either the first or the second meaning of the term *empirical* and the third sense in which the adjective is now used, there exists even less relationship. There is in fact between two of these a direct opposition. Titchener himself says, "For all experimental psychology is in the broad sense empirical, and a psychology which is in the narrow sense empirical may still have recourse to experiment."[18] But "in the narrow sense" an *empirical psychology,* in this very *Brentano and Wundt* article, is contrasted with an *experimental psychology.*[19] Hence we find an antithesis between the "broad" and the "narrow" meanings of the same phrase! A double usage of this sort is calculated to confuse the modern reader.

Certainly, little or no relationship between this novel third meaning of the phrase *empirical psychology* and the second, or epistemological, use of the word as *non-nativistic,* can be demonstrated. If *empirical,* in the third sense, be taken as synonymous with *metaphysical* or merely *speculative*—words which in fact do not do justice to the important conception of *empirical psychology* as set forward by Titchener—it might be urged in the light of history that the empirical psychologist is characteristically a nativist. Such a contention, however, would be subject to many exceptions. As suggested above in the consideration of Mill's system, it may well be that the correlation between methodological empiricism and a non-nativistic view of mind is more than coincidental; but it is a relationship of such an involved sort that it surely cannot be used to define the word *empirical*.

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[16] Cf. J. S. Mill, *Autobiography,* 1924, 158, and *idem, System of Logic,* 1887, Books V and VI. Wundt may have been the first to use the phrase *experimental psychology.* But, in regard to the origin of this conception, Titchener has said: "Whence now did Wundt derive his idea of an experimental psychology? . . . No one can read the introduction to the *Beiträge* without being reminded of the sixth book of John Mill's *Logic;* and no one, I think, who after such reminder compares the two compositions can doubt that Mill, for whom psychology is explicitly a science of observation and experiment, gave the cue both for Wundt's emphasis on improvement in method and for the concrete means to improvement, statistics and experiment, which Wundt propounds" (Wilhelm Wundt, this JOURNAL, 32, 1921, 164f.).

[17] I have previously considered certain phases of this problem in two papers: Heredity and environment: are they antithetical? *J. Abn. & Soc. Psychol.,* 20, 1925, 245-260; and Development of behavior in vertebrates experimentally removed from the influence of external stimulation, *Psychol. Rev.,* 33, 1926, 51-58.

[18] Titchener, Brentano and Wundt, *op. cit.,* 111.

[19] *Vide supra.*

It may be said with assurance, then, that between the three uses of the phrase *empirical psychology* no logical relationship exists which may serve to make homogeneous their disparate meanings.

III. Is it Expedient to Continue the Present Triple Use?

In light of the fact that the three usages of the single phrase *empirical psychology* are not only incongruous, but in certain instances positively antithetical, is it expedient to continue the present triple use? It is the contention of the present paper that to do so is confusing and therefore inadvisable. The first meaning we have seen is historical: broadly speaking, the *empirical* method *is* the *experimental* method. In this sense the term is well established and may continue to be used. Secondly, the term, *empirical*, as characterizing a psychology of individual acquirement is legitimate enough.[20] To the present writer, however, it would seem on the whole better to substitute the term *ontogenetic* psychology in place of the epistemological use of the phrase *empirical psychology*.

In regard to the third use of the term *empirical psychology*, we should at once agree that a writer may coin a new phrase, or, as in the present instance, add a new meaning to an old phrase if he so desires. In the present case, the significant and critical conception which Titchener has built up around the phrase *empirical psychology* is novel and illuminating. Nevertheless, it is unfortunate that the phrase chosen is ambiguous. To understand the words *empirical psychology* in this new sense, demands a radical acceptance, in a curiously literal fashion, of the contextual theory of verbal meaning. This fact makes it all the more difficult to suggest a new word that will 'carry' the meaning which now 'accrues' to the adjective *empirical* in Titchener's writings. The old term *rational*, however, seems to fit the case rather precisely.

In the way of this substitution, it is true, there are obvious difficulties. For example, its adoption would entail the assertion that Brentano's so-called *empirical system*, in so far as it is not really based upon the results of the empirical method, is a misnomer.[21] But after all is this not the case in Brentano's psychology? Does not a clear use of terms always make conceptions themselves more succinct?

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[20] It will, of course, be remembered that fundamentally, and in derivation, *experimental* and *experiential* are not unrelated. Cf. A. Bain, The empiricist position, *Mind*, 14, 1889, 369-392.

In conclusion then it seems that less ambiguity would result if *empirical*, in the first sense considered above, be still used as broadly synonymous with experimental, that *empirical* in the second sense be supplanted by *ontogenetic*, and that *empirical* in the third sense, be supplanted by *rational*.[22]

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[21] Cf. F. Brentano, *Psychologie vom empirischen Standpunkte*, 1874. It is interesting to note that Titchener has been characterized as an empirical psychologist. Cf. Vernon Lee, Introduction to Semon's *Mnemic Psychology*, 1923, 34.

[22] It may be further asked, if the present terminology is adopted, whether, in practice, an empirical psychology can be completely independent of a rational psychology? The present writer is inclined to believe that a pure empirical psychology cannot be developed. Premises may be concealed: metaphysics may be implicit: but in psychology, as in physics, certain fundamental assumptions must be made if the scientific data are to present a seemly and systematic order. The empirical method, purely applied, might well result in a 'scrap-book' of irrelevant items. In this day of conflicting 'scientific' systems of psychology, which agree in little save that they are 'non-metaphysical,' certain words of Professor Dewey, written some years ago, are still important. He says: "No writer can create nor recreate his material, and it is quite likely that the philosophic implications embedded in the very heart of psychology are not got rid of when they are kept out of sight. Some opinion regarding the nature of mind and its relations to reality will show itself on almost every page and the fact that this opinion is introduced without the conscious intention of the writer may serve to confuse both the author and the reader" (*Psychology*, 1886, p. iv.).

THE EFFECT OF VARYING PERIODS OF ADAPTATION ON THE FLIGHT OF COLORS*

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INTRODUCTION

In a previous article the writer has described an experiment on the flight of colors in the after-image of a bright light.[1] The purpose of that experiment was to investigate the relation between the after-image and the varying intensities and durations of stimulation which gave rise to it. As a fixed period of five minutes' adaptation to darkness conditioned every observation, the possible effect of different durations of adaptation on the image was not investigated. The present paper describes further experiments on the after-images following various degrees of adaptation to darkness.

Apparatus. The observations in this experiment were conducted in a dark room with apparatus identical, with one exception,[2] to that of the previous experiment.[3] In place of one of the four 'day-light' mazda bulbs, rated at 25, 50, 75, and 100 w., a stimulus light, rated at 75 w., was used throughout the investigation. Furthermore, a constant duration of 40 sec. exposure to this stimulus conditioned every observation.[4]

Subjects. All of the 28 *S*s were undergraduate students who had had at least one course in psychological training. Twelve *S*s—An, Ed, Gr, Ha, Lu, Ne, Oa, Pe, Ra, Su, To, and Wi—reported 37 times each in the first experiment which continued 4½ months. Eleven *S*s—Ba, Co, Fe, Ho, Kr, Lh, Pr, Ro, Se, St, and Wh—each reported for 5 observations during a six weeks' summer session. The 5 *S*s of the third group—Ab, Fr, Ri, Sc, and Sl—each served 100 times during a period of 12 weeks. Altogether 695 observations were completed in the experiment which lasted 9 months.[5] With the exception of 7 subjects—Ha, Pe, Ra, Kr, Pr, St, and Fr—whose eyes were corrected by lenses, all the *S*s had apparently normal vision.

The experiments were performed at regular periods between 8:00 A. M. and 4:15 P. M.; consequently the *S*s always entered the dark room from the light. No check was made upon the daylight which varied in intensity from day to day. The varying intensities of the pre-darkness period probably affected the after-images following zero and short adaptations to darkness, as the report of one *S*, who had been looking out of a window before entering the dark room, clearly shows. This *S* reported after no adaptation a long rectangular blue background around an imaged cross.

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*Communicated by C. A. Ruckmick.

[1] A. M. Shuey, The flight of colors, this JOURNAL, 35, 1924, 559-82.

[2] A 2.8 v. flashlight battery instead of a transformer provided current for the 'ready' signal.

[3] For description of apparatus used cf. *op. cit.*, 561-62.

[4] The fact that the 75 w. bulbs with 40 sec. exposure are relatively moderate in intensity and duration made them desirable as uniform stimuli in this problem.

[5] The writer acted as *E* throughout the experiment.

EXPERIMENTAL PROCEDURE

A preliminary experiment conducted to give *S*s practice in describing their images preceded the principal observations. In this series *E* tested 0, ½, 1, 2, 3, 4, 5, 6, and 7 min. adaptations to darkness. As a result of this procedure, 0, 3, 5, and 7 min. were used as the various lengths of *S*s' adaptation before they were subjected to the light stimulus. The latter part of the experiment included longer periods of adjustment to darkness, *i.e.* 15, 30, 45, and 60 min. During the period of adaptation[6] before each observation *E* read the following instructions to *S*:[7]

> "At a given ready signal followed by the flash of light, you will steadily fixate the black spot in the center of the Greek cross and continue to do so during exposure to the stimulus. As soon as the cross is illuminated report all visual changes noticed. After exposure, describe as fully as possible all visual effects and changes, including quality and form. Proceed from the center of the image outward. Signal as soon as you experience any image."

After she gave the directions *E* said 'Ready,' flashed the signal, said 'Now,' and switched on the stimulus light and started the stop-watch. At the end of 40 sec. *E* turned off the current and reset the stop-watch immediately to time the after-image effects and the length of the total image. *E* noted in a column at the left of the report the second at which each statement in the observation was given.[8] After every experiment *E* questioned *S* concerning his images, the rapid alteration of which occasionally prevented his giving all their characteristics as they appeared. *E* was in doubt as to whether the *S*s were all experiencing the same qualities as their descriptions indicated. Consequently she asked *S* to compare the imaged colors[9] in their different stages with the Milton Bradley rainbow, tonal, and tinted drawing papers. *S* likewise signified the various changes in the size and shape of the image by reproducing them on paper.

RESULTS

Course of the Image: (a) Rise of Image. As the conditions for both series were identical, results of the first observations following adaptations of 0, 1, 3, 5, and 7 min. will be examined in this paper with those following 0, 15, 30, 45, and 60 min.[10] These results were separated according to the characteristics of the *S*s' images which are indicated by (1) rise and subsequent duration of image; (2) hue, tint, and saturation of image; (3) fluctuations in image; (4) size and shape of image; (5) position of image; and (6) rivalry and movement of color in image. Table I following shows the relation between the length of adaptation and the moment of appearance of the after-image.

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[6] When *0* adaptation preceded an observation *E* read the instructions to *S* before he entered the dark room.

[7] *Op. cit.*, 562-63.

[8] *Ibid.*, 563.

[9] The following symbols were used in recording the colors: *R*–red, *B*–blue, *Y*–yellow, *Li*–light, *L*–lavender, *Bn*–brown, *Br*–bright, *Pk*–pink, *P*–purple, *G*–green, *g*–gray, *Bk*–black.

[10] The italic numerals *0*, *1*, *3*, *5*, *7*, *15*, *30*, *45*, and *60* are used to indicate respectively 0, 1, 3, 5, 7, 15, 30, 45, and 60 min. of adaptation.

TABLE I

Showing length of adaptation and moment of appearance of after-image (Sixty-six observations were obtained at every length of adaptation).

Length of Adaptation	First Appearance of the Image (Seconds)								
	1	2	3	4–7	8–10	11–15	16–20	21–30	31–40
0	17	6	12	17	10	4			
1	15	6	23	11	6	3	2		
3	14	16	14	16	4	2	1		
5	15	12	25	8	2	3	1		
7	13	9	21	17	5	1			
15	7	18	13	21	2	2		3	
30	9	5	12	28	9			2	1
45	9	13	11	16	10	7			
60	5	16	4	20	14	5	2		

As is indicated in the table the greatest number of images following *0* adaptation and the least after *60* appeared within 1 sec. after removal of the stimulus. As adaptation was increased, therefore, the number of images that appeared immediately decreased. More than one-half of the images following brief adaptations, *i.e. 0, 1, 3, 5, 7,* and *15,* appeared within 3 sec. upon removal of the stimulus while following *30, 45,* and *60,* the greater number came after 4 sec. Practically all of the images following every adaptation, however, appeared within 10 sec. after stimulation had ceased.

There were marked individual differences in the images as is indicated by the fact that Lu's, To's, and Pe's images often first appeared later than 4 sec. after every adaptation. 48 of the 60 images that came after 7 sec. from the removal of the stimulus were images of 6 *S*s—of Lu, To, Pe, Fr, Sc, and Ro. Two thirds of the images that came after 7 sec. following *15, 30, 45,* and *60* min. adaptation were those of Lu, Fr, Sc, Sl, and Ro.[11] On the other hand 9 *S*s always experienced the image before 7 sec. regardless of the length of adaptation. In short we may say that the time of appearance of the image altered but slightly with varying adaptations to darkness.

(*b*) *Duration of Image.* The average length of the images of *0* was 2 min. 45 sec.; of *1,* 2 min. 33 sec.; of *3,* 2 min. 16.25 sec.;

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[11] To and Pe were not subjected to experiments involving adaptations of 15, 30, and 60 min.

of *5,* 2 min. 16.7 sec.;[12] of *7,* 2 min. 16.5 sec.; of *15,* 1 min. 55 sec.; of *30,* 1 min. 56 sec.; of *45,* 1 min. 54 sec.; and of *60,* 1 min. 48.5 sec. An increase in the adaptation from *0* to *3* accordingly affected the length of the image in inverse proportion. An increase in adaptation, however, from *3* to *7* altered the duration of the image only by a few tenths of a second. As adaptation was lengthened from *7* to *15* the images became shorter as in the first instance. When adaptation was increased from *15* to *30* and *45* the length of the image varied only by 2 sec.; and between the images following *45* and those following *60* there was a difference of 3.5 sec. If we consider only the larger steps, *i.e.* *0, 1, 3, 15,* and *60,* we may conclude that there existed an inverse ratio between length of adaptation and length of the image. *S*s whose images were relatively brief or long after *0, 15,* and *30* experienced correspondingly brief or long images after *45* and *60.* Ed's and Wi's images, for example, lasted longer than the average image after every period of adaptation, while Gr's, Ra's, An's, Fr's, and Sc's were comparatively short in duration. Fr's images after every adaptation disappeared before 1 min. 10 sec. There appears to be no relation between the moment of appearance of *S*'s image and the length of its course.

(*c*) *Typical Color Changes.* After-images following all periods of adaptation may be marked off into stages according to the predominate colors in each. *Y* was the color reported most often first except after *45* and *60,* when *R* and *B* were noted more frequently. This *Y* appeared immediately in almost two-thirds of the images following *0* and *1,* in less than one-half of images following *3, 5, 7, 15,* and *30,* and in one-third and fewer of images after *45* and *60.* Other colors that appeared immediately instead of *Y* were *R, B,* and *Bk* named in the order of their frequency. *P* and *G* appeared rarely at first. No definite relation has been established between the occurrence of these colors and the adaptation periods. From one-third to one-seventh of the images altered very soon after their appearance. Within 7 sec. after the first color appeared, after *0,* approximately four-fifths of the images were *Y* and the rest *R* with the exception of one *G* image. After other adaptations, on the contrary, there was no increase in the number of *Y*s present in the image; there was, however, almost twice the number of *R* images as those that appeared first. *B* and *Bk* gave place to *R* after a few seconds of time. In this first stage only one color was present in about one-half of the images. More frequently, however, there was more than one color present after *0* than after *15* and longer adaptations. *Bk, B, R, Y,* and *G* were often experienced as halos or margins around the central colors.

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[12] Note a difference of 1 min. 18.3 sec. between the average lengths of images of present and earlier experiments both of which followed the same intensity and duration of stimulus, *op. cit.*, 565. This discrepancy may be explained by the fact that *S*s in the present experiment were greater in number and less experienced in introspective analysis than *S*s in the former one, or by the fact that adaptation to darkness was not as definitely controlled in the earlier as in the present experiment.

As we follow its course we find that the image at approximately 35 sec.[13] changed in hue. *R* followed the first color almost universally, *i.e.* in nine-tenths of the images, after *0, 1, 3, 5,* and *7*. About three-fourths of the images of *15* were *R*; while after longer adaptations only a little more than one-half were *R*. In the images where *R* did not appear *Bk* was most often experienced. *B, P, Li,* and *G* were also noted occasionally after long periods of adaptation. The hue in the second stage was apparently not affected by that of the preceding one, for *R* after the first *Y, R,* or *B*, was the color most often reported. As *R* appeared in the images following *0* to *7* it often surrounded the *Y* which became smaller and faded away leaving the *R* in its place. Although this occurred in one-half of the images after *0*, in not more than 8 observations following other adaptations was it reported. This fact indicates that the images changed quickly after adaptations as long as 7 min. The majority of images in the second stage had, surrounding the central color, between one and three hues, the number of which did not alter with changes in adaptation. Adaptation likewise did not effect the hues of these surrounding halos which in all cases were *B, Bk, Y, P,* and *G*.[14] As the images of *0* were of longer duration than those following adaptation to darkness, so the second stage in the image similarly varied with adaptation. *R* disappeared after *0* at 1 min., 23 sec. upon the average, 22 sec. after the images following 7 min. of adaptation.

As *R* or the color in the second stage of the image often surrounded *Y*, so that color following the principal one in the second stage closed in upon it. *R* faded more slowly after *0* than after adaptations as *Y* did before it. This phenomenon is shown by the fact that *R* was reported as being present in the surrounding color in twice the number of images after *0* as after *15*, etc. After these adaptations *Ss* reported that *R*, etc., vanished quickly and the *Bk* came in its place.[15] *Y* remained in the center of the surrounding color in the few images when *R* was omitted. *Bk* was most often present in the third stage after all adaptations. *Bk*, however, was not so frequently present following *0* to *7* as longer adaptations as is shown by the fact that *Bk* appeared in one-half of the images of *0* to *7*, in three-fifths of those of *15* and *30*, and in seven-ninths and five-sevenths of those of *45* and *60*. Colors other than *Bk* that took the place of *R* were *B, GB,* and *G* and occasionally *P*. *B* and *P* were noticeably present after *0*. All images in this stage with the exception of 4 or 5 possessed one or infrequently two halos. *Y* was the halo experienced in about one-half of all images with the exception those of *60*. After this adaptation *g* and *Pk* were experienced almost as repeatedly as *Y*. Other halos around the *Bk* or *B*, etc., were *LiG, LG, g, Pk,* and a *B* which was seen rarely after shorter adaptations. A complementary relation frequently existed between the central color and its halo; for *Bk* was surrounded by the complement *Li* and where 2 halos were present around the central image the *Y* halo appeared within a *B* or *B-P*, and the *G* within a *P*.

After the third color sequence almost one-half of *Ss*' images did not change in hue.[16] About 36 of the 66 images following each adaptation altered at least once after *Bk* or the third color, and approximately 9 of the 36 changed two or more times.[17] Many of these changes however were only variations in halos. *Bk* or *g* with just enough *Li* or color around it to make it distinguishable was continually experienced in the latter part of the image.[18] The halos around *Bk* were usually *Li-g*, *Li-B*, *Pk*, or *Li-G*.

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[13] This time varied proportionally with the length of the period of observation.

[14] Ss reported a *YG* or *Pk* halo around the perceived cross. The halo of the image was probably a result of this phenomenon.

[15] *R* remained in the center of *Bk* or its surrounding color from 10 to 55 sec.

[16] Fewer of these alterations in color occurred in short rather than in long images.

The image, therefore, faded away as *Bk* with a little *Li-g*, or colored haze against the darkness of the field of vision. Colors near the end were experienced in a few more images following *0* than adaptation periods.

When the principal color decreased in saturation it also darkened in tint regardless of adaptation. The halos or surrounding colors, however, often decreased in chroma with no alteration in tint, as they were described as remaining *Li* or as becoming *Li-g*. Images following *0* began to lose their color between 1 min. 30 sec. and 3 min. After *1*, *3*, *5*, and *7*, 55 of every 62 images began to diminish in chroma between 1 and 2 min. upon removal of the stimulus. Succeeding longer adaptations to darkness a few *S*s experienced grayish colors in the very first stage of the image but the most of their images darkened and became of poorer saturation at and after 30 sec. With one or two exceptions, after the hues began to decrease in saturation and darken in tint they continued hazier and darker until they finally vanished.

On comparing the images with the colors in the charts *S* usually indicated that after *0* the first *Y* was like '3' or '13;' after *15* it was '2' or '3;' and after *60* usually '2.' The '2' is somewhat grayer than '13' and much grayer than '3.' The *Li* halo in the latter part of the image was like '2' or grayer than '2.' The latter part of the image, *S* noted, was of poorer saturation than the first part. *R* in the second stage was like '9,' a *R-O*; '10,' a *Li-R*; or '11,' a vivid *R*; and when it was reported as growing darker it was '32' or '33,' a less saturated and darker color than the others. The vivid *R*, '11,' was more uniformly indicated after *0* than after adaptations. The *B* in the cross was '19,' a well saturated *B*, and that in the halo was a *B-G*, '21,' or a *Li-B*, '20,' *P* or *L* was between '39,' a color of medium tint and saturation, and '26,' a *Dk-G*.

The following introspective reports of *S*s concerning the quality of image supplement the preceding section:

An "First *G* bright; other colors dark." *0*.
Fr "*Y* in *Bk* cross is grayer and duller when it comes back." *0*.
Ed "*B* border rich. First *B* that appeared with *Y*, was cold but not a dark *B*." *0*.
Su "*R* came in spots; first a *Li-R* that became darker. I saw these same colors, *R*, *Y*, and *G* on coming into the room from the light." *0*.
Sc "Colors glow like a flame. The edges are shining." *0*.

Ed "*Y* gets more distinct after a sec. or two. During stimulation I saw a double cross with *R* and *G* edges. *R* gets darker. *B* diamond around *R* darker than powder *B*." *1*.
Su "Long *B* strip and cross in it at first. I saw the same *B* strip when I came from other room as I had been looking out of the window." *1*.

Lu "Cross *Li* and then became darker." *3*.
Ne "Outline around *R* may have been dark *B* at first. It became *Bk*. *R* looked as if electric light were behind it." *3*.

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[17] From 1 to 4 images following *15* and longer adaptations did not change in quality from the beginning to the end. There was only one change in 2 to 7 images following each period. About 14 times as many images of *60* and of *0*, moreover changed only once.

[18] *B* and *G* spots were occasionally reported instead of *Bk*.

An "*B* became darker all time till it faded." *5.*
Lu "*Pk* halo like an electric light." *5.*
Oa "Only a dark mass at the end." *5.*
Ra "Halo around *Bk* seemed like a dim electric light." *5.*

Wi "Vertical bar became *Bk* first while the rest was still medium *R.*" *7.*
Ha "*B* halo seemed like gas flame." *7.*

Ha "Cross and background became indistinct." *15.*
Wi "*Pk* background gets duller." *15.*
Co "Everything dissolves into haze." *15.*
Sc "*Y* sun glow around *Bk.*" *15.*
Fr "*Bk* gets dimmer in *Y* background." *15.*
Lu "*R* not like paper. Like electric light." *15.*

Ab "Colors not like colors on chart. They are more like electric lights." *30.*
Co "*Bk* bars melt into *Y* haze." *30.*
Ed "Rosy background gets faint and muddy before it vanishes." *30.*

To "Colors get mottled and muddy after a few minutes." *45.*
Co "Ray of light over it and it all goes." *45.*
Sc "*Bk* is faint like a shadow against the deep shadow. *G* a 'flat' color." *45.*

Lh "The darker the cross, the lighter the halo." *60.*
Ne "*B* gets dimmer." *60.*
Pe "*Y* wavery and faint near the end." *60.*
Ab "*Pk* background gets lighter as cross gets darker." *60.*
Sc "Image blurry near the end." *60.*

The after-images proceeding from all periods of adaptation, therefore, were similar in the following respects: they could be generally divided into periods, in which one color *Y*, *R*, *Bk*, or *G* or *B* predominated, a complementary relation existed between the tint of the central part of the image and its halo but not one between the hues, the image decreased in saturation and at least the central part of it darkened in tint. The images, especially those after brief adaptations, were often described as being like "lights" not as flat or surface colors.[19] Adaptation influenced the quality of the image in a comparatively small degree. The first *Y* was most constant and had more halos around it after *0* than after adaptations. The number of images in which *R* was present decreased as adaptation increased. Colors changed more quickly after *15* and long adaptations than after *0* to *7*. *Bk* increased in prominence and the chroma decreased earlier with increases in length of adaptation.

(*d*) *Fluctuations.* There was a little correlation between the length of adaptation and the number of fluctuations in the image. For although there was no difference between the number of them following adaptations of *0* and *7* there were about twice as many vacillations following *0* as *15*, *30*, etc. After *0* furthermore the greatest number of these occurred between 1 and 2 min.; after *15*, *30*, etc., between 30 sec. and 60 sec. These fluctuations were more generally scattered throughout the image after *0* and *1*, than after long adjustments to darkness. The individual differences in regard to this characteristic of the image are marked. For example, Ed, in 27 images, experienced 10 fluctuations, one to every 2 or 3 images. Pe, on the other hand, reported on the average 4 fluctuations with every image. Ra experienced approximately 5 changes during every image, while several *S*s seldom reported any vacillations.

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[19] M. F. Martin, Film, surface, and bulky colors and their intermediates, this JOURNAL, 33, 1922, 451.

(*e*) *Shape and Size.* The varying adaptations had little effect upon the shape of the after-image. After *15* and longer periods in one-third to one-half of the images the halo appeared a few seconds before the cross, while after *0* the background appeared first in only one-sixth of them. With few exceptions[20] the imaged cross was the shape of the perceived one. As the image progressed it was often thinner and sometimes "pointed at the ends." Occasionally a part of the cross, *i.e.* one or two 'bars,' vanished irrespective of adaptation. After *60* the cross in more than four-fifths of the images vanished between 10 sec. and 1 min. before the halo did, after *15* the cross vanished previous to the halo in one-half of the images, while after *0* it did so in less than one-twelfth of the images. The shorter the adaptation, therefore, the more persistent was the cross in its halo.

The size of the cross which was mentioned in two-thirds of the images was rarely observed as larger than the perceived cross. The first cross was reported as smaller than the perception in one-sixth of the *S*s' images after *0*, and in one-third of them after *15*, *30*, etc. The cross with its halos continued to diminish in size to the end of the after-image. After all adaptations only 4 images were reported as increasing in size and 2 as remaining the same size as they were in the beginning of the series. After long adaptations, however, the imaged cross decreased earlier than after short adaptations. For instance, between 30 and 60 sec. one-fourth of the images decreased after *0*, one-third after *15*, and one-half after longer adaptations. The other images began to noticeably decrease between 1 min. and 1 min. 30 sec. after the beginning of the image. Some descriptive phrases used by *S*s concerning the size of the cross are "shrinking," "thinner," "getting smaller and irregular," "thinner and pointed," "about one-half the sensation now." When the image became very small the darkness was often reported as "swallowing it up" or "casting a shadow over it," The cross accordingly was especially constant after *0* adaptation and fairly constant after any adaptation; it was the size of or a little smaller than the perception when it first appeared and decreased as the image continued with more diminutions appearing earlier after long adaptations.

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[20] An *S* occasionally described the image as 'fatter' or 'longer' than the perception. Pr often experienced the first cross of the image with round balls on its ends.

(*f*) *Position of Image.* Practically nothing can be said of the position of the image independent of its movement since it was reported in only one-ninth of all the images. When the position was noted the image was always described as being far away and very small in size. One *S* said that it seemed "on the level with my eyes" and another that it was "far away and not clear."

(*g*) *Movement and Rivalry of Color.* Movement, which was noted in about one-half the experiences, was most pronounced during the second 30 sec. of the image. The image usually went "up and to the right." Twice as many times, regardless of adaptation, was it considered as going up rather than down, and only 2 times did a *S* note that it went to the left. Sometimes Ed reported that she was looking over her shoulder. Once Su reported that when she kept her eyes closed the image did not move. Ed on the contrary, thought it make no difference in the image whether her eyes were closed or opened.

In practically all after-images regardless of adaptation, colors were reported as moving from the periphery to the center. *R* seemed to move in quickly or slowly on *Y* and in turn *R* was consumed by *Bk* or a color around it. The following introspections after various adaptations indicate this fact:

Ba "*Bk* goes in over *Pk*." *0.*
Wi "*Dk* outline eats *R* up." *0.*

Gr "*G* cross always comes from sides and bottom." *1.*
Ha "*R* first came in from points, met in center and poured out." *1.*

Ed "*R* moves in and absorbs *Y*." *7.*

Ri "*Bk* moves on *R* and makes it darker." *30.*
Sl "*P* thickens over *R*, *Bk* comes in over it from the sides like a curtain." *30.*

Co "*Y* halo covers up cross." *45.*
R "*Bk* comes in till covers up whole cross." *45.*
Sl "*R* comes in over *Y* cross." *60.*

CONCLUSION

In conclusion we note that to a small degree the length of adaptation influenced the flight of colors, qualitatively and quantitatively.

(1) As adaptation was increased the number of images in which *Y* and *R* were experienced decreased and those in which

Bk occurred increased. All colors likewise possessed fewer surrounding halos and changed more quickly with increase in adaptation.

(2) As adaptation was lengthened the number of images that appeared immediately decreased as did the length of the images.

(3) The fluctuations were more numerous and more rapid at the beginning of images following decreasing adaptations than following increasing periods.

(4) The cross appeared after the halo did and vanished before it more frequently, with increase in adaptation. Furthermore, as adaptation to darkness was lengthened the image decreased in size at an earlier stage in the process.

(5) Adaptation apparently did not affect either the relative amount of movement or the absolute position of the image.

EFFECT OF SERIAL POSITION UPON MEMORIZATION

By Edward S. Robinson and Martha A. Brown, University of Chicago

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I. Problem

The readiness with which an associative connection is formed depends upon its position in any series of which it may be a part. To this principle of memorization a substantial number of experiments lend support. In general, items appearing very early or very late in a list show a relatively high rate of learning, while items appearing between these extreme positions show a relatively low rate. Regarding the advantage of late position there is almost universal agreement. The advantage of early position, however, is not so certain. Welsh and Burnett,[1] for example, claim that primacy is really a specious facilitator of learning and that the first item is better learned only because there is a chance of rehearsing it while the other items are being presented. The factor of primacy would thus reduce to that of frequency. Woodworth and Poffenberger[2] see the advantage of primacy residing in the fact that the earlier part of a series suffers relatively little from proactive (associative) inhibition. It is apparent, therefore, that the effects of serial position upon memorization still constitute something of an issue. There are, furthermore, subsidiary problems, such as the relation of the total length of the memorized material to the importance of the positions within it, which have scarcely been touched.

In the course of two recent investigations[3, 4] in the Chicago laboratory there have accumulated considerable data bearing upon this question of serial position. We have results for two kinds of material and for a number of lengths of each. It is our main purpose to present and discuss these data. But first it seems advisable to bring together from the literature the principal results hitherto obtained.

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[1] G. B. Welch and C. T. Burnett, this Journal, 35, 1924, 396-401.

[2] R. S. Woodworth and A. T. Poffenberger, *Textbook of Experimental Psychology* (mimeographed edition), 1920, 71-72.

[3] E. S. Robinson and W. T. Heron, *J. Exper. Psychol.*, 5, 1922, 428-448.

[4] E. S. Robinson and C. W. Darrow, this Journal, 35, 1924, 235-243.

II. Earlier Findings

Experiments dealing with serial position in relationship to memorization may be divided into two distinct types: (a) those in which the order of testing the various associations is definitely controlled, and (b) those in which the subjects are free to recall the memorized items in any order. Of course there are other differences in experimental procedure which have affected specific results, but we believe that this difference is the most important.

(a) *Experiments with Controlled Recall:* Miss Calkins[5] used the method of *paired associates*; 7, 10, or 12 pairs of colors and numbers were presented. Each color was exposed for 4 sec. and its numeral which followed immediately was likewise exposed for 4 sec. There was an 8-sec. interval between pairs. At the close of a series the colors were again presented but in altered order. The subject wrote down the 'suggested' number as every color appeared. The general procedure employed by Miss Calkins will be sufficiently clear if we consider her description of her first group of experiments: "some one color appeared several times in each series, once in an unimportant position with any chance numeral, but also once or more in some emphasized connection—either repeatedly with the same numeral (a 'frequent' combination), or at the very beginning or very end of a series (cases of 'primacy' and of 'recency'), or with a numeral of unusual size or color (an instance of 'vividness')." Thus she was able to determine the strength of the so-called emphasized connections as compared with that of the unemphasized connections. Our own interest is merely in the results for 'primacy' and 'recency.' She found that 'primacy' shows a certain advantage over chance positions where the total list is relatively short. But even in such a list 'recency' is a more important factor. With long lists 'primacy' is ineffective.

W. A. Owens[6] conducted an experiment which had as its purpose the critical evaluation of certain of the conclusions drawn by Calkins from the work we have just cited. According to Owens, Miss Calkins went further than her facts justified in interpreting the high value of recency in terms of time interval between memorization and test. He shows that a final association of a series may be learned readily for either of two reasons or for both. The final association may be tested sooner after it has been formed than are the other associations. But even if this were not true, the final association might benefit simply by being in an emphasized position in presentation. Our interest is mainly in this question of the value of various presentation positions. In regard to it Owens' data are quite convincing. Like Calkins, he used a form of the method of *paired associates.* Seven pairs of colors and numerals or nonsense syllables and numerals made up his lists. Unlike Calkins, he used a single syllable or color only once in a single list. His results regarding position are not, therefore, based upon the double possibility of association with a single color or number, but merely upon the learning values at different positions. This seems to us a simpler and more straightforward method than that of Calkins. True, it assumes that the different syllables or colors are, apart from position, of the same difficulty, but this assumption is readily justified if massed results are considered. In order to have some basis for distinguishing the factor of time interval between learning and test from that of serial position, Owens calculated the average time interval for each serial position. The following table includes those of his results which are most relevant to our discussion:

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[5] M. W. Calkins, *Psychol. Monog.*, 1, 1896, No. 2, 35-56.

[6] W. A. Owens, unpublished Master's thesis, University of Chicago, 1911, typed copy in the University library.

TABLE I

Average of Entire Experiment in which 11 *S*s Memorized Twenty Lists Each

	Position						
	1	2	3	4	5	6	7
% Recalled	39.3	34.5	36.3	35.5	34.1	37.1	60.0
Time in sec. between learning and recall	76.8	67.6	58.0	48.6	38.0	26.8	20.4

The final position shows the outstanding advantage, though it is clear that advantage may well be the result of the relatively short interval which, on the average, separated the learning and the test at this serial position. The first position shows only a slight advantage, but when one considers that the time interval in this case is markedly the longest, there seems good reason to believe that the actual advantage of this position is somewhat greater than the results directly show it to be.

Ebbinghaus[7] reported an experiment in which the time interval between memorization and test was the same regardless of serial position in presentation. He used the well known *prompting* or *anticipation* method in which the items of the memorized list must be recalled in original order. When failure occurs at any item, that item is displayed to the subject. The advantage of this method is that a test trial is also a further learning trial. The results in terms of number of promptings (an inverted measure of efficiency) are presented below. They are based upon a number of lists of 10 and 12 nonsense syllables. Ebbinghaus was the subject throughout.

TABLE II

	Serial Order											
	1	2	3	4	5	6	7	8	9	10	11	12
No. promptings												
10 syll. series	0	3	6	9	23	24	31½	25	23	5½		
12 syll. series	0	11	21	13½	35	36	36	29½	43	37½	34	11

With both lengths we here find a strong advantage at both the beginning and end of the series. These results depart from those of Calkins and Owens in that they show a greater advantage for the early than for the late positions. In so far as we are interested in the effects of serial position, this departure is worth taking into account, because the Ebbinghaus experiment is, after all, the first among those mentioned that, by establishing equal, or nearly equal, time intervals between learning and testing of the various items, approaches direct measurement of the importance of serial position.

Warden[8] conducted an experiment in the field of motor learning similar to that of Ebbinghaus. Stylus mazes were learned by human subjects. Two patterns were employed and each pattern was learned with either the right or the left hand. Each of these four conditions was represented by 15 *S*s. In maze learning the efficiency of the *S* after one or more trials is tested by putting him through another trial under conditions which are essentially like those of the learning itself. Thus the order of learning is also the order of testing and one element in the complex act is not likely to show to advantage because of the benefit accruing from a shorter time interval between learning and testing. This is the reason why we consider Warden's experiment importantly similar to that of Ebbinghaus.

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[7] H. Ebbinghaus, *Grundzüge der Psychologie*, 1902, 1, 624-626.

[8] C. J. Warden, *J. Exper. Psychol.*, 7, 1924, 98-116.

Warden divided his mazes into three sections: (1) the first three cul-de-sacs; (2) the next four cul-de-sacs; (3) the last three cul-de-sacs. Those of his results that bear most directly upon the present problem are in terms of the mean number of cul-de-sacs entered in different sections of the mazes during the complete acquisition of this habit and in terms of the mean number of trials required to eliminate cul-de-sacs in these successive sections.

TABLE III

	Section		
	First	Middle	End
Av. cul-de-sacs entered	11.2	23.3	16.9
Av. trials required to eliminate cul-de-sacs	16.4	34.8	23.7

Here again the merits of the first and final serial positions are clear and, in agreement with the Ebbinghaus results, the first positions are better than the final ones.

Finkenbinder,[9] who used a form of *prompting* method in connection with his study of the curve of forgetting, has reported that: "the initial and final syllables of the series were the first to be mastered; the central section of the series, usually the seventh or eighth syllable, was the last to be learned." He does not, however, give any quantitative results along this line. Neither does he mention any difference in rate of learning between the first and final positions.

(b) *Experiments with Free Recall:* In a number of the experiments upon the effects of serial position there has been no control of the order in which the various items in the list have been tested. The *S*s, after the presentation of the material, have simply been instructed to recall as much of it as possible. Before considering the individual experiments of this type, we shall point out one result that is naturally to be expected from them. If, at the beginning of the process of recall, *S* is not required to recall in any set order—for instance in the order of learning—there is a strong likelihood of his first recalling the item most recently observed, which is of course the one occupying the final position. When this occurs the final item is protected against being forgotten, while the earlier items in the list have more than a fair chance of being forgotten. Thus, in experiments of this type there should be a greater advantage for finality than its rôle in learning warrants and also a smaller advantage for primacy than its rôle warrants.

Bigham[10] employed 10 types of memory material: (1) visible numbers, (2) audible numbers, (3) visible colors, (4) audible colors—color names, (5) visible geometrical forms, (6) audible names of forms, (7) visible words, (8) audible words, (9) visible nonsense syllables, (10) audible nonsense syllables. The *S* was presented once with a list of one of these types and then required to recall what he could remember. The results were treated in such a way as to throw light upon a number of problems, among them that of serial position. In this case the data seem to be the result of pooling the results obtained from 6 *S*s performing under all 10 conditions. Three measurements of efficiency are given for each serial position. They are *misplacements*, *forgetting*, and *insertion of wrong words*, all of them being inverse measures. The following table presents these values as determined from tests made 2 sec. after presentation.

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[9] E. O. Finkenbinder, this JOURNAL, 24, 1913, 8-32.

[10] J. Bigham, *Psychol. Rev.*, 1, 1894, 453-461.

TABLE IV

	Serial Position									
	1	2	3	4	5	6	7	8	9	10
Misplacements	2.5	7.5	11.7	12.5	15.7	12.1	10.8	11.3	6.3	2.5
Forgetting	5.8	11.3	15.9	23.8	20.4	14.6	23.8	14.6	12.1	6.7
Wrong words	3.3	1.3	1.7	2.1	1.3	0.4	0.8	1.3	0.8	0.8

According to Misplacements and Forgetting the first and final positions show about the same advantage over the other positions. There is little to be said about the insertion of Wrong words except that it is not at all closely correlated with the other types of efficiency. Perhaps it is natural to expect more errors of this sort where efficiency is in other respects relatively high.

W. G. Smith[11] employed lists containing 10 nonsense syllables which were visually presented at a rate varying with the desires of the *S*s. Presentation was visual and recall was written. Results are given where memory was tested after 1, 3, 6, 9, and 10 presentations. Eight *S*s served in the experiment. The following table brings together the findings regarding serial position.

TABLE V

Percent Recalled after Different Numbers of Presentations

Number of Presentations	Serial Position									
	1	2	3	4	5	6	7	8	9	10
One	81	52	24	16	16	24	26	26	62	84
Three	84	67.5	39	38	34	33	29	44	69	92
Six	81	61	42	42	34	32	46	54	74	85
Nine	89	67	49	41	32	33	48	64	77	93
Twelve	92	58	46	41	56	57	57	61	84	91

Again we see that both the first and final positions are advantageous for learning and again we have a case in which the difference between them in this respect is very slight. Fortunately Smith recognized that the order of recall might be important. He tells us that, though the first two syllables were usually recalled first, the last two as a rule came next. This means that on the average the time between memorizing and test was shorter for the last two than for the first two syllables. As we have already pointed out, there would thus be a tendency to exaggerate the merely positional advantage of the final syllables. If in this experiment of Smith's the syllables had been recalled in original order so that the time interval would have been of the same magnitude whatever the serial position, it is quite possible that the first positions might have proved to be appreciably more advantageous than the final positions.

Woodworth and Poffenberger[12] describe an experiment performed at Columbia in which lists of 10 digits were read to a class of 26 students. In all, 10 lists were used. Immediately after the reading of a list the *S*s attempted to reproduce it in writing. The results follow:

TABLE VI

	Serial Position									
	1	2	3	4	5	6	7	8	9	10
% of Error	1	10	17	23	27	24	24	19	13	2

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[11] W. G. Smith, *Psychol. Rev.*, 3, 1896, 21-31.

[12] R. S. Woodworth and A. T. Poffenberger, *op. cit.*, 71-72.

Woodworth and Poffenberger point out that the relative advantage of the first position is not quite so great as in the Ebbinghaus results, but they do not mention that the order of recall was fixed in the latter case, a fact that may easily account for all of this small difference.

Three experiments on serial position have recently been reported from the Bowdoin laboratory. The first, conducted by H. W. Lamb and described by Welsh and Burnett;[13] was an attempt to test the laws of primacy, frequency, recency, and vividness. Nonsense syllable lists of 8 items each were employed. Six *S*s were presumably presented with 8 lists during the course of the experiment. The presentation was serial and visual. After the exposure of a list *S* recorded what he could remember of it. The recall values for all 8 positions are not given, but the relative values of primacy and recency are as 44 and 65 respectively. M. W. Waterman repeated this experiment two years later with 11 *S*s. He obtained a reversal of the relative advantages of primacy and recency. The values obtained for these two factors were as 64 and 55 respectively. A similar, but somewhat more extensive experiment was conducted by Welch and Burnett. Twenty-six *S*s were employed. In this last experiment there was an important innovation. The *S*s were given specific instructions not to rehearse the earlier syllables during the presentation of the later ones. The relative values of the different serial positions are given as follows:

TABLE VII

	Serial Position							
	1	2	3	4	5	6	7	8
Recall Scores	30	34	37	27	31	29	59	142

Welch and Burnett are quite convinced that their instructions against rehearsing the earlier syllables during the display of the later ones is the main explanation of their failure to show any degree of advantage for the first syllables. Of course this may be true, but they tell us quite frankly that their *S*s often wrote the last syllable immediately upon being given the signal to write. Their experiment is, therefore, open to explanation in terms of the greater time interval between memorizing and recall in the case of the first syllables. Welch and Burnett have somewhat rashly presumed that the advantage of primacy as it has been shown in the past rests upon a failure to prevent rehearsals of the first syllable. But their discussion of the history of this problem is based upon second or third hand sources, namely three elementary textbooks and one elementary manual of experiments. If they had gone to the sources they could hardly have missed the fact that Smith's experiment showed a substantial advantage for the first syllables, even though the *S*s were required to read each syllable aloud during presentation, a procedure which it seems to us, was a better guarantee against rehearsal of the first syllables than the instructions upon which Welch and Burnett put such faith, especially when it is remembered that Smith's exposure rates were all faster than one syllable a second.

Certain observations on casual memory reported by Galton and by Calkins[14] show something of the importance of primacy. These workers recorded the relative numbers of childhood and recent associations. Professor Calkins has included the results of both studies in the following simple table.

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[13] G. B. Welch and C. T. Burnett, *op. cit.*

[14] M. W. Calkins, *Psychol. Rev.* 5, 1898, 461-462.

TABLE VIII

Records of	Number of subjects	Percent Childhood Associations	Percent Recent Associations
Galton	1	39	15
Wellesley Studs.	90	14.7	32.7
Old and Middle-Aged Persons	87	33.4	30.9

Obviously there were many other factors aside from serial position effective in determining the distribution of these reminiscences, but the facts do, nevertheless, point strongly toward the importance of primacy.

Summary of earlier findings. Items occupying an early or late serial position clearly have an advantage over those in an intermediate position. All of the experiments cited in which a comparison is possible between learning of items late in the list and those in middle positions, show that the late items are learned most readily. Because these late items are often tested sooner after they have been learned than are other items, the advantage which they seem to show may be somewhat exaggerated. The fact, however, that finality appears to advantage in the experiments of Ebbinghaus and Warden seems to indicate that the final positions have a certain merit even when they are not favored by less chances of being forgotten. The advantages of primacy are not shown quite so universally as are those of finality. Welch and Burnett found no advantage for primacy and Calkins found that this factor was of no importance in learning long lists of material. But in most other cases the advantages of primacy are clear and occasionally they show up more clearly than those of finality.

III. SOURCES OF OUR DATA

In the first of the two experiments from which our own data are drawn the materials memorized were lists containing 6, 9, 12, 15, or 18 nonsense syllables. The *S*s had previously had several experiences in learning nonsense syllables. In our second experiment the memorized materials were lists of 4, 6, 8, or 10 three-place numbers.

The method employed in the learning was a form of the prompting method which is sometimes spoken of as the method of *anticipation.* The list was presented serially, 2 sec. per item. After the first presentation of a series as a whole and upon the presentation of the first item, *S* tried to spell the second item. When the second item appeared, he tried to spell the third item. When the third item appeared, he tried to spell the fourth, and so on. The same procedure was followed until *S* could go through the entire list without an error or omission. During every presentation after the first a record was made of the syllables which were correctly anticipated within the given time (2 sec.) and of those upon which *S* failed. The responses were scored either *right* or *wrong.* There were no partial credits. That item which was actually first in the list did not receive a score, because it was used merely as a cue. Thus we obtained a very complete picture of the progress of the learning, not only for the lists as wholes, but also for the separate items.

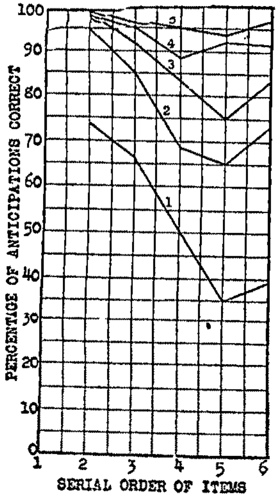

FIG. I
SIX-SYLLABLE LISTS

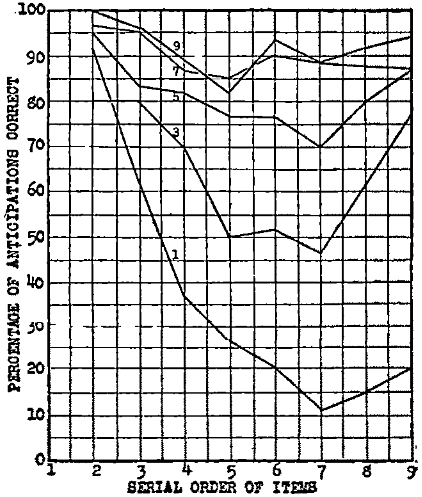

FIG. II
NINE-SYLLABLE LISTS

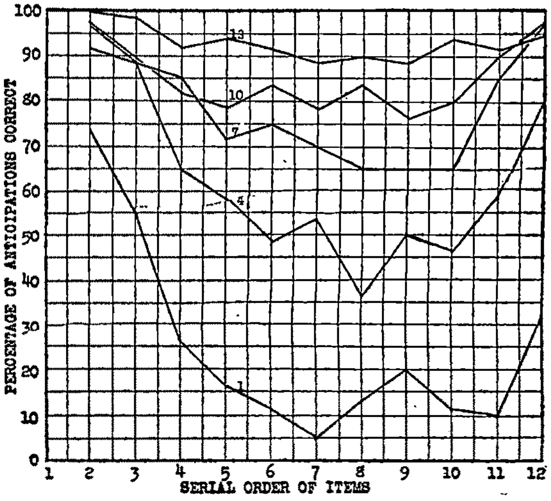

FIG. III
TWELVE-SYLLABLE LISTS

NOTE: The numbers on the curves indicate repetitions.

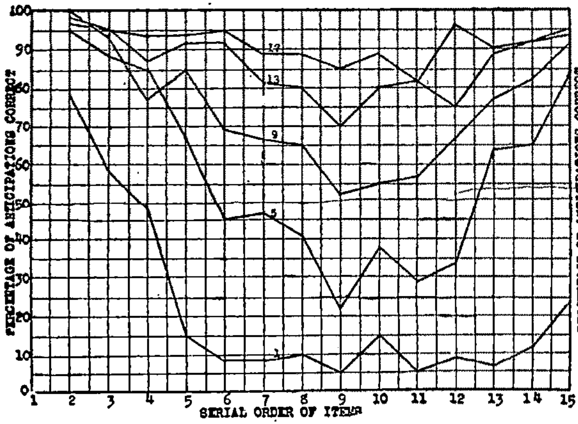

FIG. IV.
FIFTEEN-SYLLABLE LISTS

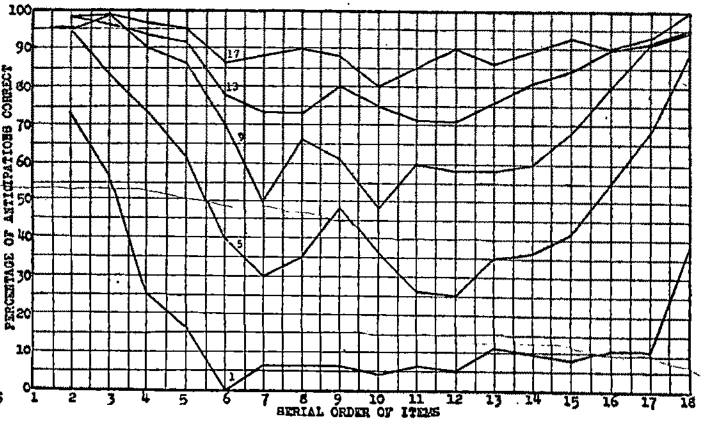

FIG. V.
EIGHTEEN-SYLLABLE LISTS

NOTE: The numbers on the curves indicate repetitions.

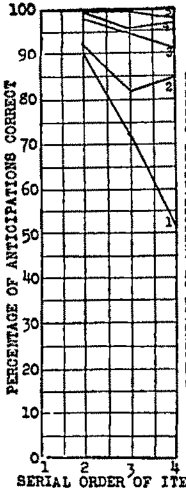

FIG. VI.
FOUR-NUMBER LISTS

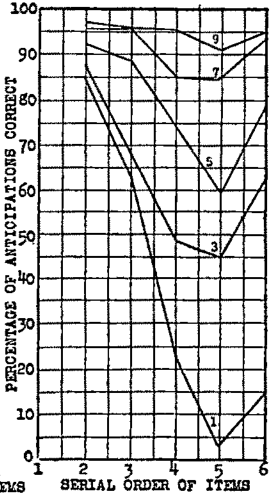

FIG. VII.
SIX-NUMBER LISTS

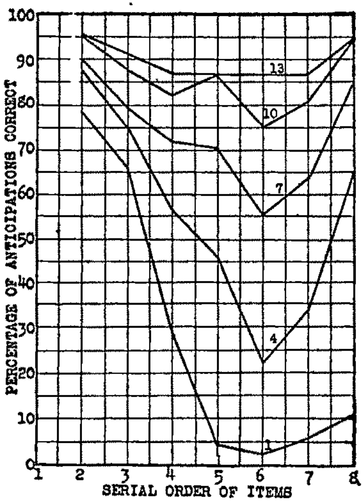

FIG. VIII.
EIGHT-NUMBER LISTS

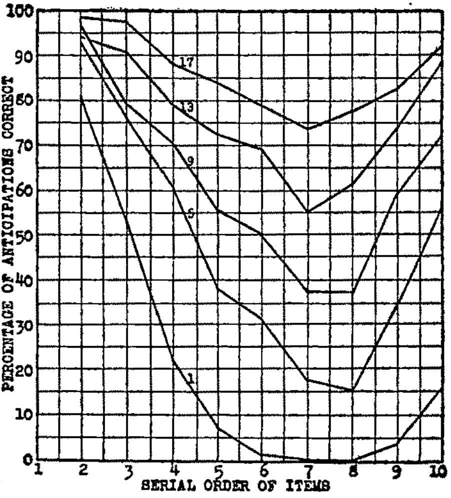

FIG. IX.
TEN-NUMBER LISTS

NOTE: The numbers on the curves indicate repetitions.

IV. EFFECT OF SERIAL POSITIONS IN LISTS OF DIFFERENT LENGTH

Figures I to IX show the percentage of correct responses for each item during the various repetitions. Every curve for the syllable lists is a composite of 60 performances. Ten *S*s learned 6 lists of every length. Thus a percentage value in one of these curves of 91.7 would mean that of 60 attempts to anticipate a syllable in a certain position in a list of a certain length 55 were correct. Every curve for the number lists is a composite of 88 performances.

Eleven *S*s learned 8 number lists of every length. We are presenting these results in graphs because we feel that the rather extensive data involved are more readily surveyed in that form. The numerical values have been tabulated elsewhere[15] and we do not believe it necessary to present both the graphs and tables. Not all of the repetitions involved in the complete learning of every length of list are represented. If they were, nothing of significance would be added and the facts would simply seem more complex than they really are. We do not believe that there is any reason, however, why the method of sampling which has been employed does not give a very ample picture of the facts as a whole.

That primacy and finality are both favored positions is clearly evident in this set of figures. Usually there are a number of middle positions which seem to be at a distinct disadvantage, although in the first stages of learning in the case of the shortest lists this is not true. In the 6-syllable lists as observed after one presentation there is only one syllable, the fifth, which is less thoroughly mastered than the last. In the four-number lists as observed after one presentation there is a steady decrease in advantage from the first to the last position.

In our experiments primacy is consistently a stronger factor in favoring learning than is finality. We do not believe that this is due to rehearsal of the first few syllables during the presentation of the later ones. *S* actually recited the items aloud, just before they appeared, if possible, and, if not, as soon as they appeared. In the syllable lists the separate letters were recited and in the number lists the separate digits were, of course, recited. This fact gives fairly good assurance that *S* was engrossed with that part of the list which happened to be before him at any given time.

The primacy factor also affects more positions than does that of finality. Judging again on the basis of the first repetition, in the syllable lists between the first 4 and the first 6 items show a benefit from their early position. In the number of lists

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[15] Martha E. Brown, Master's thesis, University of Chicago, 1925, typed copy in the University library.

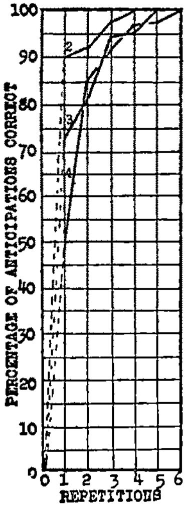

FIG. X.
FOUR-NUMBER LISTS

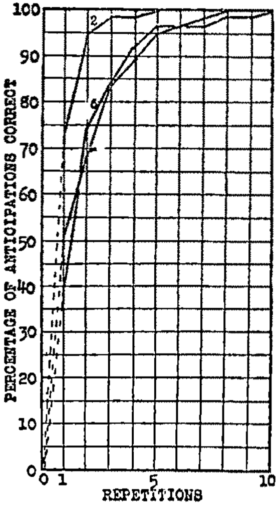

FIG. XI.
SIX-SYLLABLE LISTS

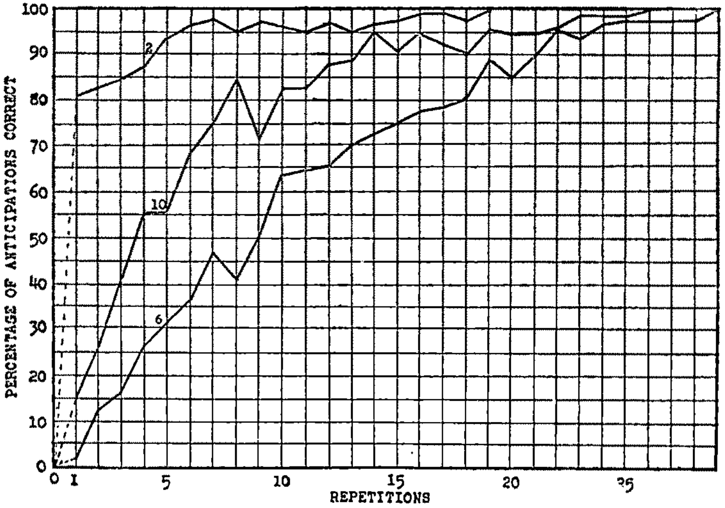

FIG. XII.
TEN-NUMBER LISTS

NOTE: The numbers on the curves indicate item in list.

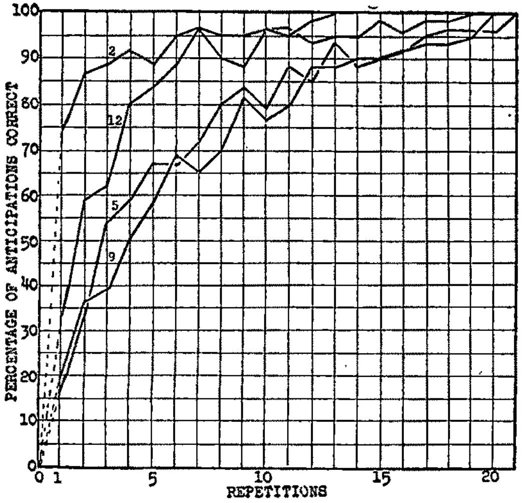

FIG. XIII.
TWELVE-SYLLABLE LISTS

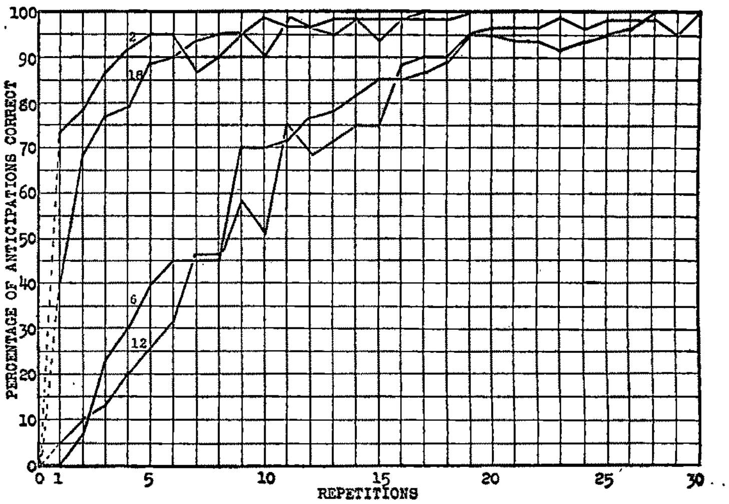

FIG. XIV.
EIGHTEEN-SYLLABLE LISTS

NOTE: The numbers on the curves indicate item in list.

from 3 to 5 of the earlier items show such a benefit. When the lists become subjected to more and more repetitions the decreasing of the primacy and recency effect, as we consider later, naturally becomes less marked. In the first repetition of both types of lists the maximum number of positions included in the finality effect is two. With the progress of learning the finality effect takes on a somewhat wider scope in the 15- and 18-syllable lists, but in practically all instances the scope of the primacy effect remains greater throughout the learning.

V. MEMORIZATION CURVES FOR INDIVIDUAL ITEMS

So far as we are aware none of the investigators of the effects of serial position upon memorization has discussed the form taken by the memorization curve for individual items. There have been studies of memorization curves for lists as wholes,[16] but we feel that the course of memorization for individual items is also worth consideration. Because of the nature of our records we have been able to construct curves showing the nature of the process of memorizing as it shows itself in the various units of our materials. These curves are shown in Figures X to XIV. Because of considerations of clarity and space we are presenting only a small number of the curves furnished by our data. Those omitted, however, fall into the picture of the samples given.

The curves all take the negatively-accelerated form so typical of learning curves for all kinds of activities. The degree of negative acceleration varies with the serial position of the item in question. The second and last items in the lists show a much sharper deceleration than do those in the middle portions. And the deceleration shown by the second item is in practically all instances more marked than that shown by the last item.

Among the curves here presented there are three that manifest early positive acceleration. This is true of the sixth and possibly of the twelfth syllable in the 18-syllable lists and for the sixth number of the 10-number lists. In each of these cases the item showing this positive acceleration is situated in a relatively unfavorable position in a relatively long list. One can readily appreciate why early positive acceleration might be expected under such circumstances. If a list is very long there will be some items the learning of which can scarcely get under way at all until the learning task is simplified by the mastery of part of the list. Of course, if learning cannot begin at once in all parts of the list, we should expect it to involve at first only those items which are situated favorably, *i.e.* at the earlier and later positions. After these items are, so to speak, able to take care of themselves, we should expect the learning of the less favorably located items to gather momentum.

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[16] C. L. Kjerstad, *Psychol. Monog.*, 26, 1919, Whole No. 116; E. S. Robinson and W. T. Heron, *op. cit.*; E. S. Robinson and C. W. Darrow, *op. cit.*

VI. SUMMARY

It is evident from the work of earlier investigators that both primacy and finality of serial position facilitate memorization. Except in the experiments of Ebbinghaus and of Warden, the importance of primacy has probably been somewhat undervalued and that of finality somewhat exaggerated. In our own experiments the advantages of both primacy and finality are pronounced; and primacy shows an even greater advantage than finality. We have also been able to show that this primacy effect extends in some degree over several items in the first portion of a list, while the finality effect is much more limited in scope. From our data we have constructed curves of memorization for representative separate tiems. In their general negative acceleration these curves are like most other curves of learning. The sharpness of deceleration varies according to the serial position of the item in question. The more favorably situated items show a sharper deceleration. There is evidence of early positive acceleration in the case of items which are unfavorably situated in very long lists.

THE BIOLOGICAL CONCEPTION OF LIBIDO

By K. C. Mukherji, Dacca University, India

The meaning of 'libido' is still open to controversy. The term was first used by Jung in the sense of primordial energy, but various interpretations of it have since been made. Freud has attached it to 'sex' energy. Alder defines it in terms of the 'will-to-power,' and Boris Sidis, adhering to Alder, interprets it in the sense of self-preservation. Bergson and McDougall have ascribed to it a biological significance and conceived it as identified with *élan vital* or vital energy. No one, however, has shown what it exactly means, how it is related to physical and neural energy, or wherein lies its specificity.

It is difficult to say in exact terms what energy is, but it is generally expressed in terms of work. The same kind of energy may play against different forces and perform different actions. So a mere difference in the work done can not justify the postulation of a difference in the essential characteristics of the energy involved. If it be accepted that physical energy, as expressed by Adrian,[1] is the only fundamental energy which is transferable into other forms—in accordance with the laws of thermodynamics—then it is obvious that what is neural or mental is nothing but the special manifestation of physical energy and that the conception of any other kind is quite unnecessary. But Adrian seems to forget that impulse studied in the isolated nerve may not be the same phenomenon as that occurring in the animal. Even if it were a fact—as it is not—that electrical manifestations are all that we know of neural energy, we should probably not be justified in saying that there is nothing but electrical energy involved in neural functions.

Adrian finds upon experimental investigation that every stimulation brings about a change in the surface layer of the fibres and a movement of ions into the field around it. This develops in the active region an electric current which is to be, according to him, identified with a nervous impulse. Of course, physical and chemical relations are undoubtedly present in the nervous impulse; but they may also be regarded as degradation of still higher forms of inherent organic energy unknown to us. Besides, Adrian seems to fail in taking the full view of the matter. He lays particular emphasis upon one aspect of the neuro-muscular activity; that is to say, the muscular contraction which is ungraded and susceptible of exhaustion. It is called ungraded because the muscle-fibre or the nerve-fibre has been experimentally found to react to the stimulus according to the 'all-or-none' principle; there is no gradation of reaction according to the strength of the stimulus, though it is true that individual fibres may differ in their inherent sensitivity. Adrian's investigation may cover only this aspect of the neural activity; but he has totally ignored its other aspect, *i.e.* the maintenance of tone which is caused not by the consumption of available materials manufactured by the cells to provide for the intermittent flashes of activity—as in the case of contraction—but by the coöperation in an intact central nervous system of afferent impulses from antagonistic muscles. This adaptive process seems to develop a certain molecular 'set' in the living substance, a temporary static attitude which is produced and maintained by a fairly uniform environment and which may be regarded as involved in the neural law of reciprocal inhibition and facilitation. Here the physico-chemical explanation meets a failure and proves its own limitation. Besides the teleological ordering, as demonstrated in the extraordinary delicate regulation of breathing, in the fine discrimination exhibited in the excretory activities of the kidney and elsewhere, is still a mystery unless it is referred to the present-day vitalistic hypothesis. It far exceeds anything in the realm of physics and chemistry. Here lies the distinctiveness of the neural energy involved in the biological activities which not only differ in appearance but are dependent on fundamentally different processes from those known to us in the realm of physical chemistry.

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[1] E. D. Adrian, The conception of nervous and mental energy, *Brit. J. Psychol.*, 14, 1923, 121-125.

But turning to the performance of mental energy we find that there are, analogous to the two varieties of neural activity, two varieties of mental action, one variety being explosive and intermittent and resulting from the need to make numerous judgments and decisions; the other adaptive and continuous, arising from the need to maintain a certain 'set' or mental attitude. Again, the peculiarity of mental function is said to lie in its purposiveness. But this element also is not entirely lacking in neural activity, taken simply on the physiological level. Qualitative selection of stimulus, modification of neural response by dispositions due to past events, and coördination of movement on special projection are marked even in purely reflex reactions. The two systems of energy, mental and neural, develop out of the same oösperm derived from the parental organisms; and in their development also they run parallel in complexity of function. The psycho-neural energy assumes a more mental form as higher and more plastic nervous areas are brought into function. Aphasia is a clear instance in this respect. Speaking of the mechanism of repression and displacement Freud says; "The mechanism of these processes is entirely unknown to me. Anyone who wishes to follow up these ideas must try to find the physical analogies and prepare the way for a demonstration of the process of motion in the neuron."[2] This indicates that the neural process is very intimately associated even with the subtle workings of the mind. Besides, psychophysical parallelism, the validity of which we do not care to discuss here, seems to stand upon the postulated identity of the two kinds of energy. So does the conception of a hierarchy of neural centres. McDougall, following in the footsteps of Leibnitz, conceives that "it may be that the soul that thinks in each of us is but the chief of a hierachy of similar beings, and that this one alone, owing to the favorable position it occupies (I do not mean spatial position), is able to actualise in any full measure its capacities for conscious activity; and it may be that, if the subordinate beings exercise in any degree their psychic capacities, the chief soul is able, by a direct or telepathic action, to utilize and in some measure control their activities."[3] The history of life is a striving towards individuality. What is a blind or purblind impulse in the humblest creature and what remains there forever below the conscious level assumes the form of conation and is expressed most clearly and richly in man's conscious nature. It means that the same psychergy develops and differentiates in complexity in course of biological evolution, reaching its climax in the human organism.

Taking all the facts together, it seems that the neural and psychic energy is produced as a consequence of the specialization which the biological energy (with which some would now consider 'mind' in the widest sense as co-extensive) undergoes in evolution. So the psychic and neural energies are but the two differentiated forms of the biological energy, differing so far from each other in refinement that each is governed by its own laws and works in its own way. Furthermore they are so inseparably connected with each other that they constitutionally form a functional unit which expresses itself clearly in the performance of pure instincts as viewed from the standpoint which McDougall has reasonably held. It is this inborn neuropsychic energy which is to be biologically conceived and conveniently used as 'libido.' All instincts are related to this libido and derive their functional energy from it. Now whether it is the common fund of energy for all instincts or whether it is more or less divided and its component quantities attached separately to different instincts, limited in their utilisation, is a question which is also variously answered. Freud has reduced the fundamental strivings of mankind to one, the 'sexual,' and

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[2] S. Freud, *Interpretation of dreams*, 1913, 475.

[3] W. McDougall, *Body and mind*, 1911, 366.

THE MECHANISM OF CONSCIOUSNESS: IMAGES[1]

By F. Russell Bichowsky, National Research Council

The two preceding papers of this series have discussed the activity of the two lowest levels of consciousness: the pre-sensation (thalamic) and the percept (Rolandic). Above these levels, according to the view advanced here, lie at least two higher levels, the image (lobular[2]) and the judgment (frontal). This paper will be confined to the relations of the 'image level' to the lower levels.

The existence of images as distinct from percepts is one of the most patent of introspective facts; at least I have found that even first attempts at introspection almost invariably distinguish the vivid, external, projected, impersonal, concrete percept from the subjective and meaningful image, often of a different modality. Nevertheless, more careful introspection shows that, considered as pure 'feels,' the image and percept are puzzlingly alike, the sharp distinctions found by the more naïve observers depending, not on intrinsic differences of quality of the two types but on consciousness of certain unlike relations in which they commonly stand. When those factors which depend on relations between content and reference are ignored (the usual rule for laboratory introspection), it becomes very difficult to find tangible differences between the image and the percept. It is, therefore, not surprising that previous observers, working largely under instructions which required only the report of content and which by implication often depreciated the report of conscious relations, should have expressed doubt as to the existence of any content other than 'sensations.'

Thus my *Os* even when most carefully distinguishing the image from the percept would often pause to remark that they are "essentially of the same kind of feeling stuff." "If their particularizing qualities are abstracted they have the feel of pre-sensations." "The visual percept and the visual image are both visual. I can't distinguish them on that score. They are like pre-sensations too in that." Both percept and images, moreover, are individuals. They have limits and space and time qualities. In this they differ from pre-sensations. The image was thus said to be "definite," "to be projected in space and time." "It was a picture," "a tone," "clear cut" and "distinguishable from other images." "As X would put it, it could be analyzed into definite 'sensations.' "

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[1] This is the third of a series of articles on the "Mechanism of Consciousness." The first, on 'Pre-Sensation,' appeared in the October 1925 number of the Journal (36, 1925, 588-596); the second, on the 'Percept Arc,' appeared in the July 1926 number (37, 1926, 382-390).

[2] This is the association-area of the physiologist. I have avoided this name so as not to confuse the anatomical and psychological categories.

In spite of these likenesses, however, and quite apart from many at least of the striking differences of relation often exhibited by images and percepts, my *O*s could distinguish images and percepts by the following apparently intrinsic differences.

(1) *Intensity and vividness.* Thus the image was said to be "less intense" and of "less vivid quality" than the percept. At first it might be thought that this difference of quality was due to the fact that the percept is a primary type of activity while the image is a more or less weak reproduction. But even where the image was generated in ways not involving the percept with which it was compared, my *O*s still maintained that the image "had an intrinsically less vivid feel than the percept."

This lack of vividness is apparently not solely a matter of the comparative strength of the underlying neural impulse, but is a qualitative distinction; for it is possible to find images which in a real sense of vividness are more vivid than ordinary percepts, sometimes even more vivid than the percepts generating them. Under these conditions my *O*s remark "that the enhanced image is really the more intense and bright but that there is a sort of qualitative brightness and vividness which the percept has and the image has not."

(2) *Subjectivity.* The image is subjective, mental, independent of external stimulation and of bodily position. The percept is external, real; its existence depends on external stimulation. Its character is independent of the position of the body. It is not private or personal.

In detail the image is said to be "unreal," to be an "internal thing," "not possessing the quality of concrete external reality," "subjective," "definitely located as a part of experience, not as a part of the external world," "it is not objective." More specifically, "the space of the image was not the same as that of the percept." "The image 'apple' was localized in space, but its space was not objective. It was independent of the position of my body, or of the movement of my eyes. It was an inner space." "The image was in its normal coloring. The percept had a uniform yellow cast." (The *O*, unknown to himself, was looking through yellow glass. The image had been stimulated by the way of audition. In case the image is stimulated by the appropriate percept it is usually toned to accord with the tone of the percept, but not always.) In some cases the orientation of space of the image was different from that of the percept; but again not always, particularly under normal conditions. The best tentative explanation of these anomalies seems to be that the space and time frame of the image is essentially independent of that of the percept; but that stimulation of the image by the percept tends to orient the space frame of the image to correspond with that of the percept. More study of this aspect of the problem is badly needed.

Even the time frame of the image differs from that of the percept level. My *O*s remark that though the image was in time it did not fill time. "Images have time sequence, some last longer than others; but there is no sense of absolute interval." "The image of the whole lecture which seemed to last the full period was present; but there was no accompanying perception of the lapse of more than a minute or so." "I can have the image of a whole epoch in a moment," one *O* reported, though he promptly corrected himself by saying, "I guess that's somewhat exaggerated. I have the sequence right enough. It seems a long period; but of course it doesn't really seem an epoch." Another *O* reported that "a real perceptual second lasts a whole second no matter how many images I have in it. The image scale of time is artificial and relative, the perceptual real and absolute." This is a report from an *O* who had just undergone a series of tests on time-perception where the apparent length of the perceptual interval had been changed by imaginal multiplication. On pointing this out the *O* immediately suggested that "it wasn't the number of images which stretched out the interval but the large number of incipient motor reactions stimulated by them which added to the apparent interval." As far as I can judge from my limited experiments this is the case.

(3) *Privacy.* "The image is private and personal." "It is my image—not a common experience which might be produced by anyone stimulated this way." "I don't mean that I can't have images without self-consciousness, but I don't remember being conscious of self without having images too."

It is perhaps unwise to go too much into detail on this aspect of the image, since it is hardly possible to do so without making more of an excursion into the field of consciousness of self than would be proper here. All that can be safely concluded is that image-consciousness is somewhat more closely connected with self-consciousness than is perception. A similar reservation must be made concerning the motor aspect of images which will be taken up next.

(4) *Voluntariness.* "The image seems especially connected with the will." "The image seems to lead to voluntary action and change, but it is not the willing agent." Nor is the image essentially motor in character, though some selected images may have that quality.

The typical image, at least the type commonly stimulated in acts of perception, is reported by all *Os* to be 'sensory,' 'passive.' "It does seem more free and indeterminate than the percept which seems forced upon me, therefore when the image leads to action it seems free, indeterminate, not automatic, action." "In fact," says this philosophically trained *O*, "most of the characteristic qualities of the image seem to be due to the freedom of the image as compared with the percept. The image is not objective because its stimulation does not necessarily depend directly on impulses from sense organs. It is private because often stimulated by processes not directly perceptual. It is universal because the same image can be stimulated by a large number of different percepts."

(5) *Universality.* "The image, though of a particular house, seems to feel different from the percept of that house, it seems to stand for houses in general, as the word 'house' stands for all possible houses." "Though I had the picture of a particular chair I wasn't thinking of a particular chair but of chairs in general." "The picture (image) is a peg on which I hang the idea.—No; I don't mean that I have ideas apart from images, but there is something more general about images than percepts." Many more descriptions of this important characteristic, which there is reason to believe is not really intrinsic but relational, could be quoted; but we shall have to recur to this aspect later.

It will be noted that the list of properties is very meagre; and, moreover, that there is considerable ground for doubting whether all or any of these qualities is really intrinsic in the sense that color is an intrinsic quality of some percepts. To the author these qualities are distinctly relational requiring two images; but it is believed that the list covers all qualities of the image that the critical *O*, taught to report only intrinsic qualities and to ignore relations, is able to report. He will probably report less. If *O* is asked to report relations between percept and images, however, (*e.g.* such introspective facts as that the percept leads to, or stimulates, the image) a much wider field for introspection is opened—that concerned with meanings.

It will be convenient first to deal with the relations discoverable by introspection between the image and the percept. Elaborate introspections are hardly necessary to illustrate the main fact in this relation; namely, that a percept often appears to lead to, to suggest, or to stimulate, a particular image. For some percepts the image is normally of its own mode. This will be called intramodal stimulation. For other percepts, in some *O*s at least, the image stimulated is normally of a different mode. This will be called intramodal stimulation. Whether the stimulation is intramodal or transmodal depends on factors which are not entirely known and which will not be discussed. Moreover, the exact mechanism of transmodal stimulation is not entirely clear. In my case the route of transmodal stimulation, so far as I can detect, is often direct. Thus the visual perception (of a cat) stimulates the auditory image 'cat' without an intermediate visual image. In some cases, however, where there is a delay between the visual perception and the auditory image, I do detect what appears to be a visual image, as if the path from visual percept to auditory image was through visual images. This indirect type of transmodal stimulation may be more common than I suppose; but it is not necessary to decide this point here as we shall assume for the purposes of this paper the possibility of both intramodal and extramodal connections from percept to image.

The most striking feature of the stimulation of image by percept is that it is always accompanied by the consciousness of meaning and of a particular type of meaning; namely, that the stimulated image means or stands for a given percept. Thus whenever a visual percept, a spatially localized patch of brown color, stimulates, leads to, or suggests the auditory image 'cow' I am conscious that I perceived a cow. If it does not stimulate such an image, the percept remains without meaning.

"The first effect on consciousness was a gray-blue 'feel' of neutral emotional tone. I was singularly at rest at the beginning of the experiment. There was no muscular shock or other kinaesthetic effect. Almost immediately I was aware of slow kinaesthesis of eye-movements plus perception of a light gray patch across the line of eye-movement. This patch caught the eye-movement and carried it away from me giving the full perception of a gray line lying in space. The gray square (the cover of the book) now developed as lying in space, the darker gray (of the table top) being in the background. There seems no emotional value attached, no special strain. As the front edge of the book caught the somewhat wandering focus there was a feeling of slight kinaesthetic shock, *e.g.* the feeling of surprise. I winked. There was slight strain in the throat as if to say 'book.' During this period of slight strain or transitional consciousness I seemed to feel the coming of a meaning, but the full consciousness that the gray figure was a book came only with the auditory vocal image (when I seemed to hear the word 'book' spoken). Before that the gray figure was meaningless; but it did have spatial configuration and was entirely definite."

This is perhaps an exceptional introspection in the simplicity of the processes displayed; but it illustrates first the vague pre-sensation feel from which the percept gradually arises in spatial and existential relation to the parent feel which remains throughout as a background. This percept with its pre-sensational background in a moment of transition, forcasted by pre-sensational strain, stimulates a new bit of experience; and this bit of experience, while not a meaning itself, is accompanied by consciousness of meaning at least when so stimulated.

In the case given the image stimulated is of a mode different from that of the stimulating percepts. It is somewhat more difficult to separate in introspective accounts the image from the percept when they are of the same modes, but there seems to be no essential difference in mechanism.

"It (the percept of an apple) had shape and size and color but no meaning. It was just a localized patch of color. Suddenly the strain pre-sensation broke. There was some kinaesthesis, but I've forgotten what and when I was aware again that it was an apple and had meaning. It was as if I saw that the particular percept was an apple; that is to say that it fitted into the picture which always carries with me the meaning of apple (a frame of green leaves, a bit of blue sky and a suggestion of pink blossoms, all less vivid than looking at an apple but still vivid). Into this picture or scheme the percept seemed to fall as if it belonged there. There were never two apples present. It was just as if seeing the apple brought the picture to me. The 'picture' apple seemed to go with the apple 'seen' but they weren't quite the same in feeling, though they were the same in many ways. It was yellow, for instance, though when I try to think of apples without trying to remember any special apple, I have the same picture in my mind but the apple is red. . . . I'm right about this? The apple I saw was really yellow, wasn't it?"

Other *O*s report the same general stages (a) percept, (b) strained transition, (c) fusion of the percept into a new form or the conscious generation of a new form which may be quite different from the percept and into which it does not fuse, but which always arises out of it.

To one of my *O*s a row of men including the particular man seen always appeared as an image when a particular man was recognized as being a man. In another case a figure was not recognized as that of a man unless it stimulated the image of a particular fearful Chinese gardener, an association which sometimes led to the most grotesque results.

In no case was there meaning without imagery of some sort. The following observation was made. An *O* whose dominant images were auditory had exhibited to him visually a chair, and at the same time the word 'chair' was clearly spoken to him. Various results were obtained. Occasionally *O* would be totally unaware of one stimulus, *i.e.* he did not perceive it; in other cases it was present as a pre-sensation. More commonly both stimuli were observed. Under such conditions *O* was aware of the customary mixed presentation, and then of the two separate percepts lying side by side without awareness that they were particularly related and without meaning. Then during a transition period, when the attention was fixed on the visual percept and the auditory percept was not noticed, "suddenly the word reappeared as if generated or connected both with the visual chair and with the sound and I was aware that both meant chair."

Though images are necessary for meaning, so also are percepts, at least where the thing meant is regarded as an external object.

It is possible also to arouse images by indirect means not involving the usual percept. The image 'chair' may be aroused by means of other images which merely 'suggest' chair. Or *O* may suddenly and spontaneously 'think about' a chair. Or the image may be forced by asking a question; *e.g.* "What piece of furniture has four legs and a back?" In all such cases the image 'chair' appears, and, except in the second rare case,[3] has a definite meaning in respect to the stimulating source but not the meaning that it would have if stimulated by a percept. Thus in the last case the meaning attached to the imaged chair "is a piece of furniture which has four legs and a back." It does not carry the meaning of an object. Moreover, though both image and percept are necessary that one may mean the other, the mere juxtaposition of the two is not sufficient to arouse this meaning.

Having stimulated the verbal image 'chair' by one of the means just described *O* was shown a chair which he promptly perceived. Though he clearly had the perception-chair and the image-chair, nevertheless the visual percept was without meaning and the image meant "what you sit on" and not the object he saw. It was only after a temporary fluctuation, in which the image faded out and reappeared, "as if stimulated by the percept-picture," that the image 'chair' meant the perceived object.

Numerous experiments have led to the same result. Though the mechanism (presently to be discussed) by which the transition from image to image was brought about was complicated and though in some cases the images stimulated by the percept in turn stimulated other images, thus complicating the observation, nevertheless there was no consciousness that a given image carried the meaning of a given percept when that percept "stimulated," "led to," "forced me to think of," "changed into," "grew into," or "started" the image.

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[3] Even in this case it is felt "to have a meaning, but I don't know what."

Now it will be allowed that the comments made are attempts of the *O*s to describe in non-subjective terms the relations which exist during consciousness of meaning, and also that the descriptions may equally well be applied to the physiological phenomena presumably concurrent, *i.e.* the stimulation of neural activity in the lobular area by way of the Rolandic area. When taken separately and all together, however, they suggest a fair definition of what meaning, regarded as content, is. In other words, such an analysis leads to the hypothesis that meaning is nothing more than consciousness of the percept 'A' and the image 'B' together with the conscious fact that one is stimulating the other. It is therefore the stimulation of image by percept that constitutes meaning, even where image and percept are such as not to suggest that one could have any connection with the other.

In one instance the image which carried the meaning 'cow' was a particular ship at sea. A given visual percept was never thought of as a cow unless it led to, or stimulated, this ship image. This ship image, moreover, could only be evoked by stimuli which to the normal person would suggest the 'idea' cow. Thus an attempt to use as a stimulus a picture of a ship which the *O* (a trained artist) had himself drawn as presenting his image failed to stimulate this image at all, stimulating instead the image of quite another ship, which to him conveyed the 'ship' meaning. (Investigation established the fact that there was a strong, though forgotten, association between the two ideas due to an infantile experience.)

More common, but hardly less striking, evidence that there is nothing inherent in percept or image that determines meaning is the case where a percept happens to stimulate not the normal image but some image quite inappropriate to it, as when the picture of a hat stimulates the image 'bat' or 'warship.' Such cases would ordinarily be called errors in perception; but they are in fact the normal analogues of paraphasia, for the perception though 'normal' happens to stimulate a wrong or unusual image. The importance of such cases for our purpose is that, though the image is often totally unlike anything *O* would ordinarily recognize as having a connection with the percept, it has meaning when thus stimulated just as in the more 'normal' instances, the erroneous reference having all the qualities of a meaning.

This concludes our direct introspective treatment of the meaning of percepts, which leads to an extraordinarily simple mechanism for the stimulation of an image-center by a percept-center, a mechanism which is similar in principle to that connecting pre-sensation and percept. Images may be thought of as the activity of a new conscious level, an activity stimulated by percept activity and partly controlling, in its turn, the activity of that lower level, exactly as activity of the percept-arc may be stimulated by activity in the next lower level (that of

THE EFFECT OF SUGGESTION ON THE JUDGMENT OF FACIAL EXPRESSION OF EMOTION

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Some time ago, Buzby[1] reported the results of an experiment with the Boring and Titchener [Piderit] model for demonstrating the facial expression of emotion.[2] He used only six of the faces analysed by Piderit—Anger, Dismayed, Horrified, Disdainful, Disgusted, and Bewildered. Judgments were obtained from 716 *S*s. No suggestion was given regarding the emotion portrayed. The *S*s were merely asked to choose the best descriptive term from a list of 18 among which were the 6 correct terms. The results show a wide scatter—in the case of Anger, for example, every one of the 18 descriptive terms was chosen at least once. Buzby's general conclusions, which seem to be clearly justified from his results, are: (1) the total facial expressions of Piderit for Anger and Dismayed do not give a high percentage of correct judgments; (2) those for Horrified, Disdainful, Disgusted, and Bewildered give a high percentage of correct judgments, especially when one combines other descriptive terms which closely resemble the expression represented." The largest number of 'correct' judgments obtained for any one of the figures was 63 percent—*i.e.* for Horrified. Four of the faces gave correct judgments in less than 40 percent of the cases.

The Boring and Titchener model of the Piderit faces has a much more valid demonstrational value than these results would indicate and so it was determined to repeat the Buzby experiment introducing two degrees of suggestion. The same six faces, employed by Buzby, were used throughout the present experiment. The experiment was divided into two parts, each part representing a different degree of suggestion.

In the first part, judgments were obtained from 995 students (men and women, graduate and undergraduate) in the courses in psychology at the University of Pennsylvania. The following instructions were given: "You will be shown successively six faces each of which represents the expression of an emotion. In each case, the correct name of the emotion it is supposed to represent will be given you. You will record (1) whether the face represents the emotion named; and (2) if it is such a representation, you will further record the degree of excellence, using the following terms only 'poor,' 'fair,' and 'good.'" These instructions represent the lower degree of suggestion. The results from this series will hereafter be spoken of as from the 'named' experiments.

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[1] D. E. Buzby, The interpretation of facial expression, this JOURNAL, 35, 1924, 602-604.

[2] E. G. Boring and E. B. Titchener, A model for the demonstration of facial expression, *ibid.*, 34, 1923, 471-485.

Four months later, the same faces were shown to 891 students (almost the same group who had made the judgments before) and with the same instructions. But this time, one of the authors, who has a certain amount of facility for doing this, first assumed the mimic and pantomimic expression of the emotion before the group, analysed the facial expression with regard to brow, eyes, nose and mouth and then, before the group and with verbal explanations, built up the model face to correspond to this analysis. This, we believe, represents the highest degree of suggestion possible under demonstrational conditions. The results from this series will hereafter be spoken of as from the 'analysed' experiments.

Results

The results for both procedures for the total percentage of correct judgments are given in Table I. The results of the present study are compared with those obtained by Buzby, which appear in the first column. In the second column will be found the percentage of correct judgments for the lesser degree of suggestion (named) without regard to the degree of excellence, and in the last column are similar results for the greater degree of suggestion (analysed). These results are shown graphically in Chart I. In this chart, the Buzby results are shown in shaded figures, the 'named' results in white, and the 'analysed' in black.

Table I

Percent Correct Judgments

EXPRESSION	BUZBY	NAMED	ANALYSED
Anger	2	36	56
Dismayed	6	80	86
Horrified	63	98	95
Disdainful	37	98	98
Disgusted	53	99	99
Bewildered	36	96	96

The percentage of correct judgments for Anger were very low for Buzby (only 2%). Under the suggestion of the name, these increase to 36%, and under the total suggestion of the analysis of the face, they rise to 56%. The results for Dismayed show a somewhat similar course—a marked rise in percentage correct from 6% (Buzby) to 80% when the name is suggested and a still further increase to 86% correct when the face is analysed. The results for the other four faces (Horrified, Disdainful, Disgusted, and Bewildered) are very similar—increasing for 'named' to 96-99% correct, and showing no further increase with the greater suggestion of analysing the face. There is one slight inversion for Horrified—a reduction from 98% correct for 'named' to 95% correct for 'analysed.' In the cases of Disdainful, Disgusted, and Bewildered, the percentages for the two degrees of suggestion are identical.

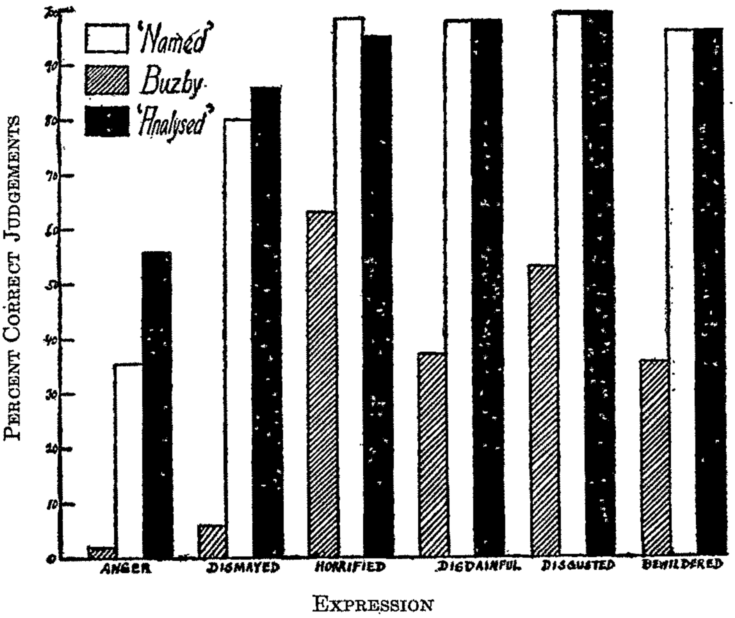

Chart I. Comparison of Buzby's results with those from the 'named' and 'analysed' experiments

The results for Anger and Dismayed seem to indicate that these faces of the Piderit model cannot be brought to a high enough percentage of correct judgments to be adequate for demonstrational purposes, no matter what degree of suggestion is used. For the other four cases, the judgments are so high that we may consider them unanimous choices—and this is equally true for the lower degree of suggestion as for the total analysis of the face.

The results with regard to the degree of excellence are given in Table II. Four columns are given for each of our experimental procedures—the first four contain the percentages of correct judgments for 'named,' and the second group of four

TABLE II

Percent of Judgments with Degree of Excellence

EXPRESSION	NAMED				ANALYSED			
	No	Poor	Fair	Good	No	Poor	Fair	Good
Anger	64	28	7	2	44	35	16	5
Dismayed	20	32	35	13	14	17	41	18
Horrified	2	7	21	69	5	12	23	60
Disdainful	2	7	27	64	2	7	28	64
Disgusted	1	6	22	70	1	7	25	68
Bewildered	4	11	40	45	4	9	38	49

columns for 'analysed.' The percentages for judgments of 'no,' 'poor,' 'fair,' and 'good' are given in each case. There is no means of comparing these results with those obtained by Buzby.

These results are similar, in many respects, to those already discussed for the total percentages of correct judgments. In the cases of Anger and Dismayed, the higher degree of suggestion not only increases the number of correct judgments but it also increases slightly the percentage of judgments with the higher degrees of excellence (fair and good). In the cases of the other four faces, the percentages for the different degrees of excellence show very little change. For all of these four faces more than 80% (and frequently 90%) of the judgments are rated either as 'fair' or 'good'.

A further analysis of these results with regard to sex and also with regard to the degree of psychological sophistication of the *S*s is shown in Table III. Buzby, in the former experiment, found that "women give a slightly greater percentage of correct judgments than men, and both men and women show a slight decrease in the percentage of correct judgments of facial ex-

TABLE III

Analysis of Judgments with Regard to Sex and Degree of Psychological Sophistication

Degree of Suggestion	Expression	NAÏVE								SOPHISTICATED							
		Men				Women				Men				Women			
		N	P	F	G	N	P	F	G	N	P	F	G	N	P	F	G
NAMED	Anger	62	31	5	2	74	22	4	0	47	37	13	3	66	20	12	3
	Dismayed	15	29	42	14	14	37	36	13	29	29	31	11	33	36	21	10
	Horrified	3	9	19	70	2	5	20	72	2	5	19	74	1	8	27	64
	Disdainful	2	10	25	63	2	2	30	66	2	7	28	63	2	3	30	65
	Disgusted	2	8	24	66	2	1	11	87	2	7	29	62	0	7	23	70
	Bewildered	2	12	41	44	3	7	43	48	7	13	39	41	7	10	34	49
ANALYSED	Anger	45	43	12	1	57	26	12	5	24	39	32	6	31	33	24	12
	Dismayed	9	24	48	20	21	30	36	14	12	38	32	19	14	23	41	21
	Horrified	4	12	25	60	11	12	23	54	4	12	20	64	3	12	21	66
	Disdainful	2	5	25	68	2	7	26	64	0	6	31	63	4	8	33	55
	Disgusted	2	8	31	59	1	3	14	82	1	6	35	58	0	6	23	71
	Bewildered	5	14	40	41	5	8	40	48	6	11	30	54	0	1	37	61

pression with increasing psychological sophistication." The first eight columns of the table—headed 'naïve'—contain the percentages for the men and women respectively in their first course in psychology. The last eight columns—headed 'sophisticated'—percentages of the students who have had at least one year of systematic psychology. A study of this table indicates that the distribution of percentages for both degrees of suggestion are very similar for both sexes and for both degrees of psychological knowledge. This would indicate that suggestion of a greater or lesser degree has the same effect upon either sex and upon the psychologically sophisticated as well as the psychologically naïve.

These results would seem to have a considerable bearing on the technique for the use of the Piderit model for demonstration purposes. It seems obvious that the emotion which each of the faces is supposed to represent should be named early in each demonstration; and that, unless this condition is complied with, the face will not be correctly recognized by a sufficiently large number of the group. Once the face has been named, the analysis of the face, which is the thing of psychological importance, will apparently not increase the number in the group who will recognize the total emotional expression.

These results seem to have a certain application to the psychology of pictorial advertizing. From a consideration of the present data and those of Buzby's experiment, it would seem that the printing of a picture—supposed to represent an emotional expression—will not be recognized by any large majority of the readers unless the emotion is named or otherwise indicated early in the 'copy.'

CONCLUSIONS

(1) Suggestion, such as the naming of the emotion and the analysis of the face, increases to a considerable degree the percentage of correct recognitions of the faces of the Pideriet model.

(2) In the cases of the faces for Anger and Dismay—which do not give a high percentage of recognitions when no suggestion is given—the amount of increase is progressive with increasing degrees of suggestion.

(3) The angry face does not seem, even under the conditions of the greatest degree of suggestion possible, to give a large enough percentage of correct recognitions, to make its use for demonstration purposes advisable. The results for Dismay, although much better than those for Anger, leave much to be desired.

(4) For those faces such as Horrified, Disdainful, Disgusted, and Bewildered, which give a relatively high percentage of correct recognitions when no suggestion is given, the suggestion of giving the name is sufficient to bring these to almost

SYNAESTHESIS: 'PRESSURY' COLD

By KARL M. DALLENBACH, Cornell University

Last semester during the routine experiments, in the qualitative laboratory, on the temperature spots, one of the students (Miss Alexandra Hobart) made the very interesting observation that "some of the cold spots are felt in my teeth;" that is, besides the primary cold sensations localized upon the forearm where the stimuli were applied, she experienced at times secondary sensations of indefinite character localized in and around the teeth and cheeks.

Questions regarding these experiences brought out that we were probably dealing with a type of synaesthesis as yet undescribed, *i.e.* 'pressury' cold;[1] we learned also, that *O*'s mother (Mrs. M. F. McClure Hobart), a student in this laboratory 27 years ago, had a complicated form of synaesthesis—studied and described in detail by Whipple.[2] Because we apparently had here a type of synaesthesis that was new, and one that might also cast some light upon the hereditary theory of synaesthesis, it seemed worth while to make a further study of this case.

I. DESCRIPTION OF EXPERIMENTS AND RESULTS

Experiments. The routine laboratory experiments for cold were repeated.[3]

Experiments were first performed upon the area that gave anomalous results—the radial side of the left forearm, 16.5 cm. above the carpal folds. When the casual observations of the class experiments were verified, the experiments were extended to an area located symmetrically upon the right forearm; to areas on the external parts of the right and left upper arms, 20 cm. from the elbow; and to areas on the right and left legs, 8 cm. above the patella. These parts of the body were shaved, so as to avoid the possible distractions of tickle from the hair, and a map, 2 cm. sq., bearing cross-section lines 2 mm. apart, was stamped on the skin. The areas were explored with temperature cylinders[4] cooled by cracked ice. The cylinders came to a point 1 mm. in diam. Stimulation was punctiform, and proceeded at the rate of one a second. Every square millimeter of the areas was worked over.

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[1] The literature on synaesthesis is very extensive. Cf. the reviews and bibliographies of F. Suarez de Mendoza, *L'Audition colorée*, 1890, 1-139; W. O. Krohn, Pseudo-chromaesthesia, or the association of color with words, letters and sounds, this JOURNAL, 5, 1893, 20-41; F. Wehofer, 'Farbenhören' (chromatische Phonopsien) bei Musik, *Zsch. f. angew. Psychol.*, 7, 1913, 1-54; and R. H. Wheeler, The synaesthesia of a blind subject, *Univ. of Oregon Pub.*, 1, 1920, No. 5, 1-61.

[2] G. M. Whipple, Two cases of synaesthesia, this JOURNAL, 11, 1900, 377-404.

[3] Miss Hobart's laboratory partner, Miss Harriet W. Leaf, acted as *E* in all the experiments reported here.

[4] For a description of the apparatus see E. B. Titchener, *Experimental Psychology, Student's Qualitative Manual*, 1900, 54.

O was blindfolded and instructed to give a complete report of her experience, but to pay particular attention to the experiences of cold and to the concomitant experiences in the teeth and cheeks. When either an 'ordinary' cold or a 'toothy' cold spot—for thus did *O* designate the spots that gave the concomitant experiences—was discovered its position was marked upon a duplicate map by dots and crosses respectively. When the 'toothy' spots were reported, *O* attempted, by pointing and by verbal description, to localize the concomitant experience. When an area had thus been worked over and mapped, *O* gave a description of the concomitant experiences. Twenty four hours later, before the map had faded from *O*'s arm, the area was worked over in the same manner and a second map obtained.

Results. The results of these experiments corroborate the casual observation of the laboratory exercises. *O*'s cold spots were of two kinds: (1) those that gave only the "bright, flashing, lancing" cold experiences with which we are familiar; and (2) those that yielded additional experiences, localized in the teeth and cheeks. The average number of these two kinds of spots per sq. cm. found in the areas investigated is shown in Table I. No great significance is to be attached to the absolute frequencies, for the procedure was rough, and both *O* and *E* were relatively inexperienced. The significance of the data, however, lies in the fact that 'toothy' spots were discovered in both mappings, in all six of the areas explored. The relative frequency of the two kinds of spots varied greatly from area to area; on an average, however, there was 1 'toothy' to 2 'ordinary' spots. A surprisingly large number of spots were re-obtained in the second mapping. The data in Table I show that of the total number discovered 69% of the 'ordinary' and 81% of the 'toothy' colds were verified.

TABLE I

Showing the number of 'cold' and 'toothy' spots obtained per sq. cm. from the right and left forearms, upper arms, and legs; and the number and percent verified at the second mapping

AREA	Obtained in first mapping		Verified in second mapping		Percent of verification	
	cold	toothy	cold	toothy	cold	toothy
left forearm	7½	3½	5½	2½	73	71
right forearm	4½	5	3	4¼	66	85
left upper arm	5½	1¼	4	1¼	72	100
right upper arm	2¾	½	1½	¼	54	50
left leg	4¼	½	3	¼	71	50
right leg	2¾	1½	1¾	1½	58	100
TOTAL	27¼	12¼	18¾	10	69	81

In the first series of experiments—those upon the left forearm—all the secondary experiences were localized to the left of the mesial plane. The experiments on the right arm and leg were undertaken to see whether this bilateral relation would persist. It did without a single exception; the concomitant experiences were always localized on the side of the body to which the stimulus was applied.

Within this limit, the secondary experiences were variously described and localized by *O*. At times the experiences were reported as 'cold' and localized in a specific tooth. Examples are:

"Cold in right upper front tooth" (right forearm stimulated);
"Cold sensation in tooth on right side lower row" (right upper arm).

For the most part, however, they were described as "pressury feels" and indefinitely localized in the teeth and cheeks, and occasionally in the eyes, ears, temples, head and neck. Examples are:

"That spot gave a reaction in the teeth. Began with a feeling in front tooth, spread to the left and disappeared" (left forearm);
"With that spot I experienced a flash in the cheek and neck" (left forearm);
"Vague sensation in left ear" (left upper arm);
"Pressury feel around left ear" (left leg);
"Pressury flash besides the left eye" (left leg);
"Pressury feel in right temple" (right leg);
"Ache in head much like that caused by eating ice cream" (right leg);
"Sensation in right eye ball" (right leg).

Movement in the secondary experiences was frequently reported. Examples are:

"Began with a feeling in front upper tooth, spread to the left and disappeared" (left forearm);
"The pressury experience begins most often in front teeth above the enamel line, that is it starts there and spreads out to one side or the other depending upon the side stimulated. It is not, however, always definitely localized" (introspection given at conclusion of the experiments).

The secondary experiences were usually concomitant with the primary. On a few occasions, however, a temporal disjunction was reported. Examples are:

"After a very cold spot, I had a sensation in the left front tooth" (left leg);
"Developed into tooth sensation. Seemed as though the stimulus had been held on the spot" (right forearm).

In order to determine whether cold was the necessary stimulus, an attempt was made to stimulate a few of the 'toothy' spots by pressure, but to no avail. Cold seemed to be the only adequate stimulus.

The examination of this case reveals that we have here phenomena that are synaesthetic in type. They are intermediate in complexity between colored hearing, colored tastes, odors, etc.—phenomena that involve the higher senses; and the 'thrills' and 'goose-flesh' occasioned by the hearing of the rasp of a file, the scrape of the finger nails, or the squeak of chalk upon the blackboard,[5] etc.—experiences which involve the lower senses and which are more or less common to all mankind.[6]

II. Discussion

The importance of this case lies not only in the fact that we have here a new type of synaesthesis, but also in the fact that it may throw some light upon the hereditary hypothesis first advanced by Cornaz[7] in 1848, and since then both denied and re-affirmed by many investigators. The evidence against the hereditary theory of synaesthesis and in favor of the associative theory lies chiefly in the fact that case histories and questionary results do not show that the trait has been transmitted from one generation to the next. But questionaries are notoriously inaccurate, and case histories are adequate only where the parents or children of the synaesthetic subjects are personally interviewed and examined. To ask a synaesthetic subject about the peculiarities of his parents' or children's experiences is the questionary over again, but at one step removed, with the chance of error and inaccuracy greatly increased.

We know the facts about our *O*. Though her mother has synaesthetic images with noises, tones, tastes, and smells, and also a few personifications, our *O* has, besides the pressury colds just discovered, no other form of synaesthesis. Had she been asked previously to this experiment if she had any synaesthetic images, she would have replied in the negative. If our *O*'s mother had been asked whether her daughter experienced any such phenomena her reply would likewise have been in the negative. A questionary or the ordinary case history would have failed to reveal the facts in this case.

An hereditary theory does not necessarily imply that the same kind of synaesthetic images should be experienced by parent and offspring. Nothing more is hypothesized than that the offspring—or some of them, for synaesthesis, like color blindness, may prove to be sex linked—inherit a peculiarity of nervous constitution—unusual anastomoses or abnormal crossing and uniting of afferent fibers—which will be evinced in some one or more of the many types of synaesthesis.[8] There is no *a priori* reason, however, why the peculiarity of nervous structure should, in different generations and different individuals, be equal in extent or identical in location. One generation may show one type of synaesthesis, the next another. Or, as in the present instance, the mother may have photisms with tones, noises, smells, and tastes, and the daughter 'pressury' colds; yet a common hereditary trait may be responsible for both.

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[5] The ice-cream pain variously localized in the head, though of extremely common experience, are not included here, for it is very doubtful whether they are of synaesthetic origin. Until more is known about them they must be regarded simply as referred pains (Cf. E. G. Boring, Sensations of the alimentary canal, this Journal, 26, 1915, 25, 35 ff.) originating in the stimulation of the esophageal and gastric branches of the vagus, or due to the faulty localizations of the sensations aroused in the esophagus.

[6] Whether they are common to all mankind is undetermined, since no reports, so far as I have been able to discover, have been made concerning the experiences of shrill sounds by aborigines.

[7] C. A. Cornaz, *Des abnormités congénitales de jeux et de leurs annexes,* 1848.

In giving the case history of *O*'s mother, Whipple reports that, "no trace of colored hearing can be found in the other members of the family."[9] Whether colored hearing specifically existed is unimportant, what is important is whether either of the parents had synaesthetic images of any kind. Furthermore, Whipple does not tell us whether he based his statement upon an examination of his subject's family. The presumption is that he did not, that he relied, as so many investigators before and since him have done, upon his subject's knowledge and report. This, particularly when it is negative, has but little value; one individual's knowledge of another's—even a parent's—mental peculiarities may be practically nil. A parent may have a pronounced case of synaesthesis; yet the children may know nothing of it. Even questions put to the individual concerned may fail to bring out the fact, either because the questions are too specific and do not cover the particular variety involved, or, as in the case of our *O*, because the individual himself may be totally unaware that he has synaesthetic images. It is only when the peculiar conditions necessary for their arousal are present that they may make themselves known.

The fact that synaesthetic images appear suddenly and are sometimes unnoticed until comparatively late in life, as in the case of our *O*, is brought in evidence against the hereditary theory. Whipple reports that *O*'s mother at the age of 16 suddenly experienced colored hearing, and remarks that "the hypothesis of heredity seems to be refuted by the fact that the first instance of the phenomenon was quite sudden and hallucinatory in character, and of late origin."[10] Suddenness and lateness of appearance is, however, no argument either for the associative theory or against the hereditary. They are merely facts that need explaining, and indeed when it comes to that they are more difficult to explain by the associative theory than by the hereditary. As a *post hoc* explanation, the associative theory may be stretched to explain the various phenomena experienced afterwards, but it is totally unable to account for the first appearance. Both of these points, however, may be accounted for upon the hereditary hypothesis by assuming that the inherited neurological condition was there, but that the essential objective conditions for the appearance of synaesthetic images were not previously present. This seems certainly to be the explanation of the suddenness and lateness of the appearance of the 'pressury' colds experienced by our *O*, for until she came to the laboratory experiments on cold the objective conditions had never been so favorable. It was only when the cold spots were stimulated separately and she was free from distractions, in the objective attitude, and set for a description of experience, that she suddenly became aware of the synaesthetic images.

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[8] Wheeler lists 34 varieties, *op. cit.*, 32 f.

[9] Whipple, *op. cit.*, 378.

[10] *Ibid.*

Now that these images have been focalized, they may of course be experienced under other and less favorable conditions; but this does not mean that the first sudden experience established an associative mechanism which is responsible for their later appearance. What factors, one might well ask, were responsible for the association in the first instance; and, in view of our results, why was the associative mechanism established only in the case of some of the cold spots—about one-third of them—and not for others.

Suddenness and lateness of occurrence, moreover, do not necessarily imply that the concomitant phenomena had never before been experienced. Doubtlessly, they had been previously experienced; but since they had no meaning and played no functional part in mental life, the subject ignored them, brushed them aside, without ever becoming fully aware of them. We do not for a moment believe that our *O* never before had 'pressury' colds, but only that never before had she become distinctly aware of the pressure component. The case is similar with the positive and negative after-images of vision and with many other mental phenomena that come to most students of psychology as 'new,' as something never before experienced. The lecture demonstration is not the first time that these phenomena have been aroused; it is merely the first time that they have been consciously present. A single demonstration is all that is needed to set the students observing their after-images under other conditions. Knowledge that such phenomena occur is all that is necessary. The case, we feel, is similar with synaesthesis. A particular subject may be unaware that he has synaesthetic images until conditions arise in which they are so clear, so intense, so insistent that he is forced to attend to them. They will of course be described as coming suddenly, and—if this collocation of conditions occurs comparatively late in life—as late. Once attended to, the synaesthetic images may recur in experience; but, as with the after-images, this merely shows the importance of 'knowledge about;' it is no argument against the hereditary hypothesis.

SUMMARY

The findings of this study may be summarized as follows:

(1) A type of synaesthesia—'pressury' cold—hitherto undescribed has been reported.

(2) *O*'s cold spots were of two kinds: the 'ordinary' spots that gave the usual experiences, and 'toothy' spots that gave also secondary experiences localized in and around the teeth and cheeks.

(3) The 'toothy' cold-spots occur on all the parts of the body experimented upon—the right and left forearms, upper arms, and legs—though with varying frequency.

(4) The secondary experiences aroused by stimulation of the 'toothy' spots were always localized upon the side of the body stimulated.

(5) A second mapping, made 24 hours after the first, verified 69% of the 'ordinary' colds and 81% of the 'toothy' colds.

(6) The secondary experiences were described as light, transient, and pressury in quality.

(7) The results of this case, in view of the fact that *O* has a synaesthetic inheritance, appear as evidence of the hereditary theory of synaesthesis.

ON THE NUMBER OF ARTICLES OF PSYCHOLOGICAL INTEREST PUBLISHED IN THE DIFFERENT LANGUAGES (1916-1925)

By Samuel W. Fernberger, University of Pennsylvania

Ten years ago, the author published an article,[1] under this same title, in which he attempted to demonstrate certain national trends in psychology by an analysis of the languages in which the titles were written and which were listed in the *Psychological Index*. The author recognizes that such an analysis is not to be construed as indicating the *value* of the contributions of the different countries. Nevertheless, it would seem to be an index of the professional interest in psychology taken by the different countries. If for no other reason, the study seemed justified by indicating the following conclusion: "Perhaps the most striking point brought out by this study is the extreme necessity for the student of psychology—no matter of what nationality he may be—to have a facile and critical reading knowledge of both German and English." It has seemed worth while to extend this study to a consideration of the psychological product of the last ten years. This period includes the end of the World War and it is of interest to discover if this conflict has had any effect upon psychology in the different countries.

An analysis of the data in the former article, covering a period of 22 years (1894-1915) showed that up to 1903 the English, French and German speaking countries were each publishing approximately the same number of articles each year. The year 1903 seems to be a critical one in which all of the curves are more or less affected. The curve for French titles declines steadily from then on to 1915. The curve for English titles has a tendency to rise steadily and slowly but irregularly. The curve for German titles rises very rapidly in 1903, with a sudden increase of more than 1,000 titles in the year and this high position is reasonably maintained until the war period. Then in 1914 and 1915 there are marked decreases in the German curve so that, in this latter year, the number of English titles is greater than the German. Throughout this period the number of Italian titles is much smaller than any of the other three languages and this curve maintains at approximately the same level. The number of titles published in languages other than German, English, French and Italian is so inconsiderable that they may be left out of consideration.

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[1] This Journal, 28, 1917, 141-150.

TABLE I

Number of Titles in the Different Languages

Date	English	German	French	Italian	All Others	Total
1916	1682	526	126	83	2	2419
1917	2153	88	233	117	67	2658
1918	1126	218	67	157	18	1585
1919	1464	331	273	252	21	2341
1920	1769	765	195	202	39	2970
1921	1544	432	293	141	20	2430
1922	1837	1701	341	170	28	4077
1923	2015	1011	326	270	33	3655
1924	1772	1459	290	335	45	3901
1925	2082	1214	380	506	28	4210

The results for the present ten-year period (1916-1925) are given in Table I. This table contains the actual number of titles published in English, German, French and Italian and recorded in the *Psychological Index* each year. Titles of all other languages (mostly Scandanavian) are grouped together in a fifth category. The total number of titles listed in the *Index* are also given.

These results are represented in curves in the accompanying figure. An analysis of these curves indicates that there have been more English titles published each year, during this ten-year period, than in any other language. During the American entry in the war (1918) the curve drops very considerably, but from then on the rise has been slow but steady, very similar to the rise in the curve for English titles before the war.

The curve for the number of German titles continues to drop markedly throughout the war period—the low mark being reached in 1917 with only 88 titles recorded. From then on the increase has been considerable. From 1922 on, this curve seems to have hit a dead level—not so high as in the pre-war period. The peak for 1922 is anomolous because in this year the *Index* included a number of back titles actually published during the war period. So the German curve actually should not be quite so low at the trough or quite so high at the peak.

The French curve was dropping markedly before the war period and this decrease in the number of titles was accentuated during the war. In 1919 there was a marked increase and a dead level has been maintained from then on to 1925. This level is considerably lower than that maintained by the French speaking peoples prior to 1910.

It has been considered worth while to include the Italian curve in the present study rather than to throw these results into the general category of 'all others.' The number of Italian titles was also decreased during the war period. Then a rather low dead level was maintained until 1921. Since that date there has been a steady and marked increasee in the number of Italian titles. This increase has been so great that in the last two years (1924 and 1925) there have been more Italian titles than French.

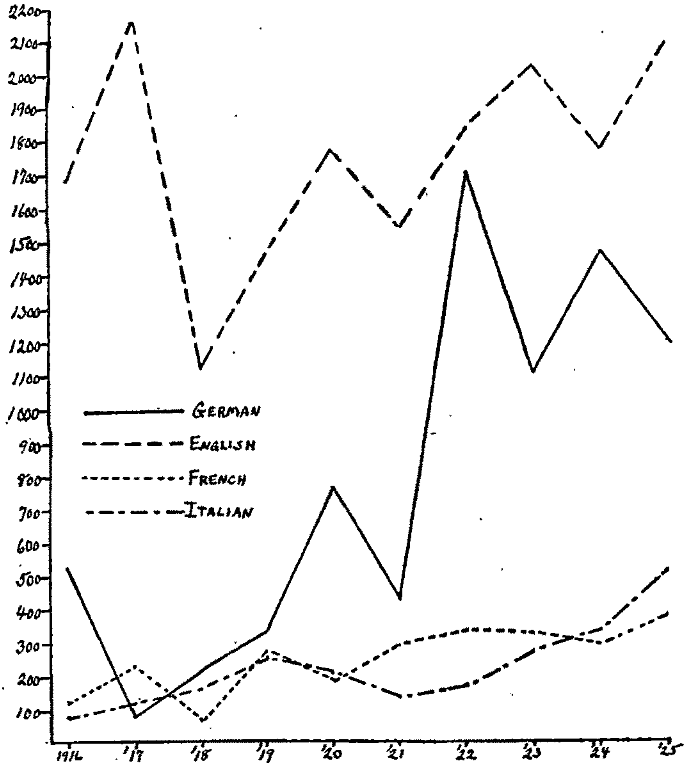

FIG. 1

Treating the Italian results separately leaves such a small number of titles in other languages (mostly Scandanavian) that it has not seemed worth while to include the curve for these. In a figure of this size, such a curve would be very close to the abcissa and would be practically parallel to it with only small fluctuations.

These data have also been thrown into the form of percentages of the titles published in the different languages. These percentages are given in Table II. The percentages of

TABLE II
Percentage of Titles in the Different Languages

Date	English	German	French	Italian	All Others
1916	69.5	21.8	5.0	3.4	0.3
1917	81.0	3.3	8.8	4.4	2.5
1918	71.0	13.7	4.2	9.9	1.2
1919	62.5	14.1	11.7	10.8	0.9
1920	59.6	25.8	6.6	6.8	1.2
1921	63.6	17.7	12.1	5.8	0.8
1922	45.3	41.7	8.7	4.2	0.7
1923	55.2	27.7	8.9	7.2	0.9
1924	45.4	37.4	7.4	8.6	1.2
1925	49.5	28.9	9.0	12.0	0.7

titles in languages other than English, German, French and Italian are so small that they may be disregarded. Only four times do either the French or Italian percentages exceed 10 percent. (French 1919 and 1921; Italian 1919 and 1925). When one combines the English and German percentages the range is from 76.6 percent. in 1919 (the high years for French and Italian) to 91.3 percent. in 1916. In eight of the ten years covered, the combined percentages of English and German titles are more than 80 percent. of the total.

From these data it would seem that the research psychologist of today should be something of a linguist. A reading knowledge of English and German is essential if he is to read the literature in any field. If he is to adequately cover *all* of the field of psychology—and who has the time to do it!—he should have a reading knowledge of French and Italian as well. If he is working in any special field, he must still read English and German. For some fields he is now better off with an additional knowledge of French—in others, with an additional knowledge of Italian. All four languages are desirable, however.

I cannot do better than to restate, as a general conclusion, what I said in the paper ten years ago. The present data, if anything, increase the validity of that final statement. "Perhaps the most striking point brought out by this study is the extreme necessity for the student of psychology—no matter of what nationality he may be—to have a facile and critical reading knowledge of both German and English."

DEVELOPMENT OF LABORATORY EQUIPMENT IN PSYCHOLOGY IN THE UNITED STATES

By C. A. Ruckmick, University of Iowa

With recent interest in the applications of psychology in a great variety of contiguous fields the question has frequently arisen, how does 'pure' psychology fare today in the amount of emphasis placed upon it among psychologists at large; how has it prospered in the progress of its natural growth? We might attempt to answer the question on the basis of several lines of investigation, but the most fruitful hope of ascertaining a natural growth seems to lie in obtaining information concerning the steady development of laboratory equipment in this country. Obviously apparatus does not make a science but in the same sense that the material aspects of a community reflect its prosperity, so the continued demand for apparatus is a fair indicator of the health and vigor of the experimental method.

For a time at least superficial evidence, like the amount of space devoted to advertisement of new 'test-forms' and the amount of published investigations based upon mental tests, gave many of us the impression that paper test materials were supplanting the usual technical apparatus in our laboratories. A detailed study reveals, however, that such is not the case.

While much emphasis is placed upon applied psychology we find, however, that the situation in pure psychology is about the same as it is in physics. The great development of engineering in its several branches, exploited as it is in many of the technical and popular journals has not retarded the quiet growth of pure physics. On the contrary it has stimulated additional research along lines of greatest pressure in the world of application. Something similar seems to be happening just now in psychology where the deficiency of the mental test program on the side of interest, 'drive,' and moral ideals is one of the dynamic forces toward a better understanding of the emotions and of the motivated self from the point of view of pure psychology.

With this incentive a study of the early history of laboratory equipment in this country was undertaken and a survey was made of its present status expressed in financial value, both on the side of a complete inventory and of an annual budget. In a sense this supplements a study made over a decade ago.[1]

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[1] C. A. Ruckmich, The history and status of psychology in the United States, this Journal, 23, 1912, 517-531.

If we go back to about 1890, seven years after the establishment of the first psychological laboratory at Johns Hopkins University, we find many of the laboratories in this country in their infancy. The manufacturers of apparatus began making psychological materials about 1892 when a Philadelphia firm, Elmer Willyoung & Company, made up apparatus according to designs furnished by Scripture of Yale. A year afterwards the Garden City Model Company of Chicago produced instruments from suggestions made by Jastrow of Wisconsin. Other manufacturers of that day include J. D. Brown, of Camden, New Jersey, designing apparatus for Cattell and Witmer, and A. Pfeifer of Baltimore who coöperated with G. Stanley Hall. These arrangements seem to be patterned after the European custom where we find the Cambridge Scientific Instrument Company making anthropometrical apparatus for Galton; Diederichs of Göttingen coöperating with Schumann; Krille of Leipzig, with Wundt; and Elbs of Freiburg, with Münsterberg. Besides these, Baltzar of Leipzig, Koenig of Paris, Kohl of Chemnitz, and Peyer, Fararger & Company of Neuchâtel, were designing special types of apparatus like kymographs, tuning-forks, tonometers and Hipp chronoscopes.

At this time the isolated makers in this country fared very poorly and the importations from Europe increased in volume. Presently, about 1899, succeeding the Chicago Laboratory Supply and Scale Company, which in turn had become successor to the W. A. Olmsted Scientific Company, Stoelting[2] of Chicago began to devote more and more of his time and resources to the manufacturing of psychological apparatus. He had the support of some of the more prominent psychologists of that decade, like Jastrow, Sanford, Scripture, Seashore, and Titchener, but psychological apparatus even then did not give profitable returns until the publication of Whipple's *Manual of Mental and Physical Tests*, which created a demand for many distinct pieces of apparatus in addition to test material. With the increased interest in psychology during and after the war, the firm confined its efforts to psychological apparatus, and is now selling material valued at about $500,000 per year. This covers, of course, not only a domestic demand, but exportation to foreign countries, especially to the Orient. The Stoelting Co. has equipped the psychological laboratories of institutions in Australia, Canada, China, England, India, Japan, Panama, Peru, Portugal, New Zealand and South Africa. Orders for apparatus aggregating over thirty-five hundred dollars have recently been filled for the Universities of Peking and Mexico, Maharaja College, at Mysore, India, and Brahmavidvashrama of Adyar, Madras, South India. This firm is now the most important producer of psychological apparatus in this country and the only one specializing in the field on an extensive basis in the country. Of course, this does not convey a complete picture of the development of psychological apparatus throughout the world because large quantities of instruments are being exported from Germany to many nations everywhere.[3]

But our task is confined to the development of laboratory apparatus in this country. Our American laboratories began their consistent growth about 1890. This is not of course the date of their actual beginning which was several years earlier and is indeed somewhat in dispute;[4] but it does mark the start of a continuous and generally wide-spread development of laboratories in our colleges and universities.

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[2] I wish here to express my appreciation to the Stoelting Co. for furnishing me with most of the information contained in this paragraph.

[3] M. Bentley, Experimental psychology in the Orient, this JOURNAL, 37, 1926, 154.

[4] C. A. Ruckmich, *op. cit.*, 519 ff.

The estimated total value of psychological materials in this country in 1893 was only $30,000.[5] About 10 years later Miner estimated that 25 of the largest colleges in the country averaged about $4,000 in total equipment, equivalent to a sum of over $100,000 invested in psychological apparatus. The most expensively equipped at that time were Clark, Columbia, Cornell, Harvard, Pennsylvania, and Yale with a valuation of about $10,000.[6] The earliest accounts of laboratory equipment on which any financial estimate can be based are those of Harvard and Cornell. In 1893, Harvard issued an inventory of the laboratory as a part of the exhibit at the World's Fair. Münsterberg says of it at the time, "the outfit is even now the most nearly complete that is anywhere at the disposal of students in psychology,"[7] and adds as a further reason for publishing the account that "experimental psychology is too often confused with experiments upon the brain by vivisection, with hypnotism, and even with spiritism. . . . This account of our experiments may also serve to correct the error which is so prevalent that experimental psychology is the study of sensations and simple reaction times." He then proceeds to list the studies that are being undertaken under the headings of association, attention, memory, judgment, space, time, the feelings, and the will. In the catalogue we find a total of 240 pieces valued at a little over $4,000. A glance at the table summarized from this catalogue will show the distribution (Chart 1). In 1900 the Cornell laboratories published a similar inventory[8] (Chart 2). An outline of the laboratory and its requirements was given by Titchener two years before. There are 375 items listed at a value approximately equal to $6500 when foreign values are converted into American denominations. About this time Titchener and Cattell[9] dis-

CHART 1

HARVARD PSYCHOLOGICAL LABORATORY—1893

(Nos. in parentheses refer to number of items listed)

I. Objects for anatomical and physiological demonstration		$ 682.10
A. Brain		
a. Human (21)	$ 383.50	
b. Vertebrate (5)	50.00	
B. Sense-organs and nerves		
a. Anatomical (11)	183.60	
b. Physiological (5)	65.00	
II. For study of sensation		$1494.00
A. Hearing (24)	$ 780.00	
B. Sight (36)	578.50	
C. Dermal and muscular Ss. (11)	135.50	
III. For higher mental processes		$1696.20
A. Time measurement (33)	$1109.20	
B. Perception, space and time (25)	261.00	
C. Association, attention, emotion (20)	326.00	
IV. Technical outfit		$ 703.05
A. Optical (14)	$ 315.00	
B. Electrical (15)	227.30	
C. Surgical (20)	160.75	
TOTAL (240)		$4249.35

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[5] W. O. Krohn, Facilities in experimental psychology in the colleges of the United States, *Report U. S. Commissioner of Educ.*, 1890-91, 1139.

[6] J. B. Miner, The changing attitude of American universities toward psychology, *Science*, 20, 1904, 303.

[7] H. Münsterberg, *Psychological laboratory of Harvard University*, 1893.

[8] E. B. Titchener, The equipment of a psychological laboratory, this JOURNAL, 11, 1900, 251-265.

[9] J. McK. Cattell, The psychological laboratory, *Psychol. Rev.*, 5, 1898, 655-658.

CHART 2
CORNELL PSYCHOLOGICAL LABORATORY—1900
(Nos. in parentheses refer to number of items listed.)

A. Physiological and psychological acoustics (67)		$1979.75
I. Anatomy and physiology of ear	$ 67.50	
II. Auditory sensation: intensity	157.75	
III. Auditory sensation: quality and clang tint	1549.95	
IV. Auditory perception, memory, etc	204.55	
V. Charts		
B. Physiological and psychological optics (119)		$1386.90
I. Anatomy and physiology of the eye	$ 37.50	
II. Visual sensation: intensity	106.70	
III. Visual sensation: quality	307.40	
IV. Visual sensation: contrast, etc	138.20	
V. Visual perception	219.75	
VI. Visual association, memory	78.00	
VII. General optical apparatus	499.35	
C. Haptics and Organic Sensation (50)		$ 469.45
I. Haptical, etc. sensations: intensity	$ 74.50	
II. Haptical, etc. sensations: quality	193.45	
III. Haptical, etc. perception	201.50	
D. Taste and smell (17)		$ 105.00
I. Sensations: intensity and quality	$ 104.00	
II. Anatomy and physiology: charts	1.00	
E. Affective processes (36)		$1234.70
I. Special instruments	$ 277.00	
II. Models	14.85	
III. General recording instruments	942.85	
F. Action (18)		$ 624.00
G. Attention (5)		65.50
H. General supplies (63)		626.55
I. Electrical outfit	$ 275.80	
II. Mechanical and chemical	194.00	
III. Physiological and anatomical	108.50	
IV. Diagrams	48.25	
TOTAL (375 items)		$6491.85

cussed in published articles the requirements of a psychology laboratory and specified sums which today at our increased scale of prices seem somewhat fantastic. Cattell considered the Cornell laboratory particularly fortunate in receiving a grant of $2000 for its establishment in 1891 with an annual support of $600, but criticised Titchener's "estimate of $300 for the current expenses" as "rather extravagant," to which Titchener replied[10] "all that my paper says is that it is unsafe to begin the working year with a less sum than $300 in hand." Concurrently articles appeared questioning the use of what was then known as the "new psychology," or the "experimental method" in the teaching of elementary psychology, and the necessity of laboratory equipment was seriously doubted, curiously enough, at an institution where now many researches are annually turned out.

Let us now glance at the situation some twenty-five years later. From the modest equipment of a $2000 laboratory with an annual budget of $600, we turn to a maximum laboratory evaluation of $32,000 with an annual budget as high as $3500. Of course, it may be said that our universities have grown in the meantime, but our charts show no intimate relationship between student enrollment and financial support for the psychological laboratory (Charts 3, 4). An institution which has a little over 5000 students comes to the top of the list as far as pure psychology is concerned. Whereas, an institution with the largest enrollment in our group has but one-third of this amount as the present estimated value of its laboratory. The chart (Chart 5) shows roughly the distribution of the values for total laboratory equipment as estimated by responsible individuals at each institution. The total equipment for all of the 32 institutions is about $300,000. The total annual budget of these institutions for apparatus is $35,000.

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[10] E. B. Titchener, A psychological laboratory, *Mind*, 7, 1898, 311-331.

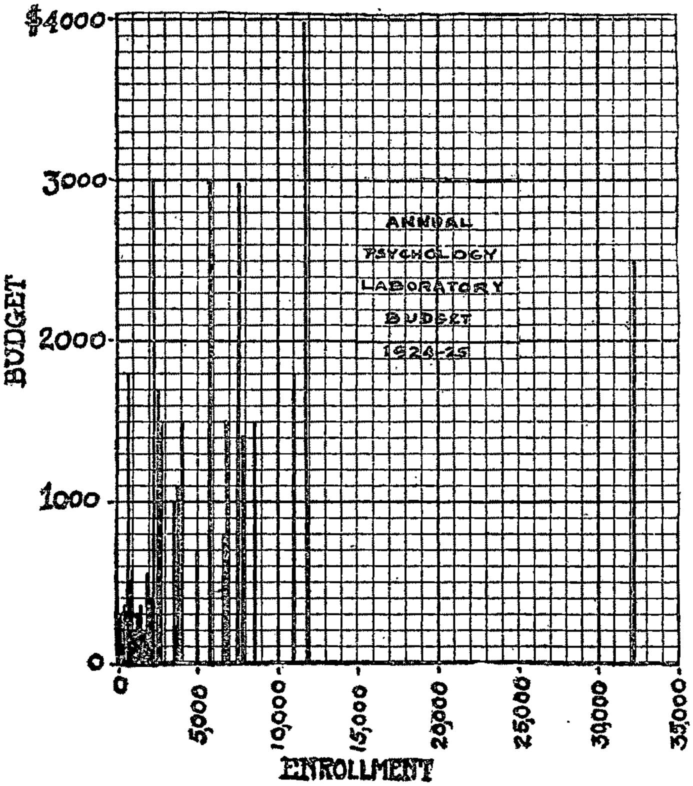

Chart 3

The questionary which was sent out covered the following items: (1a) the present total evaluation of laboratory apparatus; (1b) of test material; (2) present annual budget; (3) any special funds like endowments, received for these purposes. Information was also requested concerning the employment of a mechanician, and the extent to which he constructed new apparatus. Out of 60 requests to fill out blanks sent to the most representative institutions of the country, 32 replies were received. Since some of the larger institutions either failed to reply or explained the reasons for not making an estimate, the actual returns could probably be duplicated for the laboratories not represented in the report. In addition, there must be altogether a large amount of material in our normal schools, smaller colleges and universities to which no request was sent. Therefore, on this basis a conservative guess would be to quadruple the amount represented in the report, so that probably our psychological laboratories over the country contain apparatus worth in the neighborhood of a million dollars.

The average annual budget is in the neighborhood of one thousand dollars, ranging from $50 to $3500. This is about twice the average amount appropriated according to a study made of 21 representative institutions in 1911.[11] The latter sum included other items, however, like books and charts, which were not included in the present account, and is therefore not strictly comparable with our present figure. But it is likely that the ratio would be still further increased if the library expenses were deducted from the budgets of 1911.

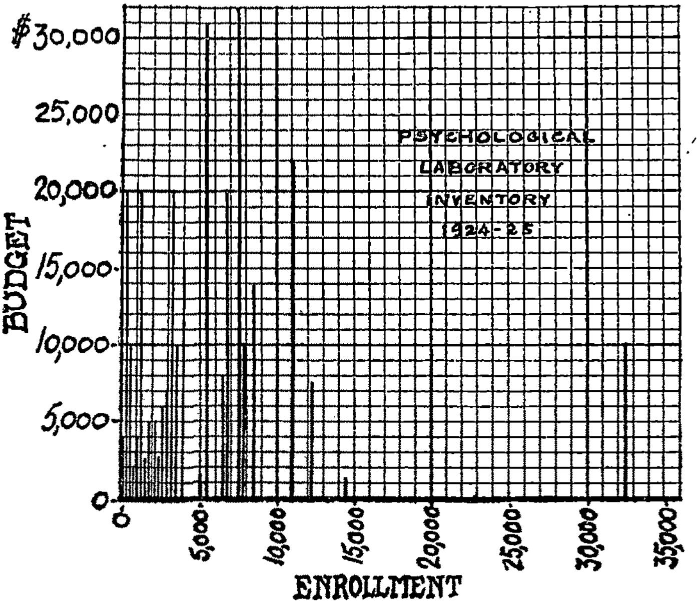

CHART 4

The report is probably weakest in its attempt to compare the support given to applied psychology in the form of test material as compared with the parallel branch of pure psychology. The reason for this lies in the great variety of affiliations that exist between these two main branches of psychology as represented in departmental and inter-departmental relationships at various academic institutions. In many cases the individual who replied to the questionary could not at all speak for the work in applied psychology, but there is nevertheless some significance in the distribution of funds, as specified in the columns of our table, which reveals a remarkably definite emphasis on pure psychology.

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[11] C. A. Ruckmich, *op. cit.*, 528.

CHART 5

INSTITUTIONS (32)	INVENTORY		ANNUAL BUDGET		SPECIAL FUNDS	% DISTRIBUTION		MECHANICIAN
Enrollment	Apparatus	Test materials	Apparatus	Test materials		Pure	Applied	
5502	$ 10195.23 u 21518.95 g		$ 3000			100	0	Spec.
7435	30000	$2000	2000	$1000	$ 0	66	33	$2100
11083	22000	15	1800	0	0	100	0	450
3581	20000 g	200 g	?	1000?	0	95	5	?
234	20000 g	500 g	1700 g	100	195000			?
6812	20000 ?	?	1560	?	200 ?	100	0	1440
1599	20000 g	250 g	?	?	0	?	?	800
3921	15000	300	1500	?	9000	?	?	2000
8583	5000 u 8200 g	?	1500	?	1000	100	?	1800
32769	10000	?	2500		5400			500
7423	10004.55	?	1400		2500	100	?	800
3873	10000	250	1100	—	0	90	10	0
772	10000 u	300 u	500	?	0	?	?	?
6582	8000	?	800	?	300	?	?	?
2656	6000 u 1000 g	100 u 100 g	1500	?	1000	100	?	?
11810	2500 u 5000 g	200 u 500 g	3000	500 u 600 g	1000 g	?	?	400
2125	5000	200	2500	500	0	84	16	0
1773	5000 u	?	200 u	300 u	0	?	?	?
2448	5000 g	1000 g	1750 g	?	140000	100?		1200
1543	3869.11	50.25	300	—	1000	88	12	Spec.
560	3000	250	300	0	75	100	0	100
2424	2500	100	?	?	1000	50	50	
1500	2200	150	100	—		75	25	
457	2000	100	225	100	0	33	66	0
1100	1800 u	200 u	300	50	0	75	25	0
1777	1500	0	50	0	0	100	0	0
5000	1500 u	200 u	500	150	0	95	5	0
825	1500	60	150	250	250	66	33	0
1965	900 u 300 g	100 u 100 g	200	200	500	66	33	?
14461	800	250			0	100	0	
560	750 u	25 u		87				
465	100 u	10 u						
TOTALS	$292137.84	$7510.25	$30435	$4837	$358225	85.5%	14.5%	$11590
Graduate	101018.95	2650						
Undergrad.	45241.23	1135						

Legend: u = undergraduate
g = graduate
? = uncertain, or found elsewhere
— = spread over both columns

In tabulating the results of the inquiry there were several difficulties which ought also to be mentioned. Many departments report that there is no standing budget, that the amount of money is conditioned by circumstances which vary from year to year, and that the amount allowed for any one year might be, for that reason, more or less than that allowed in previous years. Several institutions hope for better things in years to come and several others report that the budget is elastic and that they can obtain as much apparatus as is needed from time to time, both for undergraduate study and for graduate. Three colleges report that they have, properly speaking, no laboratory and no laboratory course, but one of these expects to obtain a well-equipped laboratory in the next two years. Another difficulty presented was the fact that in about six institutions no systematic survey had been kept for years so that the figures were, in many cases, more or less well grounded estimates. It is a pity that our academic institutions of learning do not follow the rigorous methods of reputable business concerns in keeping systematic inventories. Another ambiguity was the term 'apparatus.' The questionary asked for an evaluation "of psychological apparatus, models, charts, etc., in the laboratory, (exclusive of printed test material but inclusive of form boards, puzzle boxes, pictures, etc.)." It did not occur to the writer what status research animals would have in this connection but one director properly said that "animals were listed as apparatus." In general, too, it was difficult to define general laboratory equipment so one respondent commented: "Expenses and equipment are distributed among items which are separated in questionary and which vary with circumstances." An interesting feature was the amount of money available from student fees, especially in some institutions where the laboratory course is taken by almost 200 students. In some instances these fees go to make up the pay of the mechanician which brings us to another important consideration.

Chart 5 indicates, in general, the amount of money available for a mechanician but it does not show the variety of arrangements made for this important service. The laboratory budget may allow for any sort of arrangement from no mechanician at all to one whose salary approximates $2,000 for the academic year. One institution reports that such a mechanician produces apparatus valued at $5,000 for the year. Another reports that there is no mechanician available but one is greatly needed and that it is hoped to have one in another year. In a few institutions the person who functions in this capacity is not a technical mechanician but a general utility man, which situation reminds one of the practice in many European laboratories where the mechanician is at once a skilled expert in apparatus and a general secretary, doing clerical work for the department. Many laboratories have made arrangements with other departments which allow the services of a skilled individual, either as a flexible unit among all the scientific laboratories as in many of our smaller colleges, or as a prorated unit devoting anywhere from one-half to one-third of his weekly time to the demands of the psychological laboratory.

The following are a few comments of special significance:

"Year laboratory course 1 period (3 hr.) a week with registration ranging usually from 150-180."

"Experimental psychology here is at present chiefly incidental to the other psychology courses and clinic work. We are planning to make it an elective in science and to house the laboratory in quarters in a new science building."

"Practically all of our elementary students go through a 2-credit hour laboratory course."

"We have no apparatus exclusively for undergraduate work. All I can say about the annual budget is that we are allowed all the apparatus we need."

"I suppose Thorndike tests are given largely in the interest of applied science. Our department uses the records in many investigations that have no place, as yet, in applied psychology. Material used in demonstration in classes (paper tests) can not reasonably be put in either group."

Another interesting feature of the investigation was the encouraging amount of money which is available for special research under grants from the Rockefeller memorial funds and the Rockefeller Foundation or other private endowments, and through the offices of the National Research Council.

Of course, as in every classification so in the presentation of the net results of a questionary, a certain amount of damage is done to the individual items that appear. Gross distortion probably does not occur but almost every item has its own conditioning factors and its own complexion derived from local environments that are not fully described. It must also be noted that much of the material for pure psychology is extremely elaborate and expensive and that it is becoming more so with every refinement of method, as the writer well knows in connection with the direction that work in acoustics is taking, involving the refined circuits of the audion tube. One must not overlook the fact, however, that in applied psychology many of the better researches also involve expensive apparatus which is often borrowed from pure psychology. Then, too, the use of printed forms, if needed in great quantities, is not an inexpensive procedure when the more expensive calculators and the amount of clerical help are taken into account. It is also probably true that the demands of men equipped to apply psychology in the industrial world are becoming so great that their salaries are running far above the rates established for professorial work in pure psychology in our academic institutions. Our statistical experts in the colleges and universities of the country are high salaried men. Of course the problem is familiar to the heads of engineering schools, to the directors of chemical and physical laboratories, and to the chiefs of our geological departments.

In summary, it is worth noticing that psychology is apparently making definite progress in the establishment of its laboratories and it is safe to lay down the principle that the total inventory of apparatus in one of our larger universities ought to be, under average conditions, about \$20,000 to \$30,000, and that its annual budget should be about \$1500. It has been stated by Cattell in the early report referred to, that the trustees of our academic institutions are not in the habit of reading the psychological journals, but let us hope that the directors of our laboratories do. While some instances of too great an outlay have occurred it is probably true, according to the observation of at least one person who has travelled widely in this country investigating laboratory conditions, that the directors of our laboratories do not have the courage to ask enough for the maintenance of their equipment.[12] Chemistry and physics are so well established that large sums are easily obtained for their maintenance. Psychology has also come into its own. It is no longer referred to as the 'new psychology,' nor is the use of the experimental method questioned in connection with our introductory courses. At Iowa, for a decade or more elementary exercises have been required of the students in the introductory course in much the same way as laboratory work is offered in conjunction with elementary courses in chemistry, physics, geology, and biology. For that reason, those responsible for obtaining annual budgets should get into the habit not only of asking for more money proportionately, but should contrive ways and means of putting new pieces of apparatus actually into use either in the drill laboratory or in research work.

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[12] It is encouraging to note that the writer of this paper has already received one inquiry concerning the equipment of a new psychological laboratory building, in connection with which it was said that the statistics presented in the reading of the paper, at the annual meeting of the American Psychological Association, would be helpful in obtaining funds for the equipment of the laboratory.

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THE ELEMENTARY COURSES IN PSYCHOLOGY

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The writer hopes that this paper will not be taken as a criticism, but rather in the spirit in which it is intended and written, as a suggestion towards making the elementary course in psychology more significant. When possibilities of accomplishment are great but actual results are but meager, if not even mean, it is worth while to point out the contrasts and suggest the causes. The writer believes this condition to exist in the teaching of elementary psychology today, namely, that the course falls short of what it might accomplish, due to the general effect of one or more of the following defects:

(1) Beginning students of psychology are usually plunged into the sea of psychological data at random immediately on entering the course, and allowed to sink or swim in unknown waters as best they can. The result usually is that some students emerge from the experience fairly well drenched with isolated and, therefore, meaningless facts about the nervous system, perceptions, instincts, emotions, sensations, etc., some of which they retain more or less accurately, while the large majority of them harbor a pleasant or unpleasant memory of "having had psychology."

(2) The course is often treated as if it were but a preparation for more advanced work in psychology, with the result that certain topics and technical details are introduced that are of interest only to the specialist and confusing to the amateur. While it is true that the elementary course gives opportunity for recruiting disciples for advanced study, and no doubt succeeds in so doing, it is also true that by far the larger number do not go beyond the introductory course, and, therefore, detailed treatment of certain topics leads to mental indigestion, since there is not sufficient time to establish a relationship between the minute part and the whole from which it receives its significance. The result is, as a whole, that the student can not see the forest for the trees, and leaves many a topic in the course in a state of mental chaos. The two particular topics that suffer most from this fault are the nervous system and the sense organs. The *function* of the nervous system and of the sense organs, which is of paramount importance so far as psychology is concerned, is practically lost sight of in the maze of detailed structural analysis, which belongs strictly to anatomy and physiol-

ogy rather than to psychology. The suggestion is not that nerve and sense-organ structure should be omitted, but rather that only those items should receive attention, and only to such extent as to make the function clear and distinct. So long as psychology, as such, was short of subject matter, there was justification for physiological and anatomical detail in order to fill out a course, but with the recent development of a wealth of material of a strictly psychological nature, particularly on instinct and emotion, psychology can be emancipated from servile dependence on its neighbors.

(3) The course is often weakened by the adoption of a biased attitude which results in the presentation of a one-sided point of view of human nature. It is but natural, and even admirable, that the psychologist shall have personal leanings in favor of some particular school, but the introduction of the personal predilection into an elementary course is bound to defeat the purpose of the course, which certainly should be that of giving the student a well-rounded orientation in the field of psychology. After the smoke of the academic battle between structuralist, functionalist, and behaviorist has cleared, the common ground on which they stand comes to view, namely, that each seeks in its own way to clear up the most complex of all phenomena, human nature, and that each makes a most worthy contribution to the problem. Neither structuralist alone, nor functionalist alone, nor behaviorist alone, can possibly tell the whole story, for if one alone covered the whole field, the others could have never arisen. The very fact that more than one point of view exists is an indication that each one by itself tells but part of the story. Consequently, the delimiting of the course to a single school must lead to a one-sided, incomplete and partial outlook on human nature, human affairs, and human issues. Again, a good deal of conflict is just apparent and not actual, a difference in form and not in substance, in terminology rather than actual thought. There is enough of common substance, even in those issues in which the battle rages fiercest, upon which to build a well-rounded elementary course, without confusing the beginning student with the private battle of psychologists. The object of an elementary course can not rightly be to attract disciples to some personal inclination of the teacher or text-book, that is, to make behaviorists, or structuralists, or functionalists. Such a procedure is similar to practising dietary fads on a sickly child. The implication here is not that attention should not be called to these various points of view in psychology, but that any course is bound to be somewhat ineffective in proportion to the narrowness of the outlook adopted as the guiding principle. We can not afford to teach a school of

psychology, but only psychology, for whereas each school is right from its standpoint, prejudices, and inclinations, the standpoint of each by itself may be wrong.

(4) A fourth common weakness of the elementary course is its illogical sequence. The topics are seldom so systematically arranged as to follow one naturally from the other as a building-up process, and culminating in a conception of human nature as a unit. Most textbooks are of the nature of a series of essays on somewhat related topics, tied together by a common title. In a good many cases, a chapter might be entirely omitted, or all the chapters interchanged, without losing the sequence of the discussion. Once in a while, the phrase "as we have seen before" is introduced as a warning to the student that he is supposed to be progressing, but it is doubtful whether he either knows where he is going or why he is going, but only that he is on the way.

(5) Probably the worst defect in the elementary course, a defect that prevents the material acquired from actually functioning in the life of the student, is that, whereas the course is dogmatic in theory, it is evasive in application. The student might carry away a wealth of psychological fact, but is poverty-stricken in psychological value, since he makes no connection between life as it is lived and as it is pictured in his textbook. Even when a feeble attempt is made to apply a principle, it is found necessary to invent a fictitious John Doe and to place him in trite, unusual, even ridiculous situations. And yet the student is living his psychology while he is studying it. It seems that we are suffering from fear that our psychology will not be thoroughly scientific, and, therefore, keep it carefully away from life from which it is deduced originally. But of what value is a fact, as a fact *per se*? Of what value is learning unless followed by understanding? We do apply psychology to industry, salesmanship, politics, etc., then why not apply it to the business of living, to the functioning of the individual as an individual and as a member of society, that is, to individual and social affairs and problems? If the course is not to help the individual in the business of living, then what is it good for? Science for the sake of science is a beautiful ideal for the scientist who lives by his science, but for the layman it is but an empty slogan. Psychology should be the most vital course in the college curriculum, for, while other subjects might treat *about* life, psychology is the only study that is concerned *with* life itself. Our pride should not be so much that we are teaching psychology as a science, but rather that we are teaching psychology as life. If psychology is dehumanized, what relationship does what we teach bear to our definition that psychology

is the study of human nature? Human nature is what it does; then, if we separate the *what it is* from the *what it does*, what is left of human nature but abstract fiction?

(6) Finally, it is perhaps worth while raising the question as to whether there is any practical purpose served by introducing the elementary student to contentious material that is of private interest to the specialist, but which might only confuse the beginner, since he has no foundation upon which to evaluate problematical points. I refer to such items as theories of instinct, whether there are any instincts, etc. There has no doubt been a marked and wholesome change in our outlook on instinct, then why not present the general ideas and leave the refinements where they belong, to special treatises?

THE AIM OF THE ELEMENTARY COURSE

Apparently, the success of the elementary course in psychology will depend largely upon a clearly formulated and serviceable aim. In the opinion of the writer, such an aim suggests itself as soon as one considers the status of the subject today.

In the first place, psychology is not only with us, but swamping us. Its popularity is so great as to arouse suspicions of superficiality, or even quackery. It has almost become a fashion, so that publishers claim that the word *psychology* on the title page of a book is sufficient guarantee for a substantial sale. Beside psychology, the vogue of electrons and relativity and heredity fade into thin air. Students enter the course with the haziest notions as to what they are to expect: character analyses, dreams, subconscious mysteries, phenomenal memories, overnight success, while the public in general consumes tons of printed matter consisting of rantings and ravings about what psychology will do for your business, your character, your children, your friends, your hidden powers, and your pocketbook. One of the main aims of the teaching of psychology should certainly be to counteract this quackery and exploitation, and to create a sane, wholesome attitude towards, and an intelligent reading audience for, psychology. Would not the accomplishment of such an end be more fruitful than the accumulation of a few facts that are forgotten—and gladly so—as soon as the pressure of the final examination is removed?

In the second place, it should be the object of the beginning course to inculcate in the student a tolerant, open-minded and broad attitude toward human affairs and human problems. There has probably never been a period in the history of man when such an outlook was needed more than today. Life has never been more complex, the very foundation of society never more seriously questioned, and the tension and conflict between individual and individual, between social, economic, industrial, religious and racial groups, never more intense than at present. Since psychology deals basically with human nature, which is the source of all individual and social problems and institutions, there is certainly no other subject in the college curriculum that can present to the student as vividly and effectively the danger of bigotry, narrow-mindedness, prejudices, and pretenses in human relationships.

The problem is certainly at least worth while pondering as to whether the aims set forth above would not only make the psychological course more interesting and more vital, but also ultimately lead to a greater crop of industrial and social benefits than those garnered under the present general procedure.

The outline offered below is neither original nor revolutionary. It is simply the outgrowth of a serious attempt made by the writer and his colleagues to avoid the shortcomings, and to carry out the objectives, that have been discussed in the previous sections of this paper, and is here offered for whatever it may be worth as a suggestion.

PART I. INTRODUCTORY ORIENTATION: THE AIM, SCHOOLS, METHODS' AND BRANCHES OF PSYCHOLOGY

The course opens with a discussion of what psychology is not. For this purpose the literature of the exploitation psychology is utilized in order to point out that this is an attempt to fleece the gullible and unwary by wild promises of benefits to be derived for little money and less effort by taking advantage of mysterious discoveries that will enable one to get something for nothing, or something out of nothing.

The next step is to establish an adequate definition of psychology that will admit of both mind and behavior. Such a definition is not reached arbitrarily, but is gradually arrived at by an examination of several problem cases in abnormal behavior. Briefly, the steps in formulating the definition are somewhat as follows: (1) The dominant trait of the human organism, as of all organisms, is that it conducts itself in certain ways under certain conditions. It is the aim of psychology, in the first place, to study the relationship that exists between these ways of acting and the conditions that call them forth, for the prediction and control of behavior. (2) But, behind all behavior there is a mind of some kind. Whereas, socially, behavior is the all important manifestation of human nature—since society must concern itself primarily with what the individual does, and not so much with what he thinks or feels—individual justice demands that an attempt be made to discover the type of mind that is behind a certain kind of behavior. (3) But mind is what mind does, that is, the sole evidence of mind is in overt behavior, and consequently we can reach the mind only through its external manifestations. (4) Therefore, psychology is an attempt to study the human mind in terms of behavior as scientifically as the complexity and variability of human nature will permit. (5) The value of the study of psychology is, then, that it leads to an understanding of human nature, and it is only where there is understanding that adequate regulation, control, and direction are made possible. An adequate understanding of human nature affords the key to an understanding of one's self and of others, and this understanding leads to self-control and to social peace and stability.

In the evaluation of the different schools of psychology, structuralism, functionalism, behaviorism, mechanism, and psychoanalysis, the purpose is not so much to expound their differences and antagonism as to point out that, although they appear irreconcilable on the surface, they are fundamentally supplementary. Human nature is so complex, and presents so many angles of interest and attack, that any one point of view is bound to be partial and incomplete, and it is only when we synthesize all the schools that human nature, as it actually functions, is revealed. Each school is right from its standpoint, but its standpoint alone presents but a one-sided view of the whole picture.

The same procedure is followed in the discussion of the methods of psychology. Not introspection, observation or laboratory procedure is extolled or belittled by itself, but the limitations of each by itself, and the virtues of a combination of the three is indicated.

In the above scheme there is no marked departure from the usual procedure, excepting that the writer believes that this preliminary discussion is worthy of more time than is commonly devoted to it. In the elementary textbook there is at most one chapter given over to this introductory orientation. At best, the student can carry away but a hazy notion of what it is that psychology is after, that psychologists do not agree as to what they try to do or in how they shall do it. In a one semester, 18 weeks' course, fully six or eight recitations might profitably be thus occupied.

Part II. Motivating the Examination of Human Nature

Following the general orientation in the nature, the points of view, the methods and scope of psychology, an attempt is made to establish an objective for the analysis of human nature with which the main part of the course is concerned, and which constitutes the subject matter of the usual beginners' textbook. The aim is to inculcate in the student the point of view that human nature not only *is* something, but that it also, and primarily, *does* something, and that a clear notion of what it is can be obtained best by a clear understanding of what it does as a unit, an entity, and not in parts. In other words, that sensations, perceptions, instincts, emotions, and other elements of human nature, are the things we live with and live by, and not merely so much data to memorize and discard after the final examination.

This part of the course might be justly charged with being speculative and perhaps 'preachy.' The answer to such a charge might be that the prevailing procedure, which is factual and 'teachy' is certainly not altogether effective, and that a little of the speculative and 'preachy' might help to make the factual and 'teachy' more vital and significant.*

Part III. Analysis of Human Nature

The analysis of human nature opens with a discussion of the nervous system as the machinery of adjustment. The point of view adopted is that an adjustment to any situation is conditioned upon (1) becoming aware of a situation, and (2) reacting to the situation. Awareness and reaction are thus the two products of the mechanism which we call the nervous system, and are but two aspects of one and the same process, namely, from becoming aware of the situation to responding to the situation. Awareness is thus but a *delayed reaction*, and it is to this delayed reaction to which the general name of intelligence is given. The type and degree of the delayed reaction conditions the type and kind of response, and consequently intelligence is coextensive with behavior, and behavior with intelligence. Whenever the behavior is not of an automatic, trigger-action kind, intelligence of some type and to some degree is present.

The body of the course is then devoted to an examination of (*a*) intelligence, which begins with the vague general impression, namely, sensation, and culminates in creative imagination; and (*b*) behavior, which begins with the mechanical reflex and culminates in voluntary, purposeful action.

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*Pressure for space has made it necessary to omit at this point an illustrative syllabus of several lectures.

The treatment is thus developmental and evolutionary, proceeding from the simple to the complex, from the crude to the refined, from the low to the high.

Under the general topic of intelligence are included in sequence, sensation, perception, imagery, memory, learning, and imagination.

Sensation is treated as the first, most elemental, contact of the mind with the objective world. It is, therefore, the grist that feeds the mill of the mind, and from which the finished product ultimately emanates. This first contact of mind with matter is of the nature of a general, vague impression, namely, that something is present to a sense organ.

Perception is this general impression becoming defined and specific. A sensation is thus useful only when, and in the degree to which, it becomes a perception.

Since all perception is in terms of what is already present in the mind, the next step is to examine the nature of this past experience that makes perception possible. This problem presents three aspects for consideration, namely: (1) the state of being of this past experience, or how we carry this past experience about with us, which leads to the topic of imagery; (2) how this experience is accumulated, which suggests the subject of memory and learning; and (3) the highest use that is made of this past experience, namely, in creative imagination.

A similar procedure is followed with behavior. Under this general term are treated, in sequence: reflex, instinct, emotion, habit, and voluntary action.

The reflex is the unit of behavior out of which all higher forms of behavior are developed.

'Instinct' is a general term applied to that state of complex behavior which antedates and precludes experience, training, or learning. 'Instinct' is, then, the condition of native behavior at a certain stage of evolution. Thus the 'instinct' of the ant is not that of the bird, and that of the bird is not that of man, neither in quality nor quantity. 'Instinct' begins with *instinctive behavior*, that is, a somewhat fixed form of behavior which is typical of organisms in the lowest stages of life, but which is somewhat amenable to modification by training and experience. The next higher step is *instinctive tendency to behavior*, which is more plastic than instinctive behavior, and hence more affected by training and experience. Finally, the highest form of 'instinct' is *a general inborn tendency to behavior*, reaching its climax in man, which is most subject to training and experience, and, therefore, man is the organism that can adapt itself to the most varied and complex forms of environment, and hence the most intelligent of living forms.

Habit is to instinct what perception is to sensation, namely, the general tendency becoming fixed as a specific reaction. The general tendency is thus valuable to the organism only when, and to the degree to which, it becomes a habit.

Voluntary action is to habit what imagination is to perception, namely, the highest use made of fixed behavior, or fixed behavior directed towards the accomplishment of a projected purposeful end.

PART IV. FINAL SYNTHESIS OF HUMAN NATURE

Here the organism as a whole, that is, the personality, is treated, as it functions normally and abnormally. The following are the main topics discussed in this section: the nature of personality, the measurement of personality, the nature and significance of individual, sex, and racial differences, the principal types of personality disturbances and abnormalities, and psychoanalysis as a theory and practice.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF ANTIOCH COLLEGE

IV. The Influence of the Group on Psychological Test Scores

By S. Burns Weston and Horace B. English

In Allport's experiments concerning the influence of the group on mental processes, tasks were assigned which do not have high correlation with intelligence.[1] In the present study, an attempt was made to measure this influence when the task assigned required considerable intelligence.

Two test forms of presumably equal difficulty were made by a random division of a group of 4 'intelligence' tests. Each form contained 16 items from Thurstone's Reasoning Test, 4 from Roback's Analysis Test and 4 from his Interpretation Test, and 12 from the Opposites Test devised by Brigham for the American Council on Education (1924 edition). Ten upperclassmen acted as *S*s. The 5 individuals of Group I worked first in solitude on the tests of Form 1, then in a room with all 10 *S*s on the tests of Form 2. The 5 individuals in Group II reversed this procedure: after taking the tests of Form 2 in company with the *S*s of Group I, they took the tests of Form 1 in solitude. When the *S*s were working by themselves, the starting and stopping signals were transmitted by the experimenter from another room. Every effort was made to eliminate the effect of rivalry, and the *S*s were not informed of the precise purpose of the experiment. In scoring, account was taken merely of the number of points correct.

Table I

Group I			*Group II*			*Both Groups*	
Subjects	Score Alone	Score Together	Subjects	Score Alone	Score Together	Alone	Together
a	37	36	f	33	38		
b	32	39	g	35	40		
c	23	28	h	34	34		
d	23	31	i	25	34		
e	27	34	j	38	43		
Mean	28.4	33.6		33.0	37.8	30.7	35.7
σ distribution	3.07	3.82		4.34	3.49	5.42	4.22
σ Mean	1.37	1.70		1.94	1.51	1.72	1.34
σ difference of means	1.01			1.22		1.08	
Difference	5.2			4.8		5.0	

Table I sets forth the results. The number of cases is small but the direction of results is quite unmistakable; 2 individuals did virtually equal work, the other 8 did much better when working in company with others. The difference between the means of the two solutions is so much greater than the standard deviation of the difference as to leave very little room for doubt that further samplings of similar subjects under similar conditions would yield the same result. Conservative practice insists that the difference should be at least 4 times the standard deviation of the difference, a criterion which is satisfied in this experiment.

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[1] F. H. Allport, *Social Psychology*, 1924, 265 ff.

Approach to this problem was made in another way. Practically the entire student body of the college had been given an intelligence test prepared for the college by Dr. L. J. O'Rourke. In taking this test the students were together in groups of fifty to seventy-five. Later a random sample of nearly half the students had been given a group of three tests taken from the 1924 Intelligence Examination of the American Council on Education: Arithmetical Reasoning, Completion, Artificial Language. Raw decile ranks were assigned in both tests on the basis of the actual Antioch College distributions. Twenty-one students who had not taken the second of these examinations were selected on the basis of availability, so far as could be judged, a random group in anything affecting this experiment. These students took the examination in strict solitude. In the light of the previous experiment, we should expect to find their decile rank considerably lower in this test than in the O'Rourke test, since those with whom they are thus compared had had the advantage of working in the company of others. On the other hand, the student would undoubtedly consider this advantage a most dubious one. Table II gives the facts. As may be readily seen, the results are rather inconclusive. There is very slight difference in favor of the test done under group stimulation, but this difference is too small, in view of the variability of the means, to be reliable.

Table II

Subjects	A. C. E. Decile Rank (alone)	O'Rourke Decile Rank (together)
A	10	9
B	1	10
C	1	1
D	8	6
E	6	8
F	7	7
G	6	5
H	6	4
I	8	5
J	2	8
K	1	4
L	5	9
M	6	10
N	10	5
O	4	2
P	4	2
Q	9	7
R	1	3
S	5	4
T	7	9
Mean	5.57	6.1
σ distribution	3.05	2.78
σ mean	.66	.61

σ difference in means	.322
Difference	.52

The second experiment is obviously not so clear-cut. The variables involved are not so well controlled. One does not see, however, what factor causes the difference. Further experiment is obviously seriously needed to determine the effect of group stimulation upon the processes involved in intelligence tests.

V. Notional *vs.* Rote Memory

By Mary Gretchen Jones and Horace B. English

Striking differences between the abilities of the same individual in notional and rote memory have been repeatedly demonstrated. We know, however, that the content of any bit of memory material, and still more its form, may tremendously affect the results. In the following experiment, we sought to ascertain the extent of the divergence when the material to be memorized and the form of procedure was kept as constant as possible. *S* was told that a short story would be read to him, after which he would be asked to "repeat as much of it as you can. It doesn't make any difference whether you remember the exact words or not, but you must listen carefully so that you can repeat as many of the ideas as you can remember." This constituted the test of notional memory. The story, a simple Chinese tale of but 91 words, was one that had been used by the senior author in giving tests of notional memory to children.[1] The selection was read slowly and with rather monotonous emphasis. *S* immediately repeated as much of the story as he could remember in his own words. By having the story written vertically down on the page, *E* could check in the column assigned to each *S* the several items recalled—a method which makes the checking decidedly easier and which, though rather obvious, does not seem to have been described for this test.

Upon completion of this first recall, *S* was told that he was now to learn the passage verbatim. Care had been taken to insure that this task was a surprise to him as we did not wish his recall in the notional memory test to be contaminated by an effort to recall the exact wording. The selection was read again as nearly as possible exactly as before. This was continued until the entire passage could be repeated verbatim. Fifty-six college students of both sexes were used as *Ss*.

As arranged for scoring for notional memory, there were 31 items or 'ideas.' Even though the story is simple and short, there is a very considerable range, from 11 to 29, in the number of ideas recalled. The mean was 23.5 ideas with a S.D. of 4.17.

To master the selection verbatim required from 2 to 9 additional repetitions. The mean was 5.3 with a S.D. of 1.62.[2]

The scores for the two series showed a rather low positive correlation: $r = 0.328 \pm 0.08$ (Pearson coefficient). It is clear, therefore, that despite the several factors which would make for correlation, there is a distinct discrepancy between the ability to memorize the sense of the material and to memorize the exact words. Not only was the material identical and the procedure practically so; the first learning period was an identical factor in both tasks. If there is any common 'memory ability,' that too must have been operative in both. Yet divergence between the two tasks remains. Had it been practicable to correlate the notional memory score with the rate of improvement, the correlation would apparently have been lower still.[3]

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[1] H. B. English, The intelligence of school children correlated with social status, *Psychol. Mon.*, 23, 1917, 311 ff.

[2] Five additional *Ss*, whose records are not included, failed to master the selection verbatim after 12, 16, 10, 9 and 9 trials. The notional memory scores of these same *Ss* were respectively 25. 16, 25, 18, and 20.

[3] All these *Ss* had taken two group tests of intelligence, the Thurstone No. IV and a test devised for Antioch College by Dr. L. J. O'Rourke. Both tests showed quite low correlations with the two memory tasks: Thurstone and Notional, $r = 0.12$; Thurstone and Rote, $r = 0.04$; O'Rourke and Notional, $r = 0.23$; O'Rourke and Rote, $r = 0.15$. These are probably too low to be of much significance; so far as they can be relied upon, they show again that there is a divergence between the two memory tasks. It was originally planned to treat the data by multiple correlation but the lowness of the coefficients makes this seem rather futile.

One must sharply raise the issue, therefore, as to the cause of the difference. Rote memory as we tested it is by no means solely the mechanical association of which Bergson speaks.[4] Yet if such mechanical association exists as a discriminable factor, there is clearly much more of it in the rote memory task than in the notional memory task which would seem to be dominated by the factor of 'meaning.' The results are of course far from conclusive for a variety of reasons. But they do seem to help put the burden of proof upon those who deny the Bergson-McDougall hypothesis of a difference between 'memory' and the mechanics of habit-formation by contiguity-association.[5]

VI. Reproduction and Eduction

By Marjorie Mack and Horace B. English

Among the several principles upon which Spearman proposes to establish the psychology of cognition is the "eduction of correlates."[1] Suppose that one is presented with a certain term or fundament and with the fact that it stands in a determinate relation with something else. One tends, says Spearman, to educe or discover the other term or fundament of the relation.

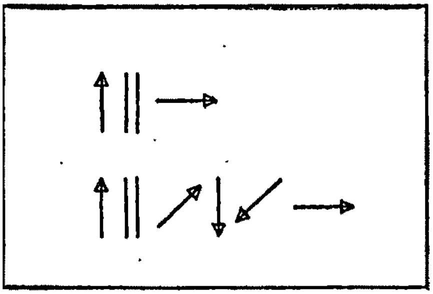

Fig. I

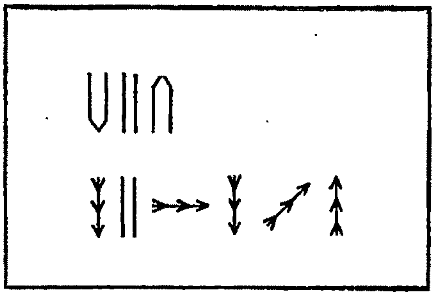

Fig. II

But the result of such eduction not uncommonly is exactly the same as if it had run off by the mechanism of reproductive association. If you ask me to give you a rhyme for *hill*, the word *rill* may come to mind quite as much from association as from any eductive, relational process. In other cases, however, it is clear enough that the 'educed' term has never previously stood in associational connection with the given fundament. It is Spearman's contention that such a process is fundamentally different from reproduction. If so, the two processes would have, almost certainly, different durations. Our problem was to test this derivative hypothesis.

In the first series reproduction of a previous associate alone was necessary, in the second the eduction of a correlate. Otherwise the conditions were as nearly alike as possible. In both series a fairly simple geometrical outline in two spatial orientations was shown in a Ranschburg mnemometer for two secs. There followed immediately the recognition-reaction presentation. This consisted of five figures in differing orientations. In Series I, the first of these, set off by a double line, repeated the first of the original pair while one of the four which followed the double line repeated the second of the original pair. (See Fig. I.) The task was the recognition and identification (by its position at first, second, third, or fourth) of the paired associate.

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[4] Henri Bergson, *Matière et mémoire: Essais sur la relation du corps a l'esprit*, 1907, Ch. II.

[5] Cf. May Smith and Wm. McDougall, Some experiments in learning and retention, *Brit. J. Psychol.*, 10, 1920, 199-209.

[1] C. S. Spearman, *The Nature of "Intelligence" and the Principles of Cognition*, 1923, Chap. VII and *passim*.

In series II, the task was essentially a kind of analogies test. The figures in the recognition-reaction presentation differed from those of the original pair. (See Fig. II.) But between the first of this presentation and the four following the same relation was presented as in the original pair. The task was the identification of the correlated figure—in the illustration, the fourth of course.

The *S*s, psychologically naïve college students, were trained in a rather protracted practice series to react by releasing a key at the same instant they named the figure. With inexperienced *S*s this is a far more natural method than the use of the lip key or even of a voice key. A few false reactions resulted but not enough to affect the results, and any time errors were well within the range of the normal variation of the reaction. Each *S* had, after the practice series, six reproduction and six eduction reactions. The following table gives the mean reaction time in secs. and mean variation for each. We have included without penalty cases where the wrong figure was named. The reactions were timed to within a tenth of a second

S	SERIES I Reproducts		SERIES II Educts		*S*	SERIES I Reproducts		SERIES II Educts	
	Mean	m.v.	Mean	m.v.		Mean	m.v.	Mean	m.v.
A	4.6	4.1	3.4	1.4	M	3.1	1.7	6.0	2.2
B	1.8	0.3	5.0	0.3	N	2.4	0.4	3.4	1.6
C	2.9	0.6	3.3	0.7	O	2.4	0.9	4.5	1.2
D	1.9	0.4	5.4	2.8	P	1.8	0.2	3.6	1.1
E	3.7	1.7	5.6	3.7	Q	3.7	1.4	4.3	0.7
F	2.3	0.2	6.6	1.4	R	2.0	0.3	6.0	3.2
G	2.6	0.6	4.6	1.8	S	2.6	1.1	4.5	1.8
H	2.8	1.2	3.5	0.6	T	2.6	0.5	5.1	1.2
I	2.5	0.5	10.0	5.7	U	2.2	0.3	4.4	1.2
J	1.9	0.4	7.1	2.2	V	2.1	0.6	5.9	3.7
K	3.0	0.8	3.0	0.5	W	3.0	1.1	6.6	5.7
L	1.9	0.4	3.9	1.9					

Mean of 23 *S*s: 2.55 m.v. ± 0.77 sec. for Reproducts;
4.72 m.v. ± 2.06 sec. for Educts.

It will be noted that the average reaction time in Series II. is nearly twice that for Series I. Taking the *S*s separately, 10 of the 23 need at least twice as much time for the recognition of the educted correlate as of the reproduced associate. Subject A has a longer reaction for reproducts only because one of her reactions ran to 15 sec., thus making her average a most unrepresentative figure. Eliminating that obviously atypical reaction, her average is 1.82 ± 0.3 sec. All others were considerably quicker in Series I. It is thus quite clear that for all *S*s the second task was much harder.

"Of course it was." Yet one does not so easily say why. To apprehend the new figures of the reaction-presentation in Series II is a matter of a very small time, hardly significantly longer than the apprehension of the repeated figures of the corresponding presentation of Series I. That factor can hardly account for the difference. The eduction task was amply illustrated and understood by all *S*s.[2] Other accessory conditions seemed to us to be identical. Two seconds seemed entirely adequate for grasping the simple relation pictured in the pair presented prior to each reaction; the apprehension of the relation thus took place before the stimulus to the reaction was presented. *S*'s task in reaction judged superficially was merely one of recognition; in the one case of a previous associate, in the other case of a logical associate. But however much alike the two tasks thus seem to be, they are clearly different as psychological processes, as proved by their temporal course.[3]

We made no attempt with untrained *S*s at an introspective analysis of the two processes. It may be that eduction is merely a very complex kind of reproduction and takes more time for that reason. Either introspective analysis or some other study of dynamic differences is necessary to decide this point. But even this preliminary study seems to make clear that there is an eductive process, hitherto unduly neglected, dynamically different from reproduction.

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[2] The relatively greater m.v. of the eduction series and some part of the lengthened reaction time may be due to an *initial* failure to comprehend the instructions. But inspection of the complete data show that this factor is not large and the final reactions are still longer for educts than for reproducts.

[3] Some degree of confirmation is afforded by the fact that there is practically zero correlation between the two tasks. Indeed such *complete* lack of agreement is rather puzzling on any supposition.

BOOK REVIEWS

Things and Ideals, Essays in Functional Philosophy. By M. C. OTTO. New York, Henry Holt and Co., 1924, pp. viii, 320.

Twelve essays intended to make articulate a social philosophy "which recognizes equally the reality of things and ideals." How can aspiration be combined with loyalty to fact? Though of a semi-popular character, these essays show (in the opinion of the reviewer) an unusual amount of care and judgment regarding the crucial moral problems. They should come to the attention of those writers who are attempting a system of ethics based on Darwinism, Freudianism and statistical sociology. The book displays an unusual mastery of language—novel metaphor and analogy abound, but not at the expense of logical continuity. Otto rejects the notion that an absolute world view is possible. Philosophers must abandon their habit of rushing in where experts fear to tread. But philosophy, as a "dumb sense of what life honestly and deeply means," is feasible. The opportunity of philosophy, in view of the present chaos of moral ideas, is to achieve a new ideal.

Human nature is torn between two allegiances: one to fact, the other to the world of make-believe. An intimate sketch of the child's first acquaintance with this dualism is given. But men, due to the practical pressures of life, must leave the world of fancy, though some, like Lot's wife, are unable to keep from looking back. Others accept the commercial code of the age in which they live; and a few others, including the author, strive to reconcile the world of fact with the world of imagined ideals. In a sense, all are idealists—the only difference is in the ideal espoused. The moral idealist is not the only reformer—the 'possessive' idealist consumes as much energy as anyone in reforming the world to suit his own selfish ends. Otto deserves credit for pointing out the only dangerous philosophers—those who mutely practice meanwhile affecting to despise.

Three essays deal respectively with the idea of right for right's sake, with right as might, and with right by agreement. Kant's defense of right for right's sake is reviewed, then rejected as practically indistinguishable from right for any sake whatever. That is, since Kant fails to distinguish particular rights as the genuine embodiments of abstract right, anyone may accept him and then exploit an empty idealism by any practice, right or wrong. Nietzsche, Treitschke, and Bernhardi proceed by this method. The author accordingly rejects formal morality as having no guiding power in particular moral predicaments. He rejects also the idea that might makes right, after careful study of its arguments. The doctrine has two meanings: "It may mean that might is *essential to the triumph of right* or that right is *merely another name for the triumph of might.*" He criticises the second meaning of the doctrine. To consider moral belief as due to an antecedent fight is not to solve the problem of the identity of might and right—it only removes it to the past. On the other hand, to treat the winning cause as the 'strongest one' is a circular argument.

The author finds moral problems the outgrowth of the conflict of motives. It is the business of reason to create a new end in which the conflicting motives will find some manner of adjustment, though this may involve the sacrifice of some desires. The crucial point about this reasonable sublimation of desires is its creative character. In the following essay the author ventures a most original analysis of the moral points of view. To abbreviate, the author distinguishes three types of theory, but all of which concur in the belief that morality consists in conformity with some preexisting moral standard. The author develops a fourth view which "instead of making the moral life dependent on a previously existing moral order, makes the moral order dependent upon a previously existing moral life" (p. 113). This, if followed out, would lead to the radical view that moral science moves in an unique realm, quite different from the realm of exact science. Whereas the facts of exact science are *discovered*, the realities of the moral sphere are *made*. Values, in short, have an unique existential status. They exist in a realm of becoming—facts *are*, values *ought to be*. Even wants only *are*. They do not, as such, generate a moral situation. It is only because they compete for realization that we can have a moral predicament.

The solution for the conflict of impulses consists in finding the 'richest' attainment of satisfied wants. Such a solution cannot be found once and for all. Also, moral conduct is an art—men should be allowed to cultivate individuality of wants.

Chapter 6 deals with the "Self." The difficulty of setting limits to the 'self' is pointed out. There is no specific date of its conscious origin, we define a man in terms of his whole ancestry only at the risk of neglecting the fact that a man is in a true sense his posterity. The author prefers to avoid this extreme and also the extreme of Hume who declares that the self cannot be found at all. The self, he thinks, takes its rise during childhood with the development of independent interests. Dewey's, "I own, therefore I am," is better psychology than Descartes' "I think, therefore I am." But among the many selves presented by our interests, we may recognize a true self in the paramount interests. Selves are not born. They are *made, created* through the contacts of experience. Granted that the self is an active interest: practically in moulding it, we are chiefly concerned with the conditions under which it develops.

Other chapters deal with topics of less significance for a review made from the psychological point of view. They are entitled, "Science and the Higher Life," "The Soul," "War and the God-Makers," and "Hunger for Cosmic Support."

Wells College C. O. WEBER

Nozioni di Psicologia, con Preliminari Filosofici. By GIUSEPPE TAROZZI. Bologna, Nicola Zanichelli, Editore, 1925, pp. viii, 165.

Besides the philosophical preliminaries the treatise is divided into three parts with subdivisions as follows: (1) Spiritual Dynamics—a general outline of psychology: sensation; association; the unconscious, sleep and dreams; attention and psychic energy; (2) Thought—representation and perception; space and time; memory; imagination; idea, concept, word; judgment and reason; belief and reason; natural talent and genius; and (3) Sentiment and Practical Activity—action and appetition; sentiment and emotion; classification of the sentiments; the superior sentiments; desire and passion; instinct; will; the ego, personality, and character. The treatment of the topics is brief but rather comprehensive, and represents the theoretical and introspective point of view. Attention consists of concentrating associations on a given object and impeding the free course of association to other representative series; it is a foundation of every mental act, beginning with perception. Without attention the mental associations would not be able to order themselves in definite groups adapted to ends, and the mental work would be vague and unproductive. Hence the importance of educating attention. The force of which we are conscious in the efforts of mental concentration is cerebral energy and at the same time psychical and spiritual energy. Appetition is basic to psychic energy. It has pleasure and pain aspects; and pleasure and pain generate, but are at the same time generated by, tendency and aversion. Mind is appetition and will, force and energy, not only when it tends to action, but also when it seems to collect in itself the images and ideas in more perfect calmness. It does not gather but forms; it does not receive but creates. "Even when it is pure thought, and perhaps more then than ever, the mind is force which produces and works, operates, consumes, and reintegrates the energies of life" (56).

Human actions are divided into instinctive and willed acts. Reflex acts, which occur in instincts, lie in the field of physiology. Instinct is a manifestation of internal impulses, which either occur spontaneously or result as consequences of internal excitations which may or may not be representations and perceptions. Instinct as a fact is a series of complex acts making toward a certain end without knowledge of the end or choice of means; but it is not wholly unconscious. It is generally determined by the perception of an object, though this perception has not the function of a true conscious end. Consciousness accompanies but does not determine instinctive acts. Instinct is more invariable and definite in its expression than volition. Definitely determined acts are in the field of instinct. Will is essentially characterized by a determination of the mind toward a given end, whether positively by impulsion or negatively by inhibition of opposing acts. Thus the author takes the feeling of effort as evidence of a mysterious original force in will, rather than of conflict among opposing impulses. James is not mentioned though his views are often closely followed. Psychology as outlined in this book is a long way from becoming a natural science, being still beclouded by the rather free intermixture of arbitrary forces comparable to those ascribed to external nature by primitive man.

Peabody College for Teachers JOSEPH PETERSON

Prolegomena to an Idealist Theory of Knowledge. By NORMAN KEMP SMITH. London, Macmillan and Company, 1924, pp. xiii, 240.

Comprising a reorganization of materials used in Mill's "Lectures in Philosophy" at the University of California, this book gives us a "formulation of an idealist theory of knowledge on realist lines." Much of the book is of an expository nature, devoted to a presentation and analysis of the views of many thinkers of prominence. The comments and criticisms of them are carefully considered. There are three chapters on "The Doctrine of Representative Perception;" one chapter on the "Nature and Functions of the Sensa;" a chapter on "The Categories;" a chapter on "Sense and Intuition;" together with an introductory and a concluding chapter.

The author shows, as he acknowledges, the influences of S. Alexander, Avenarius, Baron von Hügel, Bergson, and Stout. Out of these variant influences he constructs a theory of knowledge and a theory of reality of a clearly realistic sort. Ward's doctrine of a 'presentational continuum' is accepted with a rather radical modification. This consists in denying that the continuum is sensory. What is strictly sensory is not continuous at all. Time and space are the true continuities, they are objective, but not sensory. They are public, not private as are the sensa. The fact that (as Berkeley noted) time and space always appear with sense excitations does not prove that they are apprehended by the senses. The trait of space and time which makes them signally different from sensa is their never-failing implication of a larger space or a larger time in which they exist. Equally constant is their tendency to fall into smaller spatial or time units. The visual apprehension of space illustrates these points. The perception of size and distance is not conditioned entirely by the mere sensory impress. The same retinal impression may appear large or small, near or distant, depending on the attendant circumstances. Visual space is accordingly a matter of sense plus meaning.

As for the sensa, there are two views we may adopt regarding them: (1) either they are independently real, or (2) they are events which require certain physico-sensory conditions for their occurrence. He adopts the latter view. The author thus makes numerous concessions to subjectivism which most of the new realism of the present is striving to escape.

Wells College C. O. WEBER

Blinden-Psychologie. By KARL BUERKLEN. Leipzig, J. A. Barth, 1924, pp. 334.

This is a systematic survey of the mental processes of the blind, beginning with sensation and ending with personality. Some experimental results are given, as well as copious quotations from old and new writers, most of which are theoretical. Much space is given to an exposition of the normal psychology of each field as a background for differentiating the special case of the blind. The theoretical standpoint is that of structural psychology; for instance, there is an extended treatment of ideation. As a teacher of the blind, Buerklen is interested in the educational bearing of his subject; and in this connection, his account of the development of ways to teach the blind how to read and write is valuable.

Buerklen holds the view, supported by experimental data, that the differences of the blind from normal persons, aside from the fact of blindness, are not innate but acquired. This view is contrasted with the prescientific conception of *Sinnesvikariat,* according to which deficiency of sight is compensated by increased acuity of the other senses, either by divine favor or by drainage. Buerklen's position is obviously sound, except in so far as visual defects are innately connected with other conditions and that possibility seems remote.

Stressing the influence of environment, Buerklen attributes the superior acuity of the blind in hearing and touch, as well as their superiority in learning and retention, to practice. Their surprising powers of abstract thought are ascribed to their constant need of analysis as a compensation for the lack of sight. Environment, again, serves to explain the psychopathology of the blind; for they become conscious of inferiority, and are less well able to satisfy their impulses than normal persons. Buerklen describes the frequency and vividness of their non-visual dreams, both waking and sleeping, their great interest in religion, their interest and high powers in music and poetry, their tendencies to "sensitiveness" and "obstinacy," and their proneness to sexual perversion—conditions which in some measure are connected with repression.

Buerklen maintains that through literacy and a fit choice of occupations the blind can become useful members of society. He discourages the effort of the blind to become exactly like those who see, and points out that their defects require peculiar emphasis on their existing powers: the successful Braille alphabet, for instance, is based not on Latin characters but on configurations of points. On the other hand, he holds that knowledge gained through sight can and should be communicated to the blind; with its aid, they are capable of worthy achievement in music, poetry, religion, and philosophy. Because of the sensory deficiency, words are often the sole carriers of thought for the blind; and in any case, a special situation is involved in their acquisition of meanings. Caution must be used, therefore, in judging of the mental processes of the blind on the basis of verbal report.

Buerklen uses general psychology in interpreting the psychology of the blind: the converse relation of the special to the general, which is no less important, he does not explicitly recognize. Congenital blindness, especially, is instructive to the psychologist, since it permits an increase of the knowledge of vision through a comparison of cases when it is present with cases when it is absent. The fact that the Braille alphabet differs from the Latin emphasizes the difference of the tactual perception of space from the visual. The effect of vision on the expression of the emotions is made clearer by a study of the expressions of the blind, which according to Buerklen are peculiar. Again, the powers which the blind acquire are within the reach of normal persons: if the blind have supernormal memories or logical powers or sensory limens, the normal can be trained to acquire them. Again, the psychopathology of the blind may be fruitfully compared with that of persons who see; the results of repression are comparable whether the cause of repression be blindness or something else. The blind are no exception to the rule that the abnormal carries a message to the normal.

Harvard University J. C. Kelson

Die Vererbung geistiger Eigenschaften und der psychische Konstitution. By W. Peters. Jena, Gustav Fischer, 1925, pp. viii, 400.

Psychologists who venture forth upon an exploration of mental heredity may lose themselves in a maze of statistics and speculation. To the credit of Peters be it said that he has left at least a guiding thread behind him. His is an attempt to apply to mental heredity those concepts and principles which have yielded such fruitful results in the field of genetics. He postulates a "larger biology" which includes not only the physical organism but the psychical as well and finds in this concept a point of departure for a science of mental heredity.

At the outset he defines psychology (p. 5) as the science of consciousness, of behavior and of individuality. He thus takes a perfectly safe and (shall we say?) sane position which leaves him free to develop his thesis without wasteful contention. Three progressive principles of genetics seem to him to be fundamental in the concept of biological heredity: the determination of similarities in related individuals, the continuity of the germ plasm, and the genotype. Throughout the book the geneticist will find a familiar terminology in such words as dominant, recessive, mutation, variation, germplasm, soma, *etc.* All these the author would carry over, with more or less modification, to the field of psychology.

Peters takes into full account the effect of environment (milieu) upon psychical traits and points out that those "milieu-factors" which operate upon the physical organism are not to be confused with those which operate upon the psychical organism. Among the former he lists such forces of nature as light, temperature, atmosphere, moisture, climate, subsistence; among the latter, the culture of the race, of the nation or of the family into which the individual is born.

Fully aware of the difficulties which beset the path of one who would apply the genetic method to an investigation of mental heredity, Peters points out that up to the present time psychology has made little progress beyond first principles, *i.e.*, the determination of similarities among related individuals. The chief methods for the collection of data which have been employed are those of direct observation by the investigator himself or by field workers, biography (the "Who's Who" method), the study of educational achievement, and the questionnaire. The unreliability of the last method is soundly scored and the suggestion is offered that for the investigation of mental traits the "situation question" is more reliable than the "direct question." For example, if it is desired to inquire whether a particular individual is avaricious or not, the questionnaire does not ask "Is he avaricious?" but it inquires as to the subject's reactions in a situation in which the given trait may manifest itself. The psychologist then forms his own judgment as to the manifestation of the trait.

In his discussion of heredity and environment Peters does not hesitate to call into question the conclusions of American investigators with reference to race differences. He says (p. 323): "Even if American investigations (based on intelligence tests) show a higher performance for the American whites than for the Indian and the Negro, the question still remains whether this inferiority of the colored races is due to an inferior native ability, as is usually assumed, or to a different 'milieu influence.' If Americans believe that a similar or even identical education assures an identical milieu for the whites, the Indians and the Negroes, they obviously overestimate the 'milieu influence' of the school and overlook unquestionably different "milieu-factors' under which a child grows up in an Indian or a Negro family. Compared with such strongly fixed, traditional 'milieu conditions,' the influence of the school can be considered but little more than a varnish which is spread upon the surface of different materials and different structures. If the Negro were to construct an intelligence test conforming to the needs of the 'Negro psyche' and the Negro system of intelligence, the whites might appear on the average less gifted than the Negro."

The psychologist who knows his genetics will find Peter's book exceedingly interesting and suggestive; so also will the geneticist who knows his psychology. Not least among the good features of the book is an extensive bibliography which the author has attempted to bring up to date to the year of 1922. It is unfortunate that the volume has not been more substantially bound. My own copy is already in two hundred pieces, one for every leaf, a misfortune which I trust the publisher will note, since I should like to place the volume permanently upon my library shelves.

Harvard University C. E. LAUTERBACH

The Young Delinquent. By CYRIL BURT. New York, D. Appleton and Co., 1925, pp. xv, 619.

The problem of this book as stated by the author in his preface is "the psychology of juvenile crime." Its purpose is to give teachers, social workers and all interested in the moral welfare of young people "some notion how the criminal in the making may best be studied and handled." The material for the book is based upon actual case studies of juvenile delinquents made by the author, sufficient examples being given in the different chapters to afford concrete illustrations of the types of delinquency treated and the methods of investigation employed. While the writer has written in a popular rather than a technical style he has consistently maintained a scientific point of view and restraint in the handling of his subject. For the scientific reader copious footnotes supply a wealth of technical data and materials. Throughout the discussion methods of inquiry and treatment rather than results are emphasized.

In his first chapter the author describes the method of the psychologist in attacking the problem of the young delinquent which may be briefly stated as a scientific development of a complete case history. This is concerned with the offense, a careful examination of the child with respect to his mental, physical, and temperamental characteristics, to his environment, his past history, and to the possible means of reform including a follow-up study of results.

From the introductory statement of the problem and the general description of method used, the writer in succeeding chapters develops his problem from several angles, as: hereditary conditions; environmental conditions within the home and outside; physical conditions, developmental and pathological; intellectual conditions, subnormal and supernormal; temperamental conditions; general emotional instability; and sentiments and complexes. Following the discussion of each phase of the problem consideration is given to lines of treatment that might be prescribed.

The author stresses throughout the book the necessity of studying the child as an individual, of understanding his personality, and of making an analysis of his environment and its probable effect upon behavior. He points out the need of finding means of supplying and directing recreational and intellectual activities adapted to the child and indicates methods of diagnosis and treatment. Throughout the book the viewpoint is held that criminals are not born but rather that certain weaknesses or tendencies may be inherited which, given favorable conditions for growth, may develop into criminal traits. This view is strengthened by comparisons of the characteristics of delinquents with those of non-delinquent groups. Both hereditary and environmental influences may have a part to play. Prominent factors in delinquency are poverty, overcrowded living conditions, defective family relationships, poor discipline, and lack of desirable recreational facilities.

This carefully written and well organized book should be of great value to all workers with children, delinquent or otherwise.

Cornell University E. N. FERRISS

Rechtspsychologie: Forschungen zur Individuel- und Massen-Psychologie des Rechts und zur modernen Rechtsfindung. By KARL HAFF. Handbuch der biologischen Arbeitsmethoden; Abt. VI, Methoden der experimentellen Psychologie, Teil CII, Heft 1, Angewandte Psychologie. Berlin, Urban & Schwarzenberg, 1924, pp. 118.

This is an inquiry into the psychological factors in law and in legal decisions. The main interest in the book to psychologists lies in the various opportunities indicated for applications of psychological methods, norms, and principles in the large and diversified fields of modern jurisprudence. The treatment is theoretical and critical. The author brings out emphatically the futility of basing the development of law and the rendering of legal decisions on 'pure sociology' and on subjective principles of right which, he points out, are but expressions of certain particular types of minds and experience. He maintains that the so-called public or common will is largely a fiction; that instead of being an expression of the wills of the people generally, it always bears the stamp of certain strong and more or less self-interested personalities. Even juries are so subject to this rule of the dominance of individuals that one needs not, to get a favorable decision, bring all the jurors to one's own side of the issue, but only those who give direction to the deliberations and conclusions of the jury. Modern legal practices demand that rural, industrial, social, professional, and other classes be investigated psychologically for the establishment of norms or representative types so that the practical demands of all forms of human living may be met more empirically and objectively. The importance of a practically exact evaluation of different mental abilities and maladjustments of individuals coming before the bar of justice is pointed out. The consideration and divisions of 'individual psychology,' and its separation from group psychology, treated largely as conceived by Le Bon and Sighele, suggest that the author is not wholly up to date in his conceptions of modern psychology. The book reflects a wholesome trend in legal practice toward better working relations with this basic science.

Peabody College for Teachers JOSEPH PETERSON

The Theories of Instinct: A Study in the History of Psychology. By E. C. WILM. New Haven, Conn., Yale University Press, 1925, pp. xiv, 188.

This book should have a real value for all psychologists and others who are interested in the development of the theory of instinct as a factor in animal behavior. Taking the theory of instinct hand in hand with the concept of evolution, the author starts with Thales and systematically traces them through all their vicissitudes up to Weismann and Bergson. It should be made plain here, however, that while, in *The Theories of Instinct,* the work is brought up to date, two-thirds of the book is concerned with pre-Lamarckian views, and that for a review of the great mass of quantitative investigation that has been in progress during the last half-century the reader must look to the sequel, *Instinct and Intelligence,* (now in preparation), where, presumably, this phase will be elaborated.

The book is written in an admirable literary style, to which, as a further contribution to ease of perusal, the publishers have added a readable size of type, a dull finish paper and a wide margin. It is well foot-noted, and the material is well organized.

On the whole, the author takes a neutral position; but the reviewer would criticise his interpretation of the pre-Aristotelian Greeks, principally that of Anaximander, Empedocles and Democritus. The author takes too extreme a view in crediting these philosophers with such a modern viewpoint as he assigns to them. For example, he says of Democritus: "It will be abundantly seen, when we come to the discussion of modern theories of intelligence and instinct, from Descartes and Gassendi to Bethe and Loeb, that there is little in modern conceptions, apart from the elaboration of details impossible in Democritus' day, that was not expressed, in principle, in Greek atomism" (p. 15). "Apart from the elaboration of details" is granting too important a concession when, by this phrase, Democritus is exalted to the status of a modern evolutionist. As well try to make out of him a primordial Fechner from his use of the words "minimal sensible of hearing" (fragment 11, Diels). Furthermore, the reviewer doubts if the author is quite fair to Osborn, since the reader gets the impression that the latter held more of a brief for Empedocles than the reference (H. F. Osborn, From the Greeks to Darwin, 37-41) would indicate. The author says: "Empedocles is credited by Osborn with four modern evolutionary ideas." Osborn called them: "four sparks of truth," prefixing this with "a dim adumbration of the truth" and "by happy conjecture, gave his theory a semblance of modern evolution," which latter quotations the author omits.

A feature of his presentation is the author's emphasis on Reimarus, for which he is to be greatly credited. Reimarus has been comparatively unknown, and much neglected by writers on evolution and instinct. Yet he held remarkably well developed views for a worker in animal psychology at his time (1694-1768), and seems to have been quite a brilliant precursor to Driesch.

At the conclusion of the book is a critical comparison of "natural selection" and "the inheritance of acquired characters."

While the reader will occasionally find himself led to what seems the verge of total irrelevance, he will nevertheless be pleased to discover not a few masterly characterisations of men and concepts.

Harvard University A. RENDLE STONE

Complacency: The Foundation of Human Behavior. By ROBERT BRUCE RAUP. New York, The Macmillan Co., 1925, pp. xii, 201.

Complacency is the condition of dynamic equilibrium that constitutes the optimum state for the organism. "The first and chief business of living matter is to get into such relationships with other forms of matter as to maintain itself relatively constant while taking on their energy, utilizing it, and giving it off. . . . This satisfactory working relationship I have chosen to call complacency." It is the condition-from-which and the condition-to-which behavior is always in the process of moving. Annoyance is experienced when the equilibrium is disturbed by a stimulus; satisfiedness comes with the restoration of the optimum.

Evidence in support of the theory is cited from various fields. The irritability, conductivity, contractility, metabolism, and reproduction of living substance are all functions of complacency. A balance is maintained in the autonomic nervous system by the antithetical arrangement of the thoracico-lumbar and sacral divisions. Habit is the utilization of tried methods for the reduction of disturbances affecting complacency; a habit is fixed in accordance with the adequacy of its guardianship over equilibrium. Reflection works to facilitate the protection of complacency. In the realm of values, we find the individual prizes that which arouses disturbances for which he has reduction patterns ready. Man's good lies in the maximation of complacency through the recognition of the interdependence of the disturbance and resumptive phases. The opportunity of education is to be found, therefore, in training each child to handle his own peculiar problem in the reduction of disturbances to his equilibrium, that is, to keep his poise among the variety of situations calling for adjustment.

The complacency theory is not a new one. Avenarius interpreted living processes in the terms of "vital balance;" Kempf, Jennings, Crile, Hering (visual theory), and Rignano (physiological invariability) have also advanced the concept of equilibrium—and Raup acknowledges their contributions. Koffka's theory of closure follows the same trend. Raup has weighted his discussion with a superfluity of quotation, though his citations are extremely well chosen. The choice of the term 'complacency' is an unfortunate one, since its ambiguity is deceptive.

Though the biological approach to the study of behavior is a valuable one, its limitations are of serious consequence, as we see in Raup's treatment of the problems which concern the psychologist. The neglect of the conscious side of complacency has robbed his discussion of much of its fruitfulness. In emphasizing the physiological aspect of the theory, the author tends to lose sight of the psychological side for which the hypothesis was advanced. The interest of the book tapers off at the end where the barrenness of the behavioristic interpretation discloses itself. While complacency is a worthy concept, Raup utilizes it so persistently that its inadequacy as the one unifying explanatory principle for psychology finally becomes apparent.

The book is interesting in the sidelight it throws upon the study of the mind-body relationship. Raup points out the intimate relation of psychology to the other sciences, and the 'complacency' theory indicates how signally the physical sciences can contribute to the progress of psychology.

Harvard University W. F. VAUGHAN

Zur Psychoanalyse. By EMIL RAIMANN. Berlin, Urban and Schwarzen berg, 1924, pp. 97.

This book originated with two lectures given by the author in Vienna, and is an account of the growth of psychoanalysis, with its extension into various aspects of everyday life, and a criticism of its methods and results. Now after thirty years of its activity in the Homeland, when psychoanalysis should be revealing something of its strength and weaknesses, we are told that its growth has stopped, except that the doctrine still unfolds itself in the luxurious phrases of its cult; that by far the greater number of all clinical directors as well as their students are in a greater or less degree, generally and in details, not adherents of psychoanalysis, but are in fact opponents of it. "Today there remains scarcely a single investigator worthy of the name who would go over to Freud's work." Freud and Breuer began in the nineties with the view that in hysteria the environmental greatly outweigh the hereditary factors, and they accordingly found in psychical trauma the causes of hysterical disorders. The unscientific nature of such an interpretation is emphasized. It takes a mere symptom, an accident in the individual's life, for the cause. These traumatic experiences do not cause hysteria, but the significance they take in the life of the individual reveals his predisposition to it, this being particularly evident in the sexual abnormalities dating back into early childhood. Recent war experiences make the Freudian position as to the sex basis of the various functional disorders untenable. A whole constellation of factors must be regarded as the cause of hysteria. If unpleasant experiences are so easily suppressed, pushed into the unconscious and dissociated from the normal conscious life, one may ask if the individual is not already a candidate for hysteria. Freud has mistaken symptoms for causes, and the degree of objective validity of the memories which he traces back to youth that may exist, is but evidence of early weaknesses in the subjects concerned. It is inexcusable for Freud not to go more fully into the neural and general organic basis of these weaknesses. His methods have been those of setting up hypotheses or conceptions of the causes and then straightway treating these as established fact. The psychoanalyst has no objective standard as to which memories are significant, but he searches until he finds what he is after. Thus he moves in a circle.

Most of the usual criticisms, which we need not repeat here, are given by the author, who emphasizes the reasons for holding that the mental derangements and introversions found in paranoia ánd dementia proecox are due to actual physiological changes, some of which are now in a degree familiar to us.

While acknowledging indebtedness to psychoanalaysis for the great stimulus that it has given to the investigation of mental factors in functional disorders, the author holds that psychoanalysis has yielded no single result absolutely beyond the realm of doubt and uncertainty.

Peabody College for Teachers JOSEPH PETERSON

Talks on Psychotherapy. By WILLIAM BROWN. London, University of London Press, 1923, pp. 96.

This little book embodies three lectures given by Dr. Brown at the University of London in 1923. It is supplementary to his earlier books and is not a summary of them.

Lecture I treats of functional nerve diseases, dissociation, hypnotism, and suggestion. In this lecture, Brown discusses the nature and treatment of cases of shell-shock with which he came in contact during the World War and compares his view of the cases with the Nancy view and the Salpêtriére view.

Lecture II treats of psychoanalysis, abreaction and transference, and the libido theory. In this lecture the author discusses the merits and demerits of Freud's and Jung's theories of psychoanalysis and psychotherapy from the point of view of his own experience with shell-shock cases.

Lecture III treats of theories of melancholia, manic depressive psychoses, auto-suggestion and the will. Dr. Brown again refers to the theories of Freud, Jung, and Adler, whom he characterizes as the three outstanding authorities on psychotherapy of the present day. The lecture centers around the cause and nature of the mental conflict which produces the abnormal mental state.

An outstanding characteristics of all three lectures are the author's spirit of broad-minded fairness with which he treats the subject, and the pertinent facts with which he backs up the argument.

Harvard University C. A. SELZER

Die Menschheitsproblematik der Freudschen Psychoanalyse, Urbild und Maske. By EDGAR MICHAELIS. Leipzig, Johann Ambrosius Barth, 1925, pp. 123.

The author undertakes a personality study of Freud by presenting numerous excerpts from the latter's works. He points out apparent contradictions and the noteworthy refusal to admit considerations of the ego instinct into the orthodox doctrine. Michaelis believes that a study of Freud's personality will show a subjective basis for his 'onesidedness.' He is said to avoid the problems of the ego and the ideal not because he does not see them, but because he must suppress them due to his own complexes. "The most distant thing from every man is himself." Through numerous quotations, he passes to a discussion of the universal "deep, unfulfilled longing for gratification of the ego ideal," which he believes to be the mainspring of human conduct. He closes with an appeal for the "expression of the ego's individuality, and true self, which in this way finds its fulfillment"—"which way the psychoanalytic doctrine has systematically barred."

University of Iowa GEORGE S. SPRAGUE

The Professional Treatment of Subject-Matter. By EDGAR DURRINGTON RANDOLPH. Baltimore, Warwick & York, 1924, pp. 202.

Cost of Training Teachers. By HOMER E. COOPER. Baltimore, Warwick & York, 1924, pp. 112.

In the first of these two volumes devoted to the training of teachers the author states his problem in this way: "Should teachers' courses in a specified subject-matter (of say, history, geography, arithmetic, or grammar) be given a special treatment that will differentiate them in thoroughgoing fashion from liberal arts courses in the same field? And if so, what should be the basis and methods of differentiation?" An affirmative answer is given to the first question, while the professional treatment of subject-matter is suggested as the best solution. This involves two essential ideas: (1) "on the side of scholarship such 'new view' of generally familiar material as will reveal its racial significance and its potentialities for public education, and such extensions of scholarship as will be most likely to insure flexible control of the problems of utilizing the study for social purposes in a particular teaching position; and (2) on the side of method it implies a conscious organization of instruction in subject-matter with reference to the professional responsibilities of the future teacher—which among other things involves the transmitting of a fruitful method of study and the purposeful affecting of attitudes and technique." Applications of this idea to geography, literature, and arithmetic are suggested.

The second volume is the result of a study of New York normal schools and presents data regarding existing teacher-training institutions, present cost of training, accepted standards of training, present status as to training, and requirements for meeting the standards.

Both volumes present valuable points of view and techniques to those interested in the training of teachers.

Cornell University JULIAN E. BUTTERWORTH

Skill in Work and Play. By T. H. PEAR. New York, E. P. Dutton and Co., 1925, pp. 107.

This book is intended for the elementary student, but the first two chapters are particularly of interest to the psychologist. Here the problem of kinaesthesis in human learning is isolated, and common psychological terms like knowledge, muscular skill, ability, and capacity are clearly distinguished. The thesis of the book is that our lack of accurate introspections of bodily experiences—a lack due to the fact that our vocabulary of learning is in terms of vision and hearing—is the principle cause of the difficulty in using kinaesthetic sensations as aids in learning. As a solution the author suggests the accumulation of accurate introspective reports and further investigations by means of 'motion study' along the lines set down by Gilbreth.

A critical consideration of the problem of acquiring skill occupies Chapter 2. Here Pear examines the theoretical conclusions of Freeman's *How Children Learn.* In a certain sense Pear's discussion is a critique of Freeman's work by way of amplification. Invariably Pear considers Freeman's conclusions to be too simple to explain adequately the complex problems involved. Pear objects to Freeman's emphasis upon the "series of movements" to the exclusion of other factors, such as time-values, in the execution of these movements. He also undertakes an amplification of the psychology of learning by trial and error. He contends that it is improbable that there should be any mere stumbling by blind chance on the correct act when the "muscular activity involved has been properly discriminated, labeled, and characterized."

It seems to the reviewer that much of this difference of opinion is a matter of point of view. Freeman treats the subject from the standpoint of original endowment in learning; Pear, from the standpoint of the efficacy of the teacher in imparting an acquisition of skill. Nevertheless, Pear's suggestions are timely as a critique of the notion that learning is primarily by trial and error and thus principally dependent upon chance.

Harvard University THEODORE KARWOSKI

The Passing of the Phantoms: A Study of Evolutionary Psychology and Morals. By C. J. PATTEN. New York, E. P. Dutton & Co., 1925, pp. 103.

This little book is written for the lay reader with a view to giving him "the master-key which unlocks the secrets regarding the evolution of human morality." It recounts instances of acts which are intended to illustrate the evolution of the "mental and moral faculties in lower animals." When the embryological and anatomical evidences of evolution have been given and the author passes beyond these to the evolution of mind, many errors and crudities appear. These are sure to give the lay reader a wrong notion of psychology and its methods. The point of view is that of the arm-chair comparative psychologists of pre-experimental days, and the author goes into disputed questions with the same certainty that characterizes the announcement of established facts. Thus we are told that man is a highly gregarious animal, and the origin of 'the social instinct' is traced. Most curious and naïve accounts of learning and of the unfoldment of sorrow, joy, and other emotional complexes are given. Evidences from the author's observation are marshalled to show that hawks have a faculty of attention and that persons have gained their "confidence and love"; that these birds love music and are strongly imaginative; that "imagination is highly developed in dogs;" that these animals, as well as horses, believe in spirits; that ants have moral sense; etc. Thus morality and religion have their pre-human roots of superstition. The author regards "the evolution of theology as an inherent instinct or inherited experience in the natural history of man, which will likely go on for an immensely long era yet to come." Surely this bit of psychologizing by a man of science unacquainted with psychology cannot be surpassed for errors even in the literature of the Fundamentalists, for whom, apparently, the book is written. There seems to be room for some popular writings on real psychology. It is a question whether literature of this kind is not more detrimental to scientific progress, and to psychology in particular, than the frankly anti-evolutionary books now circulating among the laity.

Peabody College for Teachers JOSEPH PETERSON

The Fundamental Principles of Learning and Study. By A. S. EDWARDS. Baltimore, Warwick and York, Inc., 1925. Revised Edition, pp. 255.

Those who are familiar with the first edition (1920) of this book will, no doubt, welcome certain changes that appear in the revised edition. The most important changes, perhaps, are a better organization of material and the addition of chapters relating to the higher type of education. The work has been brought into line with recent developments in that subject.

In this volume the author treats, in very brief outline, the general field of the subject; but the theme of it all seems to be the *Habit Theory* of education. Habit, as used by the writer, refers "in the broadest kind of way to the permanent acquisition of the individual."

"The formation of so many thousands of habits as the aim of education is he says, "only a more clearly defined way of saying that the aim of education is the development of character . . . for *character*, when it is analyzed, *is found to be the sum total of one's habits of thinking, feeling, and doing*" (42). Again "Education is ever the modifying and remaking, as well as the making, of habits" (56).

Several recent experimental investigations concerning the transfer of learning offer positive evidence in support of the author's conclusion that specific learning is economical and valuable; but 'general training' is little as regards quantity, and its influence is uncertain, for it may tend toward improvement or impairment.

Throughout the book, quotations from psychologists and educators are effectively employed in bringing out the central idea. The selected bibliography and the review questions are of significance to the student and the teacher.

The form of presentation is clear and pointed, so that the book might be used as a text in a short course, or it might very well serve somewhat as a guide to study and discussion in a long course.

Cornell University H. D. Powers

Psychologie der Umwelt. By Willy Hellpach. Handbuch der biologischen Arbeitsmethoden, Abt. VI, Methoden der experimentellen Psychologie, Teil C, Heft 3. Berlin, Urban and Schwarzenberg, 1924, pp. 109-218.

An exposition, with desultory illustrations, of the logically possible methods of experimental psychology as applied to the study of the external factors conditioning human behavior. The three methods mentioned by the author are: (1) the method of simple observation; (2) the method of statistics (mass-observation); and (3) the method of experiment. He considers these methods with reference to the following three general classes of conditions: (A) the "natural Umwelt"—which includes weather, climate, ground or soil, and landscape, in a broad sense of this term; (B) the "mitmenschliche Umwelt"—the field of social psychology proper, interaction between persons; and (C) the "Kultur-Umwelt"—books, dogma, art, etc.

The methods discussed are too general to be of much value to the experimentalist. The treatise reads like the writing of a sociologist with addictions to logic rather than that of an experimental psychologist. He shows, also, a lack of discrimination in his choice of methods, as is evident from the five page discussion of "telepathy" as one of the methods of studying the interaction between individuals.

Peabody College for Teachers L. H. Lanier

Uber die Persönlichkeitstypen und die Methoden ihrer Bestimmung. By Otto Selz. Jena, G. Fischer, 1924, pp. 44.

The problem of types which has received much attention in Germany in the past decade, was surveyed by the author for the VIII (Leipzig) Congress of Experimental Psychology (1923), and is here published in a somewhat extended form. Particularly valuable are the fifteen supplementary pages containing a discussion of special points and a bibliography which, so far as the German work is concerned, is practically complete.

The author considers the methods of determining types under three headings, *viz.* historical science, psychology, and psychiatry. Recent progress in correlational work and theories of constitutional types are noted, but it is clear that to Selz the most significant progress has been made along the lines of the prevailing concept of *Struktur*. Dilthey, Simmel, Jaspers, Troeltsch, and above all Spranger are given prominence for their development of an 'understanding' (as opposed to an 'analytic') method. Psychograms and studies of individual differences, these writers agree, neglect the integrating factors in personality. Selz makes the additional point that the syntheses in personality (typical interests and values) are in the last analysis the really crucial *determinants* in thought processes.

Harvard University G. W. ALLPORT

Textbook Selection. By R. H. FRAZEN and F. B. KNIGHT. Baltimore, Warwick & York, 1922, pp. 94.

On the basis of five criteria—interest, comprehension, permanent methods of study involved in the text, permanent value of the content, and mechanical construction—the authors develop a score card for rating textbooks in biology. In spite of certain artificial features in this particular score card, the use of such a method of evaluating textbooks should, by directing attention to significant factors, result in wiser selection. The latter part of the volume is devoted to reporting two studies: one on applying the criterion of interest to high school English literature texts; the other on applying the criterion of comprehension to geography texts for elementary schools. The entire volume is interesting as showing efforts to use scientific method in some of the less objective phases of education.

Cornell University JULIAN E. BUTTERWORTH

A Text-book of Psychology for Nurses. By MAUDE B. MUSE. Philadelphia, W. B. Saunders Co., 1925, pp. 351.

'Psychologies' are written now by anyone and for everyone. This book purports to present 'reaction psychology' in an abbreviated form so that it can be assimilated readily by nurses in training. Abbreviation scarcely can be recommended as an aid to clarity. There are many facts and much information in the book that probably is of value to the young nurse but the psychology is not what it might be, as the following quotations bear witness. "The patient's sense of hearing: No sound by which the sick are tortured is more exasperating than the sibilant buzz of whispering." "The *range* of attention is from the pathological distractability sometimes termed 'flight of ideas,' to a fixation of attention that is likewise pathological."

Hobart College FORREST L. DIMMICK

Ethics in Theory and Application. By HORATIO W. DRESSER. New York, Thomas Y. Crowell Co., 1925, pp. xvii, 509.

Distinguishing ethics as a normative science from descriptive sciences such as biology and psychology, Dresser assumes that the self or personality is real and that the moral order, including moral values, duty, obligation, and conscience, is a part of the ultimate structure of the universe. He concludes, among other things, that individual goodness means realization of our powers through organization of our many-sidedness, orderly development and expression through dominance of a purpose, that no matter whether the actions of the individual are conditioned by his heredity and environment, still the moral consciousness admits no evasion of individual accountability, and that sin is an irrational fact which can not be explained by heredity and environment. The contention that ethics is a normative science forestalls psychological criticism; but one suspects that this immunity is a curse rather than a blessing.

University of Chicago C. M. PERRY

Suggestion and Mental Analysis. By WILLIAM BROWN. London, University of London Press, 3rd. edition, 1923, pp. 176.

Psychotherapy has had two relatively exclusive lines of development, that of psychoanalysis, which includes the word-association methods and dream analysis, and that of suggestion, which takes various forms from autosuggestion to hypnoidal and hypnotic suggestion. These lines the author sees as converging, both as to theory and method. The discussion is largely elementary.

The book would have been improved by a revision of the sixth chapter and the omission of the section (chapters X-XII) which is devoted to a consideration of Bergson's philosophy.

Harvard University E. G. WEVER

Elementary Psychology. By ARTHUR I. GATES. New York, The Macmillan Co., 1925, pp. xiv, 594.

The author offers this text as a general parallel to his earlier one which is designed for students of education. The book is one of a type of psychology texts which has been appearing in numbers recently and which assembles facts and theories from all sorts of psychological and near-psychological sources and attempts to present them without bias. This author has been successful in some measure, though the treatment of Instinct, Habit, and Learning betray his dominant interest in Education. The chapter on 'Methods and Subject Matter' is better than most of such discussions. The description of the organism is very good and is up to date and many of the new illustrations are excellent. The treatment of 'Sensations and Feelings' is however, inadequate. The last two-thirds of the book follow the educational and mental testing trend of the times. The material given there is an excellent digest of recent work in those fields and its results. The impression one gains of the text is that it is a very usable collection of valuable facts from psychology but that it is far from being a systematic presentation of psychology itself.

Hobart College FORREST L. DIMMICK

The Humanizing of Knowledge. By JAMES HARVEY ROBINSON. Published for the Workers' Education Bureau of America. New York, George H. Doran Co., 1924, pp. 117.

For those concerned with the problem of the systematic democratization of science Professor Robinson's little book should prove interesting. He discusses the position of science and scholarship in modern society and suggests reasons and remedies for its opposition by many lay minds who fail to understand what it is all about.

To some persons this book will appear as an attempt to explain why Professor Robinson wrote his *Mind in the Making*; for the author urges a less austere attitude on the part of scholars to popular but careful treatment of their subjects, on the grounds that such treatments are of great educational value and indirectly aid research by interesting those who are in positions to offer material aid to science.

In the last chapter specific suggestions are given as to how and by whom popular treatments of learned subjects should be written, and as to how publishers can coöperate in this form of adult education.

Harvard University HUDSON HOAGLAND

Were You Ever a Child? By Floyd Dell. New York, George H. Doran Co., 1921, pp. 202.

The author points to many of the glaring failures in methods of teaching in public schools; and presents, in popular style, the new educational aims which are bringing revolutionary changes into those class-rooms which are not afraid to throw off tradition.

Harvard University M. B. Pratt

Leitfaden der physiologischen Psychologie in 16 Vorlesungen. By Th. Ziehen. Jena, Gustav Fischer, 1924, pp. 653.

This is the twelfth edition of a well-known work. From a modest volume of some two hundred pages in 1890 the book has grown to formidable proportions. The present edition contains 653 pages and 85 illustrations. Naturally Professor Ziehen has availed himself of recent investigations in the field of physiological psychology to bring his outlines up-to-date. Several former editions of the book have been translated into English. It is to be hoped that the twelfth edition will be accorded similar consideration.

Harvard University C. E. Lauterbach

Der Künstler. By Emil Utitz. Stuttgart, F. Enke, 1925, pp. 64.

Four lectures: (1) The Problem of a General Science of Art; (2) The Artist's Creation; (3) Art and Mental Disease; and (4) The Character of the Artist.

Introduction to Educational Psychology, A Manual for an Introductory Course. By Howard Taylor. Baltimore, Warwick and York, 1925, pp. 172.

This syllabus is designed for use with any standard textbook in educational psychology. Every chapter presents a quite complete list of secondary references, an outline, and a list of problems and questions. Every other page is left blank for student's notes.

The Psychology of Accidents and "Symptomatic Actions." By Samuel A. Tannenbaum. New York, The Tenny Press, 1925, pp. 92.

An entertaining and effective criticism of Freud's theory of mishaps as announced chiefly in his *Psychopathology of Everyday Life.*

Intelligence in Expression. By Leone Vivante. Translated by B. Bullock, with foreword by H. Wildon Carr. London, C. W. Daniel Co., 1925, pp. ix, 205.

This book is concerned with neither 'intelligence' nor 'expression' as the terms are ordinarily used in psychology.

NOTES

MECHANISM, MEANING, AND TELEOLOGY IN BEHAVIOR: A REJOINDER

The reply to Dr. Weiss' reply[1] to my article[2] is that it is not a reply. The reasons for this are that when he considers specific points he is generally not talking about the same things I am, and when he discusses the larger questions involved it is on an assumption that shows clearly that he has not understood the point of view from which the article was written. I shall consider his paper from these two points of view.

I

Beginning with the latter of these topics, I want to repeat what I said in the article, namely, that I consider the "introduction of theories of reality into the work-shop of the scientists" a delusion and a snare. It is necessary to emphasise this because the difference between Weiss and myself, so far as our points of view are concerned, is exactly here. He maintains that behaviorism needs a metaphysical support if it is to be capable of scientific development; I hold, to use his own phraseology, that to bring philosophy 'downward' into science interferes disastrously with the primary tasks of describing, analysing, and generalising the concrete material with which, in the particular sciences, we are directly concerned. That Weiss seems not to be aware that our main difference lies here is a little surprising because it was given a prominent place in my article, and because it was on this very ground that my criticism of McDougall's advocacy of "purposive striving" as a *psychological* category was undertaken. The only difference between Weiss and McDougall, at this point, is that they appeal to different historical philosophies in order to make a 'go' of their psychologies, but neither of them escapes—Weiss less successfully than McDougall—the deadening influence of this initial blunder.

With this in mind, I am sure that anyone who reads Weiss 'reply' must be grateful to him for adding to the gayety of his fellow psychologists who, as a rule, do not have an unlimited opportunity for indulging their sense of humor. For, in the first place, Weiss is as serious as a Mediaeval Scholastic when he is confronted with his "materialistic monism," and is almost reverential in his gratitude for the light it sheds on the details of behavioristic doctrine; and, in the second place, there is something almost ecclesiastical, not to say pontifical, in the way in which he politely hurls anathemas at my head when he gratuitously assumes that, in principle, I am guilty of the same kinds of sin as himself in another cause. Since, however, as a psychologist I stand outside the wranglings of the philosophic schools, and am quite unable to understand how philosophy can be useful in developing into its details any empirical aspect of reality, I may be pardoned for wondering why Weiss thinks it is all right for the psychologist to be a 'scientific monist', and all wrong for him to be a 'dualist,' a 'vitalist,' or by whatever other name the philosophy which is not 'scientific monism' may be called. My wonder is not lessened when I observe that this obvious inconsistency determines the kind of reply he gives to certain sections of my paper.

I shall group together the passages in Weiss' reply which show (*a*) the influence of materialistic determinism in Weiss' theory of behavior, and (*b*) those in which he mistakenly supposes I am guilty of substituting philosophy for science in my criticism of behaviorism.

(*a*) To the former belong: (1) The reference to Child and Lillie. This is in answer to my statement that the present situation in biology leaves the question of materialistic determinism an open one for the psychologist. No two names, and no more complete and representative list of authorities, *on either side the question*, can be made the basis of more than an *ex parte* statement. Weiss apparently thinks otherwise, and he does so, I think, because he has already accepted a philosophical position which seems to be supported by what the writers named say with respect to the material that falls within the limited fields of their respective investigations. It should be fairly clear that the truth of materialistic determinism, *as a theory of reality*, would not be settled with the acceptance by all the scientists of the deterministic position. Perhaps only then could we begin to discuss the question with a clear understanding of the problems involved! As matters are, I do not think the psychologist should take a gambler's chance which is the only option open to him at the present time. (2) Statistics, Weiss argues, necessitate mechanical causation, even in social life. This raises the question of the relation of mathematics to the particular sciences, and ultimately to philosophy. Historically and in practice mathematics have provided a useful method for investigating nature, but itself does not and cannot tell us a single thing about the constitution of any fact that belongs to the realm of nature. It directs our investigation of nature, but does not give us our doctrine of nature. Moreover, the deductive philosophies, based on the mathematical ideal, from the time of Descartes to that of Hegel, should be for every one a sufficient warning. Statistics, which are a part of the mathematical equipment of science, are subject to the same general limitations, and their value lies in their ability to give rules of averages—not scientific laws; or general tendencies—not specific principles of behavior. They are not, therefore, useless, and I wish merely to point out that it is impossible to deduce from a statistical law the modes of behavior of any concrete individual, and that this should be possible if Weiss' contention is correct. (3) Weiss is under the impression that "mechanical monism has made physics, chemistry, biology what they are," and for that reason, he says, it is worth trying in psychology. That is, in principle, much like saying that because quinine is useful in malaria, a common cold, and loss of appetite, it is worth trying in a case of broken bones. His contention would be valid only if the material of psychology were the same in kind as that of physics, chemistry, biology; but, as I pointed out, it is exactly that question that the behaviorist has begged by his acceptance of a materialistic metaphysics. Moreover, Weiss' statement of the relation of the particular sciences to monism is incorrect. The sciences advance according to the degree of their ability to develop specialised techniques adapted to the specific subject matter of each of the sciences, and are able to describe, analyse, and generalise the aspects of reality that come within their respective fields, and not at all in the degree of the correctness of the metaphysical view of the universe that particular scientists happen to hold. Sir Oliver Lodge, for example, believes in the existence of a spirit world in communication with this planet, but whether true or not, this philosophical belief has no relation to his work as a scientist. Why, therefore, the psychologist should burden himself with scientific monism which is as irrelevant to his work as spiritualism, as Weiss wishes him to do, is not at all clear. (4) Weiss practically admits my fundamental contention in this section when he says that in defining behavior as a form of motion, he does intend to commit the psychologist to the philosophical view of materialistic determinism. I disagree with him entirely, first that the scientist is a philosophical propagandist, and secondly that science halts until the ultimate mysteries of the universe have met with a true—monistic, dualistic, or pluralistic, spiritualistic, materialistic, or immaterialistic—explanation. I contend that, as psychologists, we are nothing but scientists.

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[1] Dr. Davies on "Mechanism, meaning and teleology in behavior," this JOURNAL, 37, 1926, 450-459.

[2] Mechanism, meaning and teleology in behavior, *ibid.*, 2-24.

(*b*) I turn now to those passages in which Weiss avoids a direct question which has a scientific signification by the assumption that, like himself, I am talking from the standpoint of an accepted metaphysics. Here belong: (1) His denial that the scientist is concerned with the reality or actuality of his data. Is the question of actuality scientifically irrelevant? Does it make no difference whether, to use his own illustration, the responses Weiss makes to a stimulus are actually the responses that he does make, and whether these responses preserve their original character when in turn they become stimuli for other responses? Does it make no difference to the chemist whether the elements that are in his test tube have a fixed character, and whether the reaction between the elements is of fixed configuration? If such elementary logical requirements as these are neglected, or if they are irrelevant, how would it be possible, as Weiss contends, for either chemist or behaviorist to check "the degree of reliability (that belongs to their respective observations) by the degree of correlation which exists between the various individuals making the same observations"? The fact is that the question of reality, the assumption of a world of objective existence, is an important and fundamental necessity if science is to exist; it is only in pure mathematics, that the question of reality is irrelevant. For pure mathematics, as Bertrand Russell says, "consists entirely of assertions to the effect that if such and such a proposition is true of anything, then such and such another proposition is true of that thing. It is essential not to discuss whether the first proposition is really true, and not to mention what the anything is of which it is supposed to be true...Thus mathematics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true." When, therefore, Weiss argues that all questions of reality are irrelevant for science, he throws himself open to the retort that he is contending that behaviorism, like mathematics, "never knows what it is talking about, nor whether what it is saying is true." (2) The problems of the secondary qualities. My contention, put in the form of a syllogism, was that Weiss' advocacy of materialism ruled out, without injury, this problem, and that therefore he could not have an open mind as to the kinds of thing that might possibly exist in the empirically discoverable world of reality. Yet Weiss thinks I was arguing "to introduce the syllogism into the study of human behavior"! Would it have been simpler to say that the syllogism was my "verbal response" to his "materialistic determinism" as stimulus? (3) It is again the philosopher in Weiss that leads him to suppose that I urge upon psychologists the acceptance of metaphysical dualism, albeit in an attenuated form. He also hints, in the latter part of his paper, at my advocacy of the "spectre of vitalism." Either, it would seem, any stick is good enough to beat a barking dog, or there is unearthed here a philosophical monstrosity worthy of a place in a museum of curiosities, and in that case Weiss should have made more of his 'find.' (4) The implications of the term 'meaning' we are told, "are obviously non-scientific," by which I understand Weiss to mean 'philosophical.' As if I had not warned the reader against such an interpretation of the term! I had even said that what was required was "a study of the *mechanism* by which what is new in the conditions of life is met by behavior that goes beyond the limits of heredity. The question underlying this study is whether behavior ever exhibits this characteristic." How can my belief, or want of belief, in 'dualism' be an answer to this question? It is, of course, as irrelevant as 'materialism.' (4) The problem of perception is dealt with by Weiss in the same way. We are told that it is "loaded" so as to include a "non-mechanistic factor." Of course, for me perception is just as objective a problem as the transit of a star, it concerns the observed characteristics of behavior; it is one of several classes of behavior of which reflex action, instinct, emotional dispositions, etc., are others. All I am interested in is a description of the observed characteristics of the concrete instances that are agreed upon as perceptual. It seems so difficult for the behaviorist to understand the necessity for this! For, as Weiss admits, my illustration of a 'city street' is "too complicated for a *behavioristic* (italics mine) analysis," and he proposes to substitute "learning to dance" which, he says, is not "essentially different." In this he concedes my point, because in the former, the conditions of behavior are never twice alike, and, therefore, the behavior must in every case be different, and, in the latter, the conditions and the behavior are fairly standardised. The question, we see, has nothing to do with "loaded" definitions, but with the "loads" that concrete facts themselves carry.

II

In this section I shall deal with certain misunderstandings with regard to (*a*) the logical foundations of science, and (*b*) specific statements, whether right or wrong, of my article.

(*a*) Under defects of logical method I include: (1) The descriptive poverty of behaviorism. As Weiss understands it, behaviorism is largely a theory of how particular reactions take place, not a description of what the reactions are. I do not mean that the latter question is entirely neglected, but that the importance the former is given—based chiefly on the sucesss that has attended the study of the simpler forms of reaction that have a close analogy to physical reactions—has operated in a prejudicial way when more complex reactions are in question. These are assimilated to the type of the simple reactions and treated as such. As a question of methodology, there is no objection to this procedure provided that complexes are analysed into their simpler component reactions, and, as in the analagous case of the parallelogram of forces, these are resolved *seriatim.* But if this were done, it would be found that, as in the case of the illustration, the final resolution gave no clue to the particular constituent forces, that these could not be recovered from the former, as would have to be the case if materialistic determinism were to prove of any value in the theory of behavior. (2) Behaviorism is weak in preliminary analysis, and neglects or entirely overlooks the conditions under which behavior takes place. This false abstraction from the conditions which make behavior possible gives the impression that behaviorism is manipulating a set of very generalised concepts according to a set of formal rules. One illustration of this neglect of conditions is seen in Weiss' discussion where he says that "the statement, 'behavior is a form of motion,' is both a biosocial stimulus and a biosocial response," meaning by 'statement' in both instances the light pattern of the words on the printed page. Now, of course, the light pattern is the same objective fact in the one case as in the other; and it is impossible to say whether it is a stimulus or a response until the whole concrete situation in which the 'statement' functions has been completely described. If this were done, it would be more obvious than it now seems to be that it is exactly the difference in the contexts of identical patterns that gives to these patterns whatever meaning they may have: 'physically identical' does not necessarily mean 'psychologically the same.' It is, in fact, a careful regard for conditions that makes science possible, for as, Whitehead has recently remarked, "the key to the process of induction...is to be found in the right understanding of the immediate occasion of knowledge in all its full concreteness...We find ourselves amid insoluble difficulties when we substitute for this concrete occasion a mere abstract in which we only consider material objects in a flux of configurations in space and time. It is quite obvious that such objects can tell us only that they are where they are."[3] (3) Behaviorism is not critical of its own foundations. This comes out nicely where Weiss remarks that demands of this sort "are merely stimuli produced by movements of the speech mechanisms which may or may not be investigated with respect to their own sensorimotor antecedents," and where he brushes the question aside by saying that "philosophers have no sense organs which the physicists do not have," and that, therefore, "they cannot see a 'reality' which the physicists are unable to see." It is a mathematician who has recently warned us that "if science is not to degenerate into a melody of *ad hoc* hypotheses, it must become philosophical and enter upon a thorough criticism of its own foundations."[4] I had already insisted upon this in regard to behaviorism when I said that if it is to undergo a "systematic development" of doctrine, it had to submit to or undertake the task of "immanent criticism," that is, to turn the light of reflective criticism on to its own foundations. Weiss does not see the necessity for this, and he uncritically accepts the doctrine of materialistic determinism into conformity with which, he says, we *must* shape our psychological doctrine if we wish to be described by the sacrosanct term 'scientific.' Determinism is Weiss' *latente Einstellung,* and it not only narrows the scope of his science, but renders him incapable of recognising the negative instances, for instance 'memory' to take Russell's example, that may interfere with the smoothness of the tale.

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[3] A. N. Whitehead, *Science and the modern world*, 1925, 64.

(6) A selected number of misunderstandings of specific statements and questions is the following: (1) The question of consciousness. It ought to be clear from what I wrote that I do not employ the term 'consciousness' to imply a substantialist theory of mind. Further, no one who has read the literature of the subject since James' *Does Consciousness Exist?* should suppose that the denial of this view necessarily relegates the term to limbo. But this is what the behaviorists seem to do: they say, either you must mean by consciousness an existential something, or you must mean nothing. It was this position that I challenged in my article. Weiss' reply has not relieved me of the impression that the behaviorist is neurologically blocked on this subject in the 16th. century Continental view of the problem, and that he forgets that the English empirical school at that time were saying something of a very different sort. No one, of course, wants to confine the discussion of this subject to its historical antecedents, but too many seem not to be aware that history may sometimes save us from talking nonsense. The behaviorist's attitude toward consciousness is difficult to understand: he ignores history and refuses to adopt the empirical position, and, on the basis of a heightened emotional complex, falls into a sort of hysterical blindness whenever the word is mentioned. This is extremely unfortunate, because it deprives us of the criticism that is necessary if the facts that the word denotes are to be brought to a clear focus. I indulged the hope that help would be forthcoming. My remarks were irenical, and I went as far as I thought the behaviorist would be willing to go in advocating "a strictly empirical attitude" in the matter, and I suggested a mode of procedure that might be followed without prejudice to the conclusions. Weiss, however will have none of it; he rejects my "mental order," which was offered as a more neutral term than consciousness, even as a starting point; and he quietly but promptly dumps the terms and the problems they suggest overboard. It is clear that this attitude is based on a misunderstanding of what the discussion involves, and the misunderstanding is emphasised when he supposes that I was or am interested in "the content of the term consciousness." It is very remarkable that he should suppose this, because, in the paragraph from which he quotes, I had said definitely that we should empirically "collect specific instances" and "describe their characteristics." From this and from the whole tenor of the article, it should have been obvious that I was not concerned with the meaning or connotation of consciousness but with its denotation. To what characteristics of behavior, if any, does the term point? That was and is the question to which, if the behaviorist pays any, even a controversial, attention, we have a right to something more than a categorical denial or a wholly partisan answer. (2) The term 'apprehension.' When Weiss says that scientifically I can only mean by this term, or its correlate consciousness, a verbal stimulus and response, I can merely say that he is entirely mistaken. It should also

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[4] Whitehead, *op. cit.*, 24.

be said that I did not use the substantive but the verbal form, and that this is more in conformity with my general position. I am, however, rather frankly suspicious of the "speech reactions" of the behaviorists; they appear to me largely as empty formulae; they seem to function as a sort of Hegelian 'absolute' that obscures and swallows up all the concrete differences in our human experience in the interests of an abstract monism that mistakes itself for science. But there is another point, in this connection, that needs clearing up. I used the equivalent of the phrase "non-physical form of apprehension," and because he failed to interpret it in the light of the context in which it occurs Weiss fails to understand what the term denotes. Taken by itself, it may mean either that the process by which something is apprehended is non-physical, or that the apprehended object is non-physically present. Which of these two senses was intended should have been obvious, since the sentence which precedes the one from which Weiss quotes reads: "Or can he [the behaviorist] describe and react to it [a material object] in *its* physical absence?" I was instancing the case of 'image.' What I wanted to know was whether for behaviorism the object was always present in the sense in which it is said to be present, for example, in perception; and, if not, what account was to be given of this type of behavior, and, further, what status the object would, in such a case, be said to have. Instead of answering these questions, Weiss merely says: "It is only when imagery is forced into such categories as 'apprehension' that we get the apparently absurd condition of reacting to a non-physical condition," and that he reacts to Mr. X "in the same way" when he is absent as when he is present in the flesh; in other words, he misses the whole point in question. (3) Weiss' long statement of the meaning of the term 'environment' is not, in principle, different from mine; it is, in brief, only the application, intentional on my part, of the doctrine of the universe of discourse which is expounded in elementary books of logic.[5] I think it sound. But, first, he neglects the concrete question I raised, and secondly attributes to me a position I do not hold. The neglected question is whether we are to define 'environment' as consisting of everything outside the reacting organism, or whether it is some part of this. For either a given organism's reactions are the mechanical resultant of the sum-total of existing forces in the universe, or they are the resultant of some smaller number of these forces. In the latter case, the number of forces that become effective must depend in part upon the reacting organism and its active needs. I hold, then, that what is true of the particular organs of the body, namely, that they are functionally analytic with respect to the complex stimuli that play upon them, so that, for example, the eye picks out ether waves and the ear air waves, is true of the total organism, and that it, in like manner, picks out the particular aspects of its concrete situations which, when reacted to, satisfy the needs by which it is moved to act. From this it will be seen why I cannot hold the position Weiss attributes to me. He says that I wish to limit the term 'meaning' to a particular locus within the series of interactions which constitute the universe. I had said that I did not know where the limits, if any, are to be placed, and that our primary business was carefully to observe and accurately to report the facts as we find them in order to ascertain if the fact denoted by the term meaning was anywhere to be found in nature. I suggested that it was a characteristic of certain of our relations as behaving individuals to our environment, and, in a note, that I should probably place this characteristic lower in the scale of behavior than would Weiss. The danger, therefore, that Weiss points out does not really exist; rather, it lies in the opposite direction, for, so far from limiting it to a given locus, I should be more inclined to regard it, directly or indirectly, as a universal factor of anything that by careful definition could be called behavior.

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[5] Cf. for example my *Textbook of logic*, 1915, 50 ff.

(4) Weiss accuses me of "arbitrarily placing all social action into a teleological category." An examination of the passage which he quotes from my article will show clearly that I was not concerned with the question implied in his criticism. I definitely confined my discussion to the limits that Weiss himself gave to his biosocial category. And besides there would be no point in attempting to prove a universal when my purpose could be met equally well by proving a particular. What the passage in question aimed to show was that Weiss' removal of biosocial phenomena from the category of the biophysical necessitated the use of some other principle of explanation than the mechanical for these phenomena, and I asked what this principle could be if it were not the teleological. It is exceedingly gratifying that there is no difference between Weiss and myself as to the answer to be given to this question. I am, for example, in hearty agreement with him in the summary paragraph where he says that social life aims at experimentally establishing progressively an "optimum ratio" between what he calls "labor" or group interest, and "leisure" or the individual interest. Indeed, I had pointed out the same contrast a number of years ago under the terms "personal" and "individual," and argued that social and moral progress depended upon the establishment of a certain balance between these forces.[6] There is more than external similarity between our positions, for Weiss admits, what is implied in both, that the fact that he denotes by his "optimum ratio," and which I denote by "balance," is, to quote him, a "*final* tendency" (italics Weiss'), and, of course, final tendency is exactly what is meant by teleology, even if Weiss thinks it helps matters along to hide the real facts of the case behind the reservation that "the scientist is justified in calling it mechanical." (5) The criterion of teleology, Weiss says, is not clearly enough stated. Does he mean by this that what is stated leaves him in doubt as to the kind of thing I mean by 'teleology,' or does he mean that I have not developed a final definition and justification for teleology? If he means the latter, I can only say that it did not accord with the nature and purpose of the paper; if he means the former, it is obvious that what I said was explicit enough as a stimulus since Weiss has responded with a larger number of actions to this than to any other single topic in the paper.

In conclusion, I wish to say that it would gratify me exceedingly if the reader were to take what I have written as a measure of my appreciation of Weiss' work, and if he were to interpret the sharpness of the contrasts I have pointed out between Weiss' position and my own as being due, in part, to the necessity of focusing attention on the points at issue, and, in part, to the importance of defining the problems common to behaviorism and psychology, and of doing this in such a way that all ambiguity is removed as to the facts that the terms we use are intended to denote. When we have dispersed the mists that obscure our problems and data, it will be time enough to inquire if behaviorism is better able to give an explanation of them than psychology. No doubt, we all have our opinions about that. I have mine. But that is irrelevant.

Colorado Springs ARTHUR E. DAVIES

A FINAL STATEMENT

It seems to me that instead of approaching agreement the discussion of "Mechanism, meaning, and teleology in behavior" is leading to reiteration and greater divergence. While a continuation of it would doubtlessly prove of mutual benefit to Dr. Davies and myself, I do not believe it would prove equally interesting to the readers of this JOURNAL.

Ohio State University ALBERT P. WEISS

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[6] *The moral life*, 1909, 120-153.

THE VISUAL PERCEPTION OF MOVEMENT

Dimmick's reply (this JOURNAL, 37, 1926, 461-463) to our criticism (*ibid.*, 63-115) of his study of visual movement (*ibid.*, 31, 1920, 317-332) deals (*1*) with matters of technique and instruction, and (*2*) with questions of terminology and interpretation.

Upon (*1*) we offer the following comments.

(a) The slight glow surrounding each of our stimulus-objects exercised no influence upon apparent visual movement for the perceived motion was exactly opposed in direction to any irradiation or shrinkage of light.

(b) The use of figures of our sort gave greater control than Dimmick's through the reduction of the visual field to two small lines upon a lightless ground. Under such conditions there was no grey flash, which is—as we discovered—an occasional phenomenon appearing under a particular arrangement of stimulus but not essentially related to the perception of movement.

(c) Our instructions were concise and exact. We called for "particular and precise statements as to the order and nature of the appearance and disappearance of the phenomenal object or objects" and demanded that *O* confine his reports to "qualitative, temporal and spatial properties and relations." These instructions are certainly no more ambiguous than Dimmick's "describe in psychological terms," or "characterize the perception as fully as you can."

(d) Since our *O*s were given intensive training of 80 observations each before any report was accepted and since *five* of them (as against Dimmick's *one*) had completed their doctoral training, lack of stability of report can scarcely be invoked to account for the differences in outcome in the two studies.

(e) While we grant that our experimental conditions were different from those of Dimmick, the most casual survey of our original study will show that we did not base our criticism of him upon any *comparison* of the two experiments. We first stated clearly that under our conditions we found the grey flash wholly irrelevant to the perception of movement. We then proceeded to indicate certain obvious discrepancies in his study by way of which he apparently arrived at his questionable conclusion. As we showed, his results admit of a wholly different interpretation.

Now we advance to (*2*), the interpretation of results.

(a) We did not mean at any time that kinaesthetic movement was visual movement. We suggested that the phenomenal successive objects might be integrated in various ways so as to give the perception of apparent visual movement. Where *O* consistently reported pronounced visual motion without any visual filling our problem was to show the way in which such movement occurs. It was in seeking an explanation for such cases that we said "in the absence of all visual items other than the perception of successive positions, clear movement was apprehended through kinaesthesis. These experiential qualities served to integrate the whole perceptual incorporation; that is to say, to join the otherwise unrelated visual constituents. They furnished an experiential background for the purely visual items to functionate in perception." Dimmick, on the other hand, does not attempt to explain his negative cases. He simply ignores them. Where he contends that the values "are parallel" we showed at 30σ there were 103 more cases of optimal movement than of corresponding 'process-configuration;' while at 150σ there were 135 more cases of 'process-configuration' than movement.

(b) Referring to the *quantitative* treatment of our results we explicitly stated that "the most obvious fact in Table I is the prevalence of cases of full-movement; *i.e.* movement from the position of one line to that of the other." We added, furthermore, that "it must not be assumed that full-movement always meant that a line was apprehended to move, *as a line*, across the field; although this is the usual meaning of the term in previous studies. The *O*s instead reported at times that the experience meant that one line moved from one position to another; but that the actual movement *of a line* across the field was not seen. It was just *bare* movement over the field. It was an approximation to Wertheimer's pure *phi*." We sought here merely to say that we so considered full-movement as to include pure *phi*, where no object is present in the field of movement. Dimmick, as well as Wertheimer, allots a *separate* discussion to pure *phi*. We indicated that movement may be optimal without an object moving across the field *as an object*. The large number of such reports was included under the caption of full-movement. Now, what of Dimmick? *He did exactly the same thing as we*. He considered pure *phi* under optimal movement, for he says (italics ours), "finally, a number of cases of Wertheimer's pure *phi* were reported by our *O*s. *Frequently the line was not seen during movement; there was simply 'something moving' or just 'movement.' Sometimes neither of the lines was clearly perceived; just a bit of movement downward. The cases in which both lines were clearly seen with only movement between them are entered in Table I as optimal movement and are paralleled by total process-configuration.*" For statistical purposes, then, Dimmick included under optimal movement the same sort of experience as did Wertheimer and ourselves. He did this strangely enough when he replies that he "defined 'optimal movement' as the 'movement *of a line* all the way from one position to the other.' " This is indeed curious; to place for quantitative treatment under the head of optimal movement all the reports in which there was just movement and no line moving! Since this is the same type of movement which in our case we said was a clear approximation to W's pure *phi*, no difference can here exist. We can state specifically, then, that for statistical purposes we treated full-movement in exactly the same fashion as did Dimmick. He can scarcely hope to protect the integrity of his grey flash through any appeal to such imaginary differences.

(c) Concerning our criticism of the alleged correlation between grey flash and movement, Dimmick protests that his reports are "entirely straight-forward if they are read with a proper regard for our definitions." Consider only two of the various illustrations which we offered: (1) "An oblique line appeared, then a horizontal and the oblique moved down to the horizontal;" and (2) "An oblique line moved down to the horizontal but not smoothly; there was a flash of good movement at the beginning and end." The first is supposed to be *unimembral* and the second *bimembral*. The most casual reader will immediately see that we might, with equal justice, call both *optimal*, for a line moved all the way! We can scarcely admit these and similar cases as straight-forward and unambiguous.

(d) Again, it was not clear, how Dimmick proposed to account for differences in rate of movement as increasing and decreasing under a given exposition unless the *process* reports indicated a precise phenomenal basis for the reported difference in rate of *movement*. An inspection of the actual reports does not—as we hold—show in a single case any ground for this temporal difference. We say flatly that out of the 103 reports cited by Dimmick not one includes a description of the temporal aspect of the 'process' experience. Not once in the 103 reports do such words as "brief," "momentary," "instanteous," "short," or "rapid" occur in connection with the description of the grey. In fact, no descriptive temporal terms are given; yet the temporal aspect is as important for Dimmick as the qualitative. We ask again for the evidence to account, on the side of 'process,' for the difference in rate of movement.

(e) Finally, we must consider the most serious of all the statements in Dimmick's rejoinder. We remarked that "since Dimmick found the shift in process-configuration to be directly dependent upon the temporal interval and since any shift in the process pattern or the temporal interval meant, as he thought, a corresponding change in the type of perceived movement, it is strange that he does not cite *figures* or other testimony to the alleged change of optimal over into unimembral and bimembral movement as he passes temporally in either direction from the optimal interval. *At no place does he show the number of cases of unimembral, bimembral, and intramembral movement at various temporal intervals, i.e. the relation between these movements and the time variable."* To this he replies that "it would be strange had we not done so." We insist that it is strange that he did not do so, and stranger that he should now imply that he has. He says that "Tables I and III of our report (321 f.) furnish the desired data." But let the curious read try to verify from these tables Dimmick's statement that the integration is "directly determined by the temporal interval between the exposures of the two stimulus members" by attempting to determine the cases of unimembral, bimembral, and intramembral configurations and movements. Remembering that Dimmick used *five* intervals, let him try to work out the number of such cases say at 30 or 150σ. What will he find? From Table I he will learn that each *O* reported so many cases of each kind. But what significance has this for the *particular temporal interval?* Table III shows *only* the cases of optimal movement. From neither table is it possible to obtain data which *deny* our statement that Dimmick does not "cite figures or other testimony to the alleged change of optimal over into unimembral and bimembral movements as he passes temporally in either direction from the optimal interval. At no place does he show the number of cases of unimembral, bimembral, and intramembral movements at the various temporal intervals, *i.e. the relation between these movements and the time variable."* Dimmick admits that it would be strange if the data were not there. The tables to which he refers do not give them. The reader may make his own inference.

It is impossible to review the other adverse criticism in our experimental study which Dimmick attempts to meet. Those who are interested will find exposed in our original article various internal weaknesses in his data and conclusions. The reader will readily comprehend that it was but a final error on the part of Dimmick to regard our investigation as confirming "the results of his earlier study."

University of Illinois GLENN D. HIGGINSON

ON OVERLOOKING FAMILIAR OBJECTS

Professor Titchener concludes from his own experience in overlooking familiar objects[1] that Fuchs's theory is not applicable to most situations. Fuchs's notion was that the familiar object was obscured because its own average color scheme was absorbed or 'levelled out' through assimilation to the surrounding object-environment. Titchener has not only observed no color change in the overlooked object but gives positive evidence against the theory. His explanation is of two sorts: (1) the visual image does not fit; (2) the motor response is not started by the proper stimulus.

In a number of observations on the subject since this note appeared I have found that the explanation of my experiences tends to conform to a special case of an inadequate visual image. An object that is not in the looked-for place can not, of course, elicit an automatic motor response of the sort that Titchener describes. The problem resolves itself, then, into the following stages: (a) the letter opener, blotter, or eraser is not in its usual place on the desk or work-table, and the habitual movement that is initiated becomes inhibited; (b) the hunt begins for the object that fits the visual image. I agree with Titchener in the explanation of the first stage which is largely then a matter of motor 'set' and may lead to some sort of verbal expression couched perhaps in the vernacular, but in my experience the second stage has almost always been reduced to a further analysis of the unsuitable visual image.

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[1] This JOURNAL, 35, 1925, 304-5.

Now the chief feature about the mental image that makes it inadequate seems to be its size—it is too small. The situation is similar to the familiar experience of doing problems in simplest arithmetic or in spelling most familiar words on the blackboard before the class. The writing seems strange and hard to follow because of its abnormal bigness. When I stand off at a distance the situation is quite satisfactory and the sum is easily made or the words spelt. It is obvious that the sensory experience does not tally with the average size of imagery usually attached to the perception. A contrary experience occurs when passing through the mountains on a train, we see, perhaps, a deer on a distant mountainside, or when crossing the ocean or a large lake we pass another boat at a distance on the water. If, in either case, we try to photograph the object we are usually disappointed in the smallness of its size and are quite convinced that we saw the object many times larger. There is here, as it appears, an interpretative or a perceptual attitude which makes the image much larger than its physical reality. These cases may not be identical in all their elements but illustrate a sort of attentive working over of the material.

At various times, I have had altogether five experiences in which the overlooked object on the shelf or on the table was squarely in front of me, and in roving over the visual field I invariably looked through it. When it finally dawned upon me the humor of the situation lay in the fact that the object was unusually large as compared with the picture of it that I had in my mind. Of course, the fact that the observer looks through this object or all around it and thus disregards it is a well-known fact of attention illustrated by the attentive disregard of objects seen by the other eye when both eyes are open in microscopic work or in sighting a gun at a target. At least one psychologist has told me that when he has no use for an individual it is not true that he purposely disregards him on the street. It is much more accurate to say that he does not *see* him. Of course, we have further evidence of this sort of thing in pathological cases where individuals will not note objects at hand but will nevertheless successfully avoid them in making their passage through the room.

This problem could be very neatly worked out on an experimental plan and the hypothesis verified to see whether the size of the object is the chief error in the hunt. In talking the matter over with a colleague still another explanation was offered which does not, however, fit my own case. It was that the total configuration of the objects on the desk was attended to rather than the analysis of the pattern into any one particular object. In my case I have a distinct and very vivid image, perhaps not of the type of 'recurrent image' described recently by Dallenbach,[2] but nevertheless very clear and of a definite size, although the size is wrong.

University of Iowa CHRISTIAN A. RUCKMICK

WHEN AND HOW TO REPLY TO A QUESTIONARY

Many psychologists are being embarrassed by the increasing frequency with which they receive printed or mimeographed questionaries on scientific, historical, or administrative matters. The questionaries often come from unknown persons whose ability to make adequate use of accurate replies is not known. They are often of a form that renders accurate reply impossible. Frequently the reply would require considerable research of a minor character on the part of the person making the answer, and most psychologists feel that they would rather determine for themselves the distribution of their own time in investigation. Sometines the questionary even asks for information that is as readily available through library sources to the author of the questionary as it is to the recipient of the questionary.

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[2] This JOURNAL, 35, 1924, 155; cf. also E. B. Titchener, *A Beginner's Psychology* 1915, 75.

It is plain that many of these questionaries partake of the character of a 'nuisance.' For this reason some of us in four of the larger psychological laboratories have drawn up, under the title of this note, a set of rules which constitute in a way a code of action in this matter. Within a few weeks after the printing of these rules psychologists in six other large laboratories have asked for copies for use in replying to questionaries. Since this attitude toward the promiscuous questionary seems to be so general, it seems desirable to print the 'code' in the JOURNAL, so that the principles upon which some of us have agreed may be available to all, so that a reply to a questionary may sometimes be made by reference to the JOURNAL, and so that publicity may tend to correct the error at its source. The author of this note will, moreover, have reprints of it, which he will be glad to supply *gratis* to psychologists who desire copies as long as the supply lasts.[1]

1. If the questionary interests you or can be answered accurately in a few minutes, reply as a simple matter of courtesy, unless you see some postive reason why you should not reply.
2. Do not, in general, reply to a questionary issued by a person whom you know neither personally nor by scientific reputation.
3. If the unknown author of a questionary is introduced by someone whom you know, the case may be different; but you should make sure that the author's sponsor is actually guaranteeing the value and the precision of compilation.
4. Do not reply at all if you cannot reply accurately. Do not encourage slipshod work. If the questions are ambiguous, omit them. If the questionary calls for 'yes' or 'no' and the answer in not truthfully categorical, do not reply categorically, but give the complete answer or none at all. If there is insufficient space left for an accurate answer, omit the answer or use an extra sheet. If the wording of a question is ambiguous, omit it. If, however, there are many equivocal questions or many instances of insufficient space, it is a sign that the questionary has been made without sufficient knowledge of the situation under investigation and is, therefore, unlikely to yield reliable results; do not then reply to it at all.
5. Do not reply when the questionary asks for information that is available to the questioner through library sources, unless you know the answer without looking it up.
6. In general a request for a reply should be by personal letter to you, and not by mimeographed or printed form.
7. Do not think that the receipt of a stamped envelope for return of the questionary, or thanks expressed in advance, places an obligation upon you for reply.

Harvard University EDWIN G. BORING

URBAN'S TABLES EVEN AGAIN!

Dr. S. M. Newhall points out an error in Davies' correction of Rich's checking table (this JOURNAL, 34, 1923, 312). The value, when $p=0.27$ and $x=+2$, should be 4.9665 instead of 2.9665. The error is quite obvious, and in fairness to Davies it must be said that it is not of his making. The error is typographical, made by the printers during the 'make-ready' after the 'oked' page proofs had been returned to them.

K. M. D.

ERRATUM

In the recent article by P. R. Farnsworth, "Ending preferences in two musical situations," 'mi' should have been printed throughout for 'me.' Cf. pp. 237-240 *supra*.

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[1] See also resolutions passed on the matter, this JOURNAL, 37, 1926, 468; or *Science*, 63, 1926, 474; or *Psychol. Bull.*, 23, 1926, 292.

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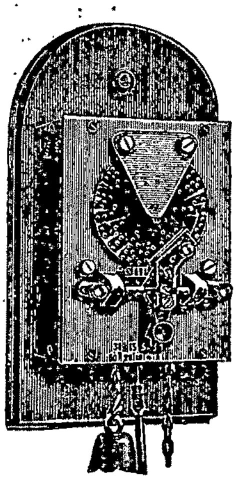

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